MEMOIRS

OF THE

NATIONAL ACADEMY OF SCIENCES

Volume XXI

WASHINGTON
GOVERNMENT PRINTING OFFICE
1926

ADDITIONAL COPIES
OF THIS PUBLICATION MAY BE PROCURED FROM
THE SUPERINTENDENT OF DOCUMENTS
GOVERNMENT PRINTING OFFICE
WASHINGTON, D. C.
AT
$2.25 PER COPY (CLOTH)

NATIONAL ACADEMY OF SCIENCES

Volume XXI

BIOGRAPHICAL MEMOIRS

J. A. Allen

NATIONAL ACADEMY OF SCIENCES

Volume XXI
FIRST MEMOIR

BIOGRAPHICAL MEMOIR JOEL ASAPH ALLEN

1838-1921

BY

FRANK M. CHAPMAN

Presented to the Academy at the Annual Meeting, 1922

CONTENTS

JOEL ASAPH ALLEN[1]

By Frank M. Chapman

ANCESTRY AND BOYHOOD

There is nothing in the immediate ancestry, the early environment, or associations of Joel Asaph Allen to account for his obviously instinctive characteristics as a student of nature. One can only say that he was born a naturalist and that the definite, pronounced interest which at an early age he evinced in natural phenomena and in plant and animal life developed spontaneously.

Doctor Allen was born at Springfield, Mass., July 19, 1838, and died at Cornwall-on-Hudson, N. Y., on August 29, 1921. He came of good New England stock. On his father's side he was a descendant in the eighth generation of Samuel Allen, who settled at Windsor, Conn., in 1640, and who came to this country from England, it is believed, in 1630 with the Dorchester Company in the ship *Mary and John*.

On the maternal side, Doctor Allen was descended from John Trumbull, great-grandfather of Gov. Jonathan Trumbull (said to have been the original "Brother Jonathan" and familiar friend of Washington), who was born in Newcastle-on-Tyne, England, and settled in Roxbury, Mass., in 1639.

Doctor Allen's immediate progenitors were farmers. His father, however, was a carpenter in his earlier days, but later bought a farm on which he passed the greater part of his life. A man of excellent judgment and sterling integrity whose advice was often sought by his neighbors, he had, however, little appreciation of his son's desire to study nature, and evidently expected him to succeed him in the care of the farm. Doctor Allen's mother, on the other hand, had much sympathy with his yearnings for a knowledge of flora and fauna, and often used her influence to secure for him opportunities to study. Possibly tastes latent in her may have found expression in her son. They were not, however, possessed by Doctor Allen's two brothers, one of whom became a molder, while the other remained on the parental farm.

Doctor Allen's early training was rigidly puritanical. Both of his parents were members of the Congregational Church and strict in their religious observances. His home was half a mile from that of the nearest neighbor; but in thus being removed from external influences, he evinced while still a toddling youngster so marked an interest in wild flowers that he was dubbed "Doctor Sykes," in allusion to an herb doctor of local reputation.

With no training or contact with the outside world, other than that to be gained by attendance during the winter session at the conventional little red schoolhouse, distant a mile from his home, the young farmer developed a desire to know more of the soil and rocks, the animal and plant life, the ever-changing phenomena of sky and air which formed his environment.

At the age of 13, after much pleading, his father presented him with a gun. At first used for sport, it soon became a means of acquiring specimens. No books were available; there was no one to turn to for advice; and without instruction of any kind the tastes, not merely of the "nature lover," common in varying degrees to most men, but the deeper, rarer instincts of the student naturalist, were manifested. The birds shot were measured, weighed, described, and named. Attempts were even made to make colored drawings of them. A new world was opening to the boy, and so far as he knew he was the only naturalist in it.

His joy may be imagined, when, a little later, he made the acquaintance of one Bradford Horsford, a teacher of drawing, who was also an amateur taxidermist and ornithologist. From

---

[1] The biographical and bibliographical portions of this memoir are based on "Autobiographical Notes and a Bibliography of the Scientific Publications," prepared by Doctor Allen, and published by the American Museum of Natural History in 1916. The more strictly historical portion is taken from the memorial address on Doctor Allen presented by the writer before the American Ornithologists' Union, at its Thirty-ninth Congress, held in Philadelphia, November 9, 1921, and subsequently published in "The Auk" for January, 1922.

him young Allen borrowed, and afterward bought, a copy of the Brewer edition of Wilson's American Ornithology. Subsequently Nuttall's and Audubon's works on North American birds were discovered in the Springfield Public Library, and the boy naturalist was launched upon the career which, with never-ceasing pleasure to himself and increasing profit to science, he followed for the succeeding 70 years.

EDUCATION

When one considers his comparative isolation and the general lack of interest in natural phenomena of the period, Doctor Allen was singularly fortunate in finding men who could give him the assistance he so eagerly sought.

Shortly after meeting Horsford, a teacher took charge of the district school who possessed a broader education than anyone with whom young Allen had come in contact. A nature lover himself, he could appreciate his pupil's aspirations, and he not only assisted him in his studies, but gave him a copy of Blythe's Cuvier's Animal Kingdom. This work greatly enlarged the boy's horizon and showed his potential broadness as a naturalist. His interest in nature did not, as often happens, begin and end with birds, but plants, mammals, reptiles, fishes, insects—in short, the living world—equally appealed to him, and for years he kept a detailed record of meteorological phenomena. His first publication, indeed, was a summary from his journal of weather conditions, which appeared in the New England Farmer for 1858.

Prof. Oliver Marcy followed the donor of "Cuvier" as the boy naturalist's friend and teacher. Later he became dean of the faculty of Northwestern University, but at that time he was the teacher of natural sciences at Wilbraham Academy, which Allen attended during the winter from 1858 to 1862. This was a productive period in his development. Under the sympathetic guidance of Professor Marcy he selected his own studies, including physiology, astronomy, chemistry, Latin, French, and German. His summers were still spent on the farm, but with Humboldt's Cosmos, Lyell's Principles of Geology, and Dana's Mineralogy for companions, it is clear that his horizon was not restricted to the hayfields. To demonstrate, however, his value as a farm hand, the far from strong boy unduly exerted himself and this, with demands made by a desire to gratify his passion for collection and study, told heavily on his health. To these long periods of overwork, Doctor Allen attributed much of the semi-invalidism from which he suffered in after years.

Much of his spare time was now devoted to the study of botany, with the aid of Gray's Manual, and to the making of a collection of plants. These, however, formed only a part of the future curator's "museum." During the years 1859–1861 he collected and mounted some 300 birds, representing nearly 100 species, numerous mammals, reptiles, fishes, amphibians, some mollusks, and several hundred insects. Local minerals and rocks also found a place on the shelves of the room which did duty for a museum. These specimens indicated not a boyish desire to acquire; they were named and catalogued as part of a naturalist's equipment.

"The whole," Doctor Allen writes, "was amateurish in the extreme, and represented merely a superficial acquaintance with a wide range of subjects, but enough to add immensely to the pleasure of living, giving, as it did, the sense of being in touch with the plant and animal life and the geological features of my immediate environment."

Few local collections have done better service than the one which inaugurated Doctor Allen's career in museum work. It not only directly increased its owner's knowledge, but by its sale to Wilbraham Academy he was enabled to pay his tuition in that institution, where future students could profit by his industry.

The balance of the fund received for his beloved specimens enabled Doctor Allen to take the most important step in his life as a naturalist. While at Wilbraham Academy he found a congenial spirit in a fellow pupil, William Harmon Niles, a nephew of Professor Marcy, and who subsequently became professor of geology and physical geography in the Massachusetts Institute of Technology.

Niles planned to enter the Lawrence Scientific School as a pupil of Agassiz, and Allen decided to join him. The necessary preliminaries having been arranged, they arrived in Cam-

bridge early in February, 1862. They were cordially greeted by Agassiz, and plans were at once made for laboratory work and for courses of lectures in the Lawrence Scientific School under Agassiz, Jeffries Wyman, Joseph Lovering, Josiah P. Cook, and Asa Gray. Thus, as by the workings of a special Providence, the young naturalist's eager inquiries for directions on the road he was destined to follow were answered by the village taxidermist, the public schoolmaster, and the academy professor, each of whom assisted him on his way to the greatest teacher of his time. How fortunate it was for the future of science in America that Agassiz should have attracted to him students of the caliber of Allen, Alpheus Hyatt, Edward S. Morse, A. S. Packard, and A. E. Verrill, and others whose subsequent labors have exerted an incalculable influence on the development of zoological research in this country!

Allen expected to specialize in ornithology and was somewhat disappointed to be given, with Niles, a collection of corals and told to find their methods of growth and laws of development. Neither books nor instructions were given them, and equipped only with a hand lens they were instructed to use their powers of observation and report the result. Weeks were devoted to this task, but eventually the problems were solved and the young students given other work. With interruptions occasioned by ill health, when he devoted himself to fieldwork, Doctor Allen continued to work at Cambridge until the spring of 1865.

FIELD EXPLORATIONS

In March, 1865, Allen was invited by Agassiz to accompany him on an expedition to Brazil. The party contained seven official members, including Charles Frederick Hartt, and six volunteers, among whom was William James, later to become eminent as a psychologist. They sailed from New York on March 26 and arrived at Rio Janeiro April 22.

After collecting in the vicinity of that city for some weeks Doctor Allen was detailed to join a smaller party which left June 9 for the northern Provinces of Brazil. A difficult journey of somewhat over six months brought him to Bahia. Although so far from well during this period that he was obliged to abandon the plan to reach the coast at Ceara, Doctor Allen's collections included several cases of birds, mammals, mollusks, and zoological specimens, besides six or eight barrels of fishes, reptiles, and other vertebrates in alcohol; and his notebooks contained many pages of detailed observations on the country through which he had passed, its flora and fauna.

On December 15 Doctor Allen sailed from Bahia on a 300-ton brigantine, and, after a trying voyage, during which they were blown from Cape Hatteras back to St. Thomas, they dropped anchor off Woods Hole, Mass., 90 days out from Bahia.

Chronic indigestion now forced Doctor Allen to abandon museum work and return to the farm; but he had experienced the joy of exploration and, as soon as his health permitted, he took the field again, collecting in June, 1867, on Sodus Bay, Lake Ontario, and during the summer in Illinois, Indiana, and southern Michigan. At the end of this time he was physically so greatly improved that in October, 1867, he returned to the Museum of Comparative Zoology to act as curator of birds and mammals in that institution.

After a year in the study, the winter of 1868–69 was devoted to zoological exploration on the headwaters of the St. John's River, then a primeval part of Florida.

The results of the Florida expedition having been reported, Doctor Allen started in April, 1871, on a nine months' collecting trip to the Great Plains and Rocky Mountains in the interests of the Cambridge Museum. General collections were made at intervals from the Missouri River to the Great Salt Lake, the selection of locality being largely dependent upon the movements of hostile Indians. At Fort Hays, Kans., the arrival of a military escort being delayed, Doctor Allen and his two assistants went buffalo hunting, accompanied by only a single hunter, securing and preparing in 8 days, of which 36 hours were occupied in traveling, 14 complete skeletons and 5 young calves. This collection was supplemented the following January by the skins of 8 buffalo in winter pelage.

July and part of August were passed in Colorado, where *Leucosticte australis* was discovered on the summit of Mount Lincoln, and after 10 days at Cheyenne Doctor Allen went to Ogden,

Utah, which became his base for the ensuing 7 weeks. In October he worked at Green River and Fort Fred Steele, and from October 20 to December 18 at Percy. Here he secured the assistance of two native hunters, and the collections, chiefly of big game, shipped from this point nearly filled a freight car. December 19 he started eastward and, after a short stop in Kansas to secure buffalo, reached Cambridge on January 22, 1872. The collection made on this expedition included 200 skins, 60 skeletons, and 240 additional skulls of mammals (mostly large species), 1,500 birds' skins, over 100 birds in alcohol, a large number of birds' nests and eggs, recent and fossil fishes, mollusks, insects, and crustaceans.

The following year Doctor Allen, representing both the Cambridge Museum and the Smithsonian Institution, again went to our western frontier, on this occasion as chief of the scientific staff attached to the survey of the Northern Pacific Railroad. Railhead on this road was then at Fargo, N. Dak., beyond which construction trains ran as far as Bismarck.

The work of the expedition lay in the country between Bismarck and a point on the Mussellshell River, about 50 miles northwest of Pompey's Pillar on the Yellowstone, a distance of about 550 miles. The journey occupied some three months from June 20.

The region was infested by actively hostile Indians who had so interfered with the survey for the railroad route that an escort of 1,400 troops under General Custer accompanied the expedition. It was only three years later that this officer and his entire command were killed some 60 miles south of the most western point reached by Doctor Allen.

After passing the mouth of the Powder River the expedition was in daily contact with Indians and twice was attacked in force; orders were given forbidding the naturalists to use firearms or to leave the line of march, and, Doctor Allen writes, "The opportunities for natural-history collecting and field research on this expedition were far from ideal," but some specimens and much valuable data were secured which later formed the basis of a report of some 60 pages. With the exception of a visit to Colorado with William Brewster, in 1882, made chiefly to regain his greatly impaired health, Doctor Allen did not again enter the field. His collecting days, therefore, were ended before those of most of his colleagues were well under way, and few who knew him only in the study realized the extent of his travels, the dangers on sea and land to which he had been exposed, and the amount of material he had secured. The present-day naturalist, who travels in palatial steamers or follows well-worn trails, has but faint conception of the discomforts of a 90-day voyage in a small sailing vessel and has perhaps never experienced the risk of being himself collected.

From 1876 to 1882 Doctor Allen gave his time wholly to research, producing his monographs on the American Bison, Living and Extinct, and North American Pinnipeds, the latter a volume of 800 pages. The intensity with which he applied himself to these and other tasks during this period overtaxed his always limited reserve powers and for long periods he was able to do little or no work.

ASSOCIATION WITH THE MUSEUM OF COMPARATIVE ZOOLOGY

Doctor Allen's association with the Museum of Comparative Zoology began when as a student under Agassiz, he acted as an assistant in routine work, and received a monthly allowance sufficient for his living expenses, together with a furnished room in the museum dormitory.

He was not, however, made a member of the museum's scientific staff until 1871, when he became "assistant in ornithology." He continued to act as curator of birds and mammals until 1885, when he resigned to accept a similar position in the American Museum of Natural History.

Practically all Doctor Allen's field work after boyhood was done for or under the auspices of the Museum of Comparative Zoology, and he thus laid the foundation for the valuable collections of birds and mammals contained in that institution. The care of this material fortunately did not prevent Doctor Allen from making the philosophical researches which soon distinguished him, and some of his most important contributions to science were produced while he was associated with the Museum of Comparative Zoology. Chiefly through his influence and that of William Brewster, Cambridge became the center of ornithological activity

in this country. In 1876 this interest in the study of birds found expression in the formation of the Nuttall Ornithological Club, which, after giving birth to the American Ornithologists' Union, in 1883, has continued its career as a prosperous local organization.

For eight years Doctor Allen served as corresponding secretary of the Nuttall Club, and as editor of its Bulletin, the latter position leading naturally to his editorship of The Auk, which, with the founding of the American Ornithologists' Union, logically succeeded the Bulletin.

While in Cambridge, in addition to his curatorial duties, Doctor Allen served as lecturer on ornithology at Harvard College (1871–1873), as curator of reptiles at the Boston Society of Natural History (1868–1871), and as curator of birds and mammals in the same institution (1870–1880).

ASSOCIATION WITH THE AMERICAN MUSEUM OF NATURAL HISTORY

When the trustees of the American Museum, under the presidency of Morris K. Jesup, decided to make research as well as exhibition the function of that institution, their choice fell upon Doctor Allen as the head of the department of birds and mammals, a post which Doctor Allen entered on May 1, 1885.

This was the beginning of a new period in his life as well as that of the museum. Although the museum's exhibition halls had a fair representation of the leading types of birds and mammals, there was no study collection of the latter, and only about 3,000 study specimens of the former. The 50,000 skins and skulls of mammals at present in the museum were all, therefore, acquired during the period of Doctor Allen's curatorship, and to him in large measure is due the size and importance of the study collection of birds. Two years after Doctor Allen came to the museum, the Lawrence collection of 12,000 specimens was purchased, and this was followed by the Herbert Smith collection of 4,000 birds from southwestern Brazil, the Scott collection from Arizona, and the collections of Arizona birds presented by Dr. E. A. Mearns, and of humming birds by D. G. Elliot. At this time also the invaluable ornithological library of Doctor Elliot was acquired. The first three years of his connection with the museum Doctor Allen worked alone, but on March 1, 1888, the writer was appointed his assistant, and to-day the combined staffs of the now separate departments of birds and mammals number 17.

Relieved now of the actual care of the growing collections, Doctor Allen devoted himself to their study, and the publications of the museum during the succeeding third of a century bear testimony to his industry and productiveness. During this period he published 37 papers on birds and 150 on mammals, based wholly or largely on museum material. To his duties as curator were soon added those of editor, a post which his natural qualifications and experience especially fitted him to occupy. For 32 years all the zoological publications of the museum, including 37 volumes of the Bulletin and 22 of the Memoirs, passed through his hands, and a large part of his time was consumed by the preparation of copy for the press and the reading of proof.

Doctor Allen was eagerly welcomed to New York by the resident naturalists of the city. He was at once placed on the council of the Academy of Sciences, and later was made president of the Linnaean Society, but he soon found that the duties of each day demanded all his strength and he was able to take only a small part in the scientific activities of the city. He, however, was one of the organizers of the original Audubon Society, and to the end was an active director of this society and its virtual successor in New York, the National Association of Audubon Societies.

ASSOCIATION WITH THE AMERICAN ORNITHOLOGISTS' UNION

But by far the greater part of the time Doctor Allen could spare from his curatorial labors was given to the American Ornithologists' Union, in the welfare of which he was as much concerned as a father in the well-being of his first born. Indeed, to Doctor Allen might well be applied the title "Father of the American Ornithologists' Union." He played a leading part in its organization, served as its president during the first eight years of its existence, and was a member of its council until the day of his death. He edited 3 volumes of the Union's Check-List of North American Birds and for 28 years was editor of its official organ, The Auk, during which period he contributed 643 papers, reviews, and obituary notices to that publication.

Only one in daily contact with Doctor Allen can realize the extent of the demands upon his time and strength made by his duties for the Union and the loving attention he gave to its affairs. It occupied a place in his affections second only to that held by members of his family, and he never spared himself in advancing its aims.

Doctor Allen was chiefly responsible for the formulation of the Union's Code of Nomenclature, a subject in which he took a deep interest and on which he was an authority. For years he served as chairman of the Union's committee on classification and nomenclature, and for the last 10 years of his life he was a member of the International Commission on Zoological Nomenclature.

PERSONAL CHARACTERISTICS

Doctor Allen's distinguishing characteristics as a man were modesty, sincerity, unselfishness, gentleness, consideration for others, and a purity of mind and purpose which made it difficult for him to believe that anyone was not actuated by the same direct, guileless motives which ever animated him. I do not recall ever hearing him speak ill of another, but he was unsparing in his condemnation of careless work, and particularly of generalizations based on insufficient data. But so impersonal was his attitude, so impossible was it for him to cherish resentment, that while for an author he would show only helpful consideration, for his work honesty would compel him to be merciless. I have seen him treat with fatherly kindness a man whose theories he had subjected to fatally destructive criticism.

As a student Doctor Allen was inspired by love of truth for truth's sake and by an intense absorbing interest in his work. "All I aspired to," he wrote (Autobiographical Notes, p. 42), "was opportunity for scientific research, believing that diligence, singleness of purpose, and honest work would bring its own reward. I was content to follow my own lines of dominating interest to such limit as the circumstances of earning a living would permit. I have never had any desire for money as such, nor any interest whatever in financial projects, nor any longing for honors beyond those my colleagues in science saw fit to impose." His powers of application and concentration were phenomenal; his enthusiasm for research so unlimited that he constantly overtaxed his physical resources and the end of the day often found him on the verge of complete exhaustion. But so vitalizing was his love for his profession that, in spite of a frail physique and the fact that he never rested from his labors when it was a possible thing to pursue them, he was actively engaged in research to within a few weeks of his death.

But he was never too absorbed in his work to be interested in that of others; an appeal to him for advice or assistance received his whole-hearted attention and he made your problem his. The writer owes him a debt which accumulated during 34 years of almost daily association. Coming to the museum in March, 1888, as an inexperienced assistant, he found in Doctor Allen not only a friend but a teacher to whom he might turn for instruction in even the most trivial matters with the assurance that he would meet with a sympathetic response. Doctor Allen's counsel was always based on a logical consideration of the facts at issue; for, as far as was humanly possible, he eliminated the personal equation in reaching conclusions. The inestimable privilege of securing Doctor Allen's advice was sought, therefore, not only by members of his staff but by workers in other departments of the museum and in other institutions.

HOME LIFE

In 1879, after five years of wedded life, Doctor Allen's first wife, Mary Manning Cleveland, of Cambridge, died, leaving him his only child, Cleveland Allen, now in business in New York City.

Seven years later, and a year after coming to the American Museum, Doctor Allen married Susan Augusta Taft, of Cornwall-on-Hudson, who survived him. "I owe to her deep love and sympathy," Doctor Allen writes, "to her supreme optimism and constant watchfulness over my health, and to her inspiration, the greater part of the little I may have achieved in these last thirty years, and doubtless many years of activity beyond those I otherwise would have attained."

CONTRIBUTIONS TO SCIENCE

Doctor Allen's first publication of major importance appeared in 1871, when he was 32 years of age. It was issued, unfortunately, under the superheading "On the Mammals and Winter Birds of East Florida" (an excellent faunal paper based chiefly on a winter's work in that State), but that portion of the paper which at once brought Doctor Allen to the attention of philosophic naturalists is contained under the subheading "With an examination of certain assumed specific characters in birds and a sketch of the bird-faunæ of eastern North America."

The subject of individual and geographic or climatic variation in the size and colors of birds was here given more serious consideration, based on detailed studies, than it had previously received.

Trinomials had not then been adopted as tools of the taxonomist, and Doctor Allen, conservative by nature, protested against the recognition as species of intergrading forms, a procedure which is now unquestioned. He argued that "whenever two forms which have both received names are found to intergrade, the more recent name shall become a synonym of the other. Some, however, still urge that every recognizable form, closely allied to others, and even intergrading, should be recognized by a binominal epithet, and that whether we call them species, or varieties, or races, or simply *forms*, that such names are none the less convenient expressions of facts."

Doctor Allen did not indorse this view, and although subspecific "splitting" was then in its infancy, he added with a prophetic foresight which indicated the thought he had given to the subject: ". . . Only experts can distinguish the forms, and frequently they only by actual comparison of specimens. . . . The names alone give us no clue to their real character, and are hence in a great measure meaningless when separated from the most explicit diagnoses, and whose affinities can frequently only be settled by the arbitrary criterion of locality."

It is a tribute to Doctor Allen's open-mindedness to follow his gradually changing point of view as with increasing experience, gained through the study of constantly growing collections and a characteristically unprejudiced estimate of the labors of his colleagues, he finally became convinced of the importance of recognizing the slightest constant geographic variation by name.

In 1883 and 1884 he published several articles or reviews advocating the use of trinomials as a convenient means of recognizing geographic forms in our systems of nomenclature. "Instead of doing violence to the 'Stricklandian Code,' " he wrote, "the trinomial system is a device, as we have stated on other occasions, to meet simply and completely a condition of things unknown and unexpected when that, in most respects, admirable system of nomenclatural rules was conceived, and is in accordance with the spirit if not the letter of that 'Code.' It is in no sense a lapse toward polynomialism." (Auk I, 1884, p. 103.)

Six years later, under the title "To what extent is it profitable to recognize geographical forms among North American birds?" he wrote: "Conscious of my own changed tendencies, it has seemed to me well to raise the above question for brief consideration, since it can do no harm to survey the field calmly and take note of the present drift in respect to a very important subject.

"Recent investigations have taken me over fields I worked, with some care, ten to fifteen years ago. In the meantime material has greatly increased; series of specimens have been obtained from localities then unknown; thus I find myself looking at things in a new light, but from, I trust, a more advanced position. My former tendencies, in common with those of others at that time, were in the direction of reducing doubtful forms to synonyms and closely related species to geographical forms. Now, with much additional experience, some increase of knowledge in respect to particular points at issue, and much more abundant material, some of my former conclusions seem open to revision, as I now realize that the resources then at command were far less adequate for the settlement of questions at issue than I then supposed them to be."

He still, however, urged caution in the use of trinomials "in order to guard against drawing too fine distinctions"; and added, "very little is to be gained by naming races distinguishable only by experts . . ." (l. c., p. 7). But a dozen years later we find him wholly committed to the recognition by name of geographic variations which are appreciable only to the experienced

systematist. Replying to an article in Science by Mr. Hubert Lyman Clark on the advisability of naming these slightly differentiated forms, he said: "We submit that the 'layman' who is naturally so troubled and confused by the modern ways of finding out how and to what extent animals are modified by their environment, is not the proper arbiter to determine the value and bearing of expert knowledge. If in other fields of scientific research it is not demanded that the investigator stop his work at the point where his results are within the comprehension of the lay mind, why should the student of birds and mammals be expected to refrain from extending his researches beyond the point of convenience for the layman?"

As an indication of this gradually changing estimate of the nature and importance of geographic variations, as well as of the increasingly great amount of material that passed through his hands, it is interesting to observe that while from the beginning of his systematic studies in 1870 to 1890 Doctor Allen described only 16 species and subspecies of mammals, during the 20 years following 1890 he described 415.

After treating at length of individual and climatic variations and illustrating his remarks with many specific instances [2] which up to that time had escaped the attention of systematic ornithologists, Doctor Allen presented an exposition of the "Causes of climatic variation," to which, after 50 years, we can add but little. After calling attention to the fact that increase in color is coincident with increase in humidity, and that the darker representatives of a species occur "where the annual rainfall is greatest, and the palest where it is least," he added: "This coincidence is clearly illustrated in the birds of the United States, when the darkest representatives of a species, as a general rule (indeed without exception so far as known to me), come from regions of maximum annual rainfall, and the palest from those of minimum annual rainfall."

Part V of this classic work, "On the geographical distribution of the birds of eastern North America, with special reference to the number and circumscription of the ornithological faunæ," has in reality a wider scope than the title implies, since it includes also a general discussion of the laws governing the distribution of life, an outline of the primary natural history divisions of the globe, and of the provinces of the North American temperate region.

When we consider the comparatively limited, and often inaccurate, data which were available 50 years ago, the soundness of the conclusions reached by Doctor Allen in his study of these problems is a tribute to the breadth of his vision, the thoroughness of his methods, and the excellence of his judgment.

"I am aware," he writes, "of the diversity of opinions still prevalent among naturalists in regard to the influence climate exerts on determining the geographical distribution of species and that many writers on this subject attribute to it only a slight importance, or altogether ignore it." That he, himself, did not share this view is evident when he adds: "That temperature is a powerful limiting influence affecting the range of species, especially in respect to their northward and southward extension, is so easily demonstrable that I am surprised to see it still questioned. I have myself subjected this principle to a rigid examination in studying the distribution of the animals and plants of eastern North America, and have been surprised at the exact coincidence I have almost constantly met with between their northern and southern limits of distribution and isothermal lines, they following them in all their numerous undulations, sweeping northward in the valleys and southward along the sides of mountain ranges. The occurrence on isolated alpine summits of species existing at a lower level only far to the northward, is of itself suggestive of the powerful influence temperature has on the distribution of animals and plants. In the Northern Hemisphere a northern fauna and flora everywhere extends along the mountains hundreds of miles to the southward of their respective limits in the adjoining plains and valleys. Various other causes have, of course, a greater or less influence in determining the range of species, but none other, on the land areas, humidity perhaps alone excepted, is nearly so potent. The want of conformity of isothermal lines with parallels of latitude has doubtless led to confusion in regard to this subject, since vain attempts have often been made to circumscribe the botanical and zoological zones by the latter. . . . The iso-

[2] *E. g.*, birds which subsequently were described as *Pipilo erythrophthalmus alleni* Coues, *Ortyx virginianus floridanus* Coues, *Buteo lineatus alleni* Ridgw., *Sturnella magna argutula* Bangs, etc.

therms of the continents are widely deflected by the irregularities of the surface of the land, running nearly straight and parallel across level areas; but in mountainous districts they bend abruptly northward or southward, following along the sides of mountains instead of crossing them. In the same manner are species, and faunæ and floræ, limited—a coincidence clearly indicative of the strong influence climates exert in determining their geographical limits."

In defining the boundaries of faunal areas and in the nomenclature adopted for them Doctor Allen exhibited characteristic common sense and independence of thought: "The boundaries of realms and provinces," he wrote, "have often been arbitrarily fixed, inasmuch as they have been frequently limited and named in conformity to the continental areas, regardless of the fundamental law of the distribution of life in circumpolar zones." He protested against "the arbitrary partitioning of an almost homogeneous Arctic Realm between two implied totally distinct life regions, and also a similar division of the two slightly differentiated regions of the North Temperate Realm. For nearly all the species, and hence of course the genera and families, of the Arctic Realm, and a considerable percentage of the species, a large proportion of the genera, and nearly all the families of the Temperate Realm, occur in the northern parts of both the so-called 'Neogean' and 'Palaeogean Creations.'" These terms, he added in a footnote, together with "Palaearctic," "Nearctic," etc., "like those of 'Old World' and 'New World,' have been given with reference solely to the length of time the different land areas of the earth's surface have been known to the dominant race of mankind, and hence regardless of the zoological history of these different land areas. Modern science has taught us that the latest discovered continent (Australia) is peopled with the most ancient types of animals and plants now in existence, and that it is, zoologically considered, the ancient continent. Also that North and South America are behind Europe, Asia, and Africa in their zoological and geological development, while they are far in advance of Australia. To apply the term 'ancient' to what is really the most recent and 'modern' to what is mediæval, is evidently too great a misuse of language to be allowable in scientific nomenclature. The sciences of geographical zoology and geographical botany concern not merely the geographical distribution of the animals and plants now living, but also those of the past. If such descriptive terms as the above are to be employed, it is evidently important that they should be used in their legitimate sense. In the present paper it has hence been considered advisable to altogether discard these terms, since to use them properly would necessitate their adoption in a manner directly opposite to their original and generally accepted application."

"Neotropical," as applied to southern Mexico, Central and South America, is also shown to be misleading, since it includes also the South American Temperate Realm.

The terms employed by Doctor Allen for the eight major faunal areas, or "realms," call for no explanation and are, therefore, far more preferable than those which disguise an old fact under a new name. They are:

I, an Arctic Realm; II, a North Temperate Realm; III, an American Tropical Realm; IV, an Indo-African Tropical Realm; V, a South American Temperate Realm; VI, an African Temperate Realm; VII, an Antarctic Realm; VIII, an Australian Realm.

Doctor Allen followed Baird in recognizing two provinces, an eastern and a western, within the limits of the American portion of the North Temperate Realm, and he then treated of the faunas, or minor divisions of his eastern province, in so adequate a manner that the lines he laid down have not been essentially changed, and this treatise remains to-day an authoritative exposition on the causes governing the distribution of life in eastern North America.

This publication won for its author the Humboldt scholarship of the Lawrence Scientific School and at once placed him in the first rank of American naturalists. It was characterized by Coues (Bibliography of North American Ornithology, p. 686) as "a highly important philosophic treatise upon the general subject, which is discussed at length with force and logical consistency; the author's broad views upon this subject had at once a marked influence upon ornithological thought."

The subject of geographic variation continued actively to hold Doctor Allen's attention and, five years after the appearance of the Museum of Comparative Zoology essay, he published a paper on "Geographical variation among North American mammals, especially in respect to size," in which the following laws in regard to fissiped carnivora were enunciated:

(1) *The maximum physical development of the individual is attained where the conditions of environment are most favorable to the life of the species.* Species being primarily limited in their distribution by climatic conditions, their representatives living at or near either of their respective latitudinal boundaries are more or less unfavorably affected by the influences that finally limit the range of the species. . . .

(2) *The largest species of a group (genus, subfamily, or family, as the case may be) are found where the group in which they severally belong reaches its highest development, or where it has what may be termed its center of distribution.* In other words, species of a given group attain their maximum size where the conditions of existence for the group in question are the most favorable, just as the largest representatives of a species are found where the conditions are most favorable for the existence of the species.

(3) *The most 'typical' or most generalized representatives of a group are found also near its center of distribution, outlying forms being generally more or less 'aberrant' or specialized.* Thus the Cervidæ, though nearly cosmopolitan in their distribution, attain their greatest development, both as respects the size and the number of the species, in the temperate portions of the northern hemisphere. The tropical species of this group are the smallest of its representatives. Those of the temperate and cold temperate regions are the largest, where, too, the species are the most numerous. . . . The possession of large, branching, deciduous antlers forms one of the marked features of the family. These appendages attain their greatest development in the northern species, the tropical forms having them reduced almost to mere spikes, which in some species never pass beyond a rudimentary state.

A year later he contributed to the Radical Review (May, 1877, pp. 108–140) an article on "The influence of physical conditions in the genesis of species," which is fundamentally so sound and logical that 29 years later the Smithsonian Institution requested permission to republish it (Ann. Rep. for 1905, pp. 375–402).

Doctor Allen here contended that the direct modifying influences of environment are more potent factors in evolution than natural selection, taken in the narrow sense of the "survival of the fittest." Climate is shown to be the most active agent in promoting variations in size and in color, but habits and food and the geological character of the country are considered to play their part. Of the action of climatic influences he wrote:

That varieties may and do arise by the action of climatic influences, and pass on to become species, and that species become, in like manner, differentiated into genera, is abundantly indicated by the facts of geographical distribution and the obvious relation of local forms to the conditions of environment. The present more or less unstable condition of the circumstances surrounding organic beings, together with the known mutations of climate our planet has undergone in past geological ages, points clearly to the agency of physical conditions as one of the chief factors in the evolution of new forms of life. So long as the environing conditions remain stable, just so long will permanency of character be maintained; but let changes occur, however gradual or minute, and differentiation begins. If too sudden or too great, extinction of many forms may result, giving rise to breaks in the chain of genetically connected organisms.

Due allowance, however, he states must be made for relative plasticity or susceptibility to the influences of environment shown by closely allied species.

He also considers the possibility of species arising through what has since become known as mutation; writing:

But it is supposed, again, that new forms are not always thus gradually evolved from minute beginnings, but sometimes—perhaps not infrequently—arise by a *saltus;* that individuals may be born widely different from their parents, differing so widely and persistently as not to be so readily absorbed by the parental stock. In proof of this, instances are cited of new species apparently appearing suddenly, and of varieties thus originating under artificial conditions resulting from domestication. Granting that new forms may thus arise, although as yet few facts have been adduced in its support, they are necessarily at first local, and in no way accord with the observed geographical differences that characterize particular regions, and which affect similarly many species belonging to widely different groups.

Meanwhile, Doctor Allen was pursuing on a larger scale the studies in distribution, of which the earlier results were announced in the fifth part of the Mammals and Winter Birds of East Florida, and in 1878 there appeared his paper on "The geographical distribution of mammals, considered in relation to the principal ontological regions of the earth and the laws that govern the distribution of animal life." (Bull. U. S. Geol. Surv. IV, No. 2, pp. 313–377.)

The fact that Wallace in his classic, Geographical Distribution of Animals (1876), adopted the faunal regions proposed by Sclater, gave Doctor Allen an additional incentive to prove the incorrectness of faunal boundaries which are not based primarily on climatic zones. "One of the reasons given by Mr. Wallace for adopting Doctor Sclater's regions," he writes, "is that 'it is a positive, and by no means unimportant advantage to have our regions approximately equal in size and with easily defined, and therefore easily remembered boundaries,' " to which Doctor Allen adds: "These arguments can be scarcely characterized as otherwise than trivial, since they imply that truth, at least to a certain degree, should be regarded as secondary to convenience."

Wallace, commenting on the criticism of Sclater's faunal regions made by Doctor Allen in 1871, said: "The author continually refers to the '*law of the distribution of life in circumpolar zones*,' as if it were one generally accepted and that admits of no dispute. But this supposed 'law' only applies to the smallest details of distribution—to the range and increasing or decreasing numbers of *species* as we pass from north to south, or the reverse; while it has little bearing on the great features of zoological geography—the limitation of groups of *genera* and *families* to certain areas. It is analogous to the '*law of adaptation*' in the organization of animals, by which members of various groups are suited for an aerial, an aquatic, a desert, or an arboreal life; are herbivorous, carnivorous, or insectivorous; are fitted to live underground, or in fresh waters, or on polar ice. It was once thought that these adaptive peculiarities were suitable foundations for a classification,—that whales were fishes, and bats birds; and even to this day there are naturalists who cannot recognize the essential diversity of structure in such groups as swifts and swallows, sun-birds and hummingbirds. under the superficial disguise caused by adaptation to a similar mode of life."

Doctor Allen was not slow to accept this challenge of the correctness of his fundamental principles, replying:

> I unblushingly claim, in answer to the main point, that the geographical distribution of life is *by necessity* in accordance with *a "law of adaptation,"* namely, of *climatic adaptation;* that such a law is legitimate in this connection, and that the reference to the "superficial disguise" adapting essentially widely different organisms to similar modes of life is wholly irrelevant to the point at issue—a comparison of things that are in any true sense incomparable; furthermore, that the "law of distribution of life in circumpolar zones" does apply as well in a general sense as to details—"to groups of genera and families" as well as to species.

He then advances the theory of dispersal of life southward from the arctic which has since been so ably developed by Matthew, writing:

> In this connection it may be well to recall certain general facts previously referred to respecting the geographical relations of the lands of the northern hemisphere and their past history. Of first importance is their present close connection about the northern pole and their former still closer union at a comparatively recent date in their geological history; furthermore, that at this time of former, more intimate relationship, the climatic conditions of the globe were far more uniform than at present, a mild or warm-temperate climate prevailing where now are regions of perpetual ice, and that many groups of animals whose existing representatives are found now only in tropical or semitropical regions lived formerly along our present Arctic coast. We have, hence, an easy explanation of the present distribution of such groups as Tapirs, Manatees, many genera of Bats, etc., in the tropics of the two hemispheres, on the wholly tenable assumption of a southward migration from a common wide-spread northern habitat, to say nothing of the numerous existing arctopolitan and semi-cosmopolitan genera. . . . The succeeding epochs of cold caused extensive migrations of some groups and the extinction of others; with the diverse climatic conditions subsequently characterizing high and low altitudes came the more pronounced differentiation of faunae, and the development, doubtless, of many new types adapted to the changed conditions of life—the development of boreal types from a warm-temperate or semi-tropical stock. The accepted theories respecting the modification of type with change in conditions of environment—changes necessarily due mainly to climatic influences—render it certain that if animals are so far under the control of circumstances dependent upon climate, and emphatically upon temperature, as to be either exterminated or greatly modified by them, the same influences must govern their geographical distribution.

Further study induced Doctor Allen to modify somewhat the views advanced in his Museum of Comparative Zoology essay of 1871, "especially in relation to the divisions of the Australian Realm, and to unite the South African Temperate with the Indo-African, as a division of the latter, and also to recognize Madagascar and the Mascarene Islands as forming together an independent primary region, in accordance with the view of Sclater, Wallace, and others."

He then presents in detail the evidence on which his conclusions are based, including a treatment of the regions and provinces contained in his realms, and after a general summary of the data supporting the belief that temperature is the most potent factor governing the distribution of life adds:

Hence, given: 1. Arctic and cold-temperate conditions of climate, and we have a fauna only slightly or moderately diversified; 2. A moderate increase of temperature, giving warm-temperate conditions of climate, and we have the addition of many new types of life; 3. A high increase of temperature, giving tropical conditions of climate, and we have a rapid multiplication of new forms and a maximum of differentiation. Again, given: 1. A long-continued continuity of land surface, and we have an essential identity of fauna; 2. A divergence and partial isolation of land-areas, and we find a moderate but decided differentiation of faunae; 3. A total isolation of land-areas, and we have a thorough and radical differentiation of faunae, proportioned to the length of time the isolation has continued. Hence, the present diversity of life is correlated with two fundamental conditions: 1. Continuity or isolation, past as well as present, of land surface; and, 2. Climatic conditions, as determined mainly by temperature.[3]

In accordance with these principles, which rest on incontrovertible facts of distribution, it follows that the nearly united lands of the North present a continuous, almost homogeneous, arctopolitan fauna; that farther southward, in the warmer temperate latitudes, we begin to find a marked differentiation on the two continents; that this differentiation is still further developed in the tropical continuations of these same land-areas, till an almost total want of resemblance is reached, except that there is what may be termed, in contrast with the more northern regions, a "tropical *facies*" common to the two. The small amount of land surface belonging to these primary land regions south of the tropics have no more in common (a few marine species excepted) than have these two tropical areas, but it is hardly possible for them to have much less. The Antarctic (mainly oceanic) region has a fauna strongly recalling the marine fauna of the Arctic, but has no resemblance to that of the intervening area.

The northern circumpolar lands may be looked upon as the base or centre from which have spread all the more recently developed forms of mammalian life, as it is still the bond that unites the whole.

Subsequently published papers on distribution treated in detail of the mammals and birds of North America and were in the main elaborations of his earlier contributions to zoogeography.

It was natural that Doctor Allen's faunistic studies should arouse his interest in the closely allied subject of migration and, in 1880, he published a brief paper on the "Origin of the instinct of migration in birds," in which he developed the theory that the seasonal movements of birds are, primarily, due to climatic changes occasioned by glaciation. He said:

Nothing is doubtless more thoroughly established than that a warm-temperate or sub tropical climate prevailed down to the close of the Tertiary epoch, nearly to the Northern Pole, and that climate was previously everywhere so far equable that the necessity of migration can hardly be supposed to have existed. With the later refrigeration of the Northern regions, bird life must have been crowded thence toward the tropics, and the struggle for life thereby greatly intensified. The less yielding forms may have become extinct; those less sensitive to climatic change would seek to extend the boundaries of their range by a slight removal northward during the milder intervals of summer, only, however, to be forced back again by the recurrence of winter. Such migration must have been at first "incipient and gradual," extending and strengthening as the cold wave receded and opened up a wider area within which existence in summer became possible. What was at first a forced migration would become habitual, and through the heredity of habit give rise to that wonderful faculty we term the instinct of migration.

The explanation here offered of the origin of bird migration remains to-day an accepted theory among students of this phenomenon.

While prosecuting these more philosophical researches, Doctor Allen was also devoting much time to the production of his classic memoir on The American Bisons, Living and Extinct, a quarto of some 250 pages, published in 1876, to his monographs of North American Rodentia (with Elliot Coues), and to his notable History of North American Pinnipeds, an octavo of some 800 pages which appeared in 1880.

After accepting the post of curator of birds and mammals in the American Museum of Natural History, the character of Doctor Allen's work of necessity changed. Curatorial, administrative, and editorial duties now demanded all of his time and strength. His thought

[3] In illustration of the above, it may be added that the circumpolar lands north of the mean annual of 36° F., or, in general terms, north of the fiftieth parallel, with approximately an area of about 12,500,000 square miles, have representatives of about 54 genera of mammals; tropical America, with an approximate area of about 5,000,000 square miles, has about 90 genera; the Indo-African Realm, with an approximate area of about 15,000,000 square miles, has about 250 genera. Hence the tropical lands are four to five times richer in genera, in proportion to area, than those of the cold-temperate and Arctic regions.

and energy were devoted to laying the foundation of the great research collections which will forever remain monuments to his power to impress the museum authorities with the need of acquiring specimens for study as well as for exhibition.

For the first few years of his curatorship he had no assistance and was himself forced to perform the clerical tasks of cataloguing and labeling specimens. But as material accumulated, he was relieved of these duties in order that he might prepare reports on the rapidly growing collections.

Thus his work became largely that of a systematist and during the succeeding 35 years a constant stream of authoritative papers, at first on birds and mammals but later exclusively on mammals, poured from his pen. This included monographic as well as faunal papers to the number of 165 on mammals and 37 on birds.

The nomenclatural questions involved in work of this nature had a growing attraction for Doctor Allen, and his genius for unraveling the tangles of synonymy and allied problems soon made him a recognized authority in this thankless field of labor and resulted in his election, in 1910, as a member of the Commission on Zoological Nomenclature of the International Congress of Zoology.

A regrettably large part of his time was devoted to the preparation of copy for the press and the reading of proofs, and, while the high standard of museum publications, both in matter and appearance, owes much to his expert care and sound judgment, one can not but feel that this editorial supervision might have been secured at less cost to his time.

The Auk made similar demands upon him, but his rare ability as a discriminating, broadly informed, fair-minded, unprejudiced critic was given opportunity for expression in the often elaborate reviews of current literature which he prepared during the 36 years of his editorship of the Nuttall Bulletin and its successor, The Auk.

The wide influence exerted by these reviews is convincingly stated in a letter to Henry Fairfield Osborn, president of the American Museum of Natural History, by Dr. Joseph Grinnell, who writes:

> Of all the eastern ornithologists active during the past thirty-five years I believe that Dr. Allen wielded the greatest influence in the field of serious scientific ornithology out here on the Pacific Coast. It was through the columns of "The Auk," especially in the review department of that journal, that Dr. Allen exercised this influence. I think others of the younger bird students here in the West would agree with me that our conceptions in systematic zoology and geographical distribution were molded more importantly by reason of Dr. Allen's sane criticisms and comments in his various reviews than through what we read in other articles and in books covering the same ground. I know that this was true in my own case.

HONORS

Doctor Allen's retiring disposition made it difficult for him even to appear before his scientific colleagues with justice to himself or to the paper he presented. The recognition, therefore, which his work received was due to its inherent scientific value.

He was awarded the Humboldt scholarship by the Lawrence Scientific School in 1871, the Walker grand prize by the Boston Society of Natural History in 1903, and the medal of the Linnaean Society of New York City in 1916, and in 1886 he was given an honorary Ph. D. by the University of Indiana.

He was elected to membership in the National Academy of Sciences in 1876, was a founder of the American Ornithologists' Union in 1883, and was annually reelected to its presidency from that date until 1891; he was an honorary fellow of the London Zoological Society (1901), an honorary member of the British Ornithologists' Union (1907), an honorary member of the New York Zoological Society (1897), to mention only the more important of the societies on whose roll his name appears, and always, he wrote, these honors came to him as a "surprise."

From a bibliography of over 1,400 titles covering the period from August, 1860, to August, 1916, which was issued with Doctor Allen's Autobiographical Notes,[4] the more important titles have been selected for republication here. To these there have been added references to all Doctor Allen's scientific publications which have appeared since August, 1916, thus completing his bibliography.

---

[4] Autobiographical Notes and a Bibliography of the Scientific Publications of Joel Asaph Allen. American Museum of Natural History, 1916.

BIBLIOGRAPHY

BIRDS

Birds of New England.<*New England Farmer* (weekly), for Aug. 11, 25, Sept. 22, Oct. 6, 20, Nov. 3, 17, Dec. 1, 22, 1860; Jan. 5, 26, March 2, May 18, June 15, July 20, Sept. 21, Oct. 26, Nov. 23, 1861; April 26, July 26, Aug. 16, Sept. 13, Oct. 18, 25, Nov. 8, Dec. 6, 1862. Also, published in the same newspaper (monthly), large 8vo, for Sept., Oct., Nov., Dec., 1861, January, March, May, June, July, Sept., Nov., Dec., 1862, and Jan., 1863.

"Twenty-five articles in all, taking the birds of New England in order from Accipitres to the middle of the Fringillidæ. Popular biographical accounts, written to interest farmers in the feathered life of their fields."—(*Coues*, "Bibliography of North American Ornithology," in "Birds of the Colorado Valley," 1878, p. 661.)

On the Mammals and Winter Birds of East Florida, with an Examination of certain assumed Specific Characters in Birds, and a Sketch of the Bird-Faunæ of Eastern North America.<*Bull. Mus. Comp. Zoöl.*, II, No. 3, pp. 161–450, pll. iv–viii, April, 1871.

"Part I, The Topographical, Climatic, and Faunal characteristics of East Florida. (Part II, On Mammals.) Part III, On Individual and Geographical Variation among Birds, considered in respect to its bearing upon the value of certain assumed specific characters—a highly important philosophic treatise upon the general subject, which is discussed at length with force and logical consistency; the author's broad views upon this subject had at once a marked influence upon ornithological thought. Variation in general size and proportion of parts, both individual and climatic, are illustrated with numerous tables of measurements. An essay on species and varieties follows. Part IV, List of the Winter Birds of East Florida, with annotations—field notes, measurements, and much synonymy and technical criticism. Part V, On the Geographical Distribution of the Birds of Eastern North America, with special reference to the number and circumscription of the Ornithological Faunæ. After general introductory remarks, the natural Provinces of the North American Temperate Region are discussed, and the Ornithological Faunæ of the Eastern Provinces are treated. The following Faunæ are laid down and characterized:—1. Floridan. 2. Louisianian. 3. Carolinian. 4. Alleghanian. 5. Canadian. 6. Hudsonian. 7. American Arctic. Various tabular summaries follow, with general remarks on the distribution and migration of the birds of the Eastern Province. A copious bibliography of American Ornithological literature concludes. The plates illustrate the variation in the bill of many species. The article gained the Humboldt Scholarship, and is one of the most important of American ornithological works. *Cf. Ibis*, 1872, pp. 189–191; *Zool. Rec.* for 1871, pp. 24, 25; *Am. Nat.*, V. 1871, pp. 364–373."—(*Coues, l. c.*, p. 686.)

Notes of an Ornithological Reconnaissance of Portions of Kansas, Colorado, Wyoming, and Utah.<*Bull. Mus. Comp. Zoöl.*, III, No. 6, pp. 113–183, July 10, 1872.

Notes on the Natural History of Portions of Dakota and Montana Territories, being the substance of a report to the Secretary of War on the Collections made by the North Pacific Railroad Expedition of 1873, Gen. D. S. Stanley, Commander.<*Proc. Boston Soc. Nat. Hist.*, XVII, 1874, pp. 33–86. Also separately published, 8vo, Boston, 1874, pp. 1–61.

"III, Report on the Birds, pp. 44–68. 118 spp., fully annotated, the list preceded by general consideration of the avifauna of the region, and several partial local lists. An important contribution."—(*Coues, l. c.*, p. 699.)

The Influence of Physical Conditions in the Genesis of Species*R*<*adical Review*, I, pp. 108–140, May, 1877.

On the influence of environment in modifying forms of mammals and birds, and discussion of Darwin's theory of the origin of species by natural selection. Reprinted (by request), with slightly modernized nomenclature by the author, in Ann. Rep. Smiths. Institution for 1905 (1906).

A list of the Birds of Massachusetts, with Annotations.<*Bull. Essex Inst.*, X, pp. 3–37, 1878.

"This may be considered to supersede previous tracts on the same subject, both by the present and other authors, as it completely sums our knowledge of the subject. The paper opens with summary considerations, followed by a valuable historical résumé. 1. Species of authentic occurrence within the State, 317. 2. Extirpated, 4. 3. Of probable occurrence, 24. 4. Hypothetical and doubtful species, 3 (*Myiodioctes minutus*, *Empidonax pygmæus* of Minot, *Thaumatias linnæi*). 5. Introduced undomesticated species, 6.—'Considered as fairly entitled to recognition as Massachusetts birds,' 340. Known to breed in the State, about 135. Extremely rare or accidental visitors, 90. North American species added since 1867, 35."—(*Coues, l. c.*, p. 736).

Origin of the Instinct of Migration in Birds.<*Bull. Nutt. Ornith. Club*, V, pp. 151–154, July, 1880.

Attributed to change of climate at the close of the Tertiary period. "What was at first a forced migration would soon become habitual, and through the heredity of habit give rise to that wonderful faculty we term the instinct of migration" (p. 153).

The Present Wholesale Destruction of Bird-Life in the United States.<*Science*, VII, No. 160, pp. 191–195, Feb. 26, 1886. Also, *Bulletin No. 1*, of the A. O. U. Committee on Protection of Birds, pp. 1–5, March, 1886.

A Revised List of the Birds of Massachusetts.<*Bull. Amer. Mus. Nat. Hist.*, I, No. 7, pp. 221–271, July, 1886.

An annotated list of 340 species, plus 4 species extirpated, 4 introduced, and 19 of probable occurrence. The third and last list of the birds of Massachusetts published by this author.

On the Structure of Birds in Relation to Flight, with Special Reference to Recent Alleged Discoveries in the Mechanism of the Wing.<*Trans. New York Acad. Sci.*, pp. 89–100, 1888.

To What Extent is it Profitable to Recognize Geographical Forms among North American Birds?<*Auk*, VII, pp. 1–9, Jan., 1890.

A protest against too fine splitting.

On a Collection of Birds from Chapada, Matto Grosso, Brazil, made by Mr. Herbert H. Smith. Part I, Oscines. <*Bull. Amer. Mus. Nat. Hist.*, III, pp. 337–380, Sept. 29, 1891.

87 species here treated: *Calliste margaritæ*, sp. nov., p. 351; *Zonotrichia capensis costaricensis*, subsp. nov., p. 374.

Part II, Tyrannidæ.<*Bull. Amer. Mus. Nat. Hist.*, IV, pp. 331–350, Dec., 1892.

Parts III and IV, Pipridæ to Rheidæ.<*Bull. Amer. Mus. Nat. Hist.*, V, 1893, pp. 107–158, July 19, 1893.

Spp. and subspp. nov: (1) *Pygmornis chapadensis*, pp. 122; (2) *Piaya cayana cabanisi*, p. 136; (3) *Buteo albicaudatus sennetti*, p. 144; This concludes the paper, which comprises a list of 324 species, with annotations and comment on allied species in a number of groups. Part IV (pp. 152–158), "Oological Notes," contains notes on the nests and eggs of 27 species.

The North American Species of *Colaptes*, considered with special reference to the Relationships of *C. auratus* and *C. cafer*.<*Bull. Amer. Mus. Nat. Hist.*, IV, 1892, pp. 21–44, and map, March 8, 1892.

Hybridization on a large scale shown to obtain between *C. auratus* and *C. cafer*.

The Geographical Origin and Distribution of North American Birds, considered in Relation to Faunal Areas of North America.<*Auk*, X, pp. 97–150, pls. iii–v (maps), April, 1893.

Merriam's Laws of Temperature Control of the Distribution of Land Animals and Plants.<*Auk*, XII, pp. 172–173, April, 1895.

Review of Dr. C. Hart Merriam's paper of this title in *Nat. Geogr. Mag.*, VI, 1894, pp. 229–238, pls. xii–xiv.

The Origin and Relations of the Floras and Faunas of the Antarctic and adjacent Regions. Vertebrata of the Land; Birds and Mammals.<*Science* (2), III, No. 61, pp. 317–319, Feb. 28, 1896.

Birds offer no satisfactory evidence of a former "Antarctic Continent."

Alleged Changes of Color in the Feathers of Birds without Molting.<*Bull. Amer. Mus. Nat. Hist.*, VIII, pp. 13–44, March 18, 1896.

"It is a summary and criticism of the work of some of the more important writers upon the subject of color changes in feathers without moult, and it deals unsparingly with those who have asserted as possible the complete rejuvenation of an abraded feather. . . ."—J. Dwight, Jr., in *Auk*, XIII, pp. 166–167, April, 1896.

List of Birds collected in the District of Santa Marta, Colombia, by Mr. Herbert H. Smith.<*Bull. Amer. Mus. Nat. Hist.*, XIII, pp. 117–183, Aug. 25, 1900.

An annotated list of 388 species. Spp. nov.: (1) *Odontophorus atrifrons*, p. 127; (2) *Myiobius assimilis*, p. 144; (3) *Ochthœca jesupi*, p. 151; (4) *Ochthœca olivacea*, p. 152; (5) *Attila parvirostris*, p. 153; (6) *Attila rufipectus*, p. 153; (7) *Grallaria bangsi*, p. 159; (8) *Hylophilus brunneus*, p. 171.

960.[1] The Birds of Britain, their Distribution and Habits. *Science*, XLV, p. 591, June 8, 1917.

Review of A. H. Evans' book of this title (Cambridge, 1916. 8vo, pp. xii+275, numerous halftone text figs.).

961.[1] Wayne's A List of Avian Species for which the Type Locality is South Carolina. *Auk*, XXXIV, p. 346, July 1917.

Review of the paper of Arthur T. Wayne by this title (Contributions Charleston Museum, III, pp. i–vi, 1–8, 1917).

MAMMALS

Catalogue of the Mammals of Massachusetts, with a critical revision of the species.<*Bull. Mus. Comp. Zoöl.*, I, No. 8, pp. 143–252, Oct., 1869.

68 species, with critical and other extended annotations. The revisional comment was unfortunately based largely on the literature of the subject, in the absence of actual specimens of many of the forms considered.

On the Eared Seals (Otariadæ), with detailed descriptions of the North Pacific Species, by J. A. Allen. Together with an Account of the Habits of the Northern Fur Seal (*Callorhinus ursinus*), by Charles Bryant.< *Bull. Mus. Comp. Zoöl.*, II, No. 1, pp. 1–108, pll. i–iii, August, 1870.

I. (1) Résumé of recent contributions to the natural history of the Otariadæ, pp. 4–19; (2) affinities, distinctive characters and synonymy, with remarks on sexual, age and individual variation, and a conspectus of the genera and species, pp. 19–45; (3) on the North Pacific species, pp. 45–89. II. Habits of the Northern Fur Seal, etc., by Charles Bryant, with notes by J. A. Allen, pp. 89–108. *Oulophocinæ*, p. 44, and *Trichiphocinæ*, p. 44, subfamm. nov. of Otariadæ; subsequently abandoned.

On the Mammals and Winter Birds of East Florida, with an examination of certain assumed specific characters in Birds, and a sketch of the Bird-faunæ of Eastern North America.<*Bull. Mus. Comp. Zoöl.*, II, No. 3, pp. 161–450, pll. 4–8, April, 1871.

Annotated list of the mammals, 35 species, pp. 168–185. *Trichechus manatus* Linné employed as the name of the Manatee (p. 171).

Notes on the Mammals of portions of Kansas, Colorado, Wyoming, and Utah.<*Bull. Essex Inst.*, VI, pp. 43–66, May, 1874.

Four distinct lists of mammals observed in the region named in the title, with notes on their habits, distribution, etc. I. Mammals of Middle and Western Kansas, 28 species, with three pages on *Cynomys ludovicianus*, pp. 45–52. II. Mammals of Park County, Colorado, 37 species, pp. 53–58. III. Mammals of Carbon County, Wyoming, 32 species, pp. 58–61. IV. Mammals of Great Salt Lake Valley, Utah, 42 species, pp. 61–66.

Notes on the Natural History of portions of Dakota and Montana Territories, being the substance of a report to the Secretary of War, on the collections made by the North Pacific Railroad Expedition of 1873, Gen. D. S. Stanley, commander.<*Proc. Boston Soc. Nat. Hist.*, XVII, pp. 33–91, June, 1874. (Also as a separate pamphlet of 61 pp., 1874.)

Mammals, pp. 36–45 (pp. 6–13 of the reprint), 31 species; notes on habits, distribution, etc.

[1] These titles are additions to the original bibliography, the numbers preceding them continue its system of enumeration.

On Geographical Variation in Color among North American Squirrels; with a list of the Species and Varieties of the American Sciuridæ occurring north of Mexico.<*Proc. Boston Soc. Nat. Hist.*, XVI, pp. 276–294, June, 1874.

Variation in color, pp. 276–286; list of species and varieties, pp. 286–294. Genera: *Sciurus, Sciuropterus, Tamias, Spermophilus, Cynomys, Arctomys.* Species, 25, with 19 additional varieties=44 forms. Vars. nov.: (1) *Tamias quadrivittatus* var. *pallidus*, p. 289; (2) *Spermophilus tridecem-lineatus* var. *pallidus*, p. 291; (3) *Spermophilus parryi* var. *kodiacensis*, p. 292.

Geographical Variation among North American Mammals, especially in respect to size.<*Bull. U. S. Geol. Surv. Terr.*, II, No. 4, pp. 309–344, July 1, 1876.

Memoirs of the Geological Survey of Kentucky. | N. S. Shaler, director. | Vol. I, Part ii. | – | The American bisons, | living and extinct. | By J. A. Allen. | With twelve plates and map. | – | University press, Cambridge: | Welch, Bigelow & Co. | 1876. Also:

Memoirs of the Museum of Comparative Zoölogy, at Harvard College, Cambridge, Mass. | Vol. IV, No. 10. | - | The American bisons, living and extinct: | By J. A. Allen | Published by permission of N. S. Shaler, director of the Kentucky | Geological Survey. With 12 plates and a map. | University press, Cambridge: | Welch, Bigelow & Co. | 1876.

4°, pp. i–ix, 1–246, 1 col. map, 12 pll. 13ll., 2 woodcuts in text. Edition of 500 copies.

These two publications were simultaneous, and only differ in the titles. The following are the contents:

Title, p. i.
Preliminary note (by N. S. Shaler), p. iii.
Introduction, pp. v–ix.

Part I

1. Distinctive characteristics and affinities of the bisons, pp. 1–3.
2. General historical account of the remains of extinct bisons hitherto found in North America, pp. 3–7.
3. Description of the extinct species, pp. 7–31.
4. Geographical distribution and geological position of the remains of the extinct bisons of North America, pp. 32–35.
5. Relation of the existing species of bisons to the extinct species, pp. 35–36.
6. Description of the existing species, pp. 36–70.

Part II

1. Geographical distribution, past and present, of *Bison americanus*, pp. 71–191.
2. Products of the buffalo, pp. 191–201.
3. The chase, pp. 202–215.
4. Domestication of the buffalo, pp. 215–221.

Monographs of North American Rodentia. By Elliott Coues and Joel Asaph Allen.=Report of the U. S. Geol. Survey of the Territories (F. V. Hayden).

The Geographical Distribution of the Mammals, considered in relation to the principal ontological regions of the earth, and the laws that govern the distribution of animal life.<*Bull. U. S. Geol. and Geogr. Surv. Terr.*, IV. No. 2, pp. 313–377, May, 1878.

Contents: I. Distribution of mammalian life in the Northern Hemisphere, considered in relation to laws of geographical distribution, pp. 313–329.—Historical résumé, with critical analysis of views of Sclater and Wallace.

II.—Mammalian regions of the Globe, pp. 329–373.—(1) Arctic Realm; (2) North Temperate Realm; (3) Tropical Realm; (4) South American Temperate Realm; (5) Indo-African Realm; (6) Australian Realm; (7) Lemurian Realm; (8) Antarctic Realm.

The realms are divided into regions and provinces.

III.—General Summary, pp. 373–377, with diagram of realms, regions, and provinces.

History of North American Pinnipeds: A Monograph of the Walruses, Sealions, Sea-bears and Seals of North America. Washington: Govern. Printing Office, 1880.=U. S. Geol. and Geogr. Surv. Territories (F. V. Hayden, U. S. Geol.-in-Charge), Misc. Publ. No. 11. 8°, pp. i–xvi, 1–785, 60 woodcuts.

Family Odobænidæ, pp. 5–186. *Odobænus* Brisson (1762) the proper generic name for the walruses, and Odobænidæ the proper name for the family—not *Trichechus* (auct. non Linné), nor Trichechidæ, which are, respectively, the proper names of the Manatees;[1] two species of walrus established, *O. rosmarus* (Linn.) and *O. obesus* (Illiger), with figures of skulls, and full history of each species, including the nomenclature, osteology and dentition, geographical distribution, chase, and commercial products.

Family Otariidæ, pp. 187–411. Technical and commerical history, with synopsis, characters, and geographic distribution of the extra-limital species, recent and fossil, as well as the North American.

Family Phocidæ, pp. 412–756. Technical history of the family, classification, synonymatic list of the genera and species, geographical distribution, fossil remains, milk-dentition, habits, migrations, locomotion on land, the seal-hunting industry and sealing-grounds, methods of capture, species hunted, products, decrease of seals from injudicious hunting, etc., pp. 412–557; systematic treatment of the North American species, pp. 557–756; Appendix: *a*, material examined (pp. 757–764); *b*, additions and corrections (pp. 765–773); index (pp. 775–785).

"A most valuable and complete history of these animals, especially of those found in North America, of their distribution and pursuit, with full synonymy, and copious tables and references. The history of the species of the group generally is also discussed, with remarks on their synonymy and distribution."—Zoöl. Rec. for 1880, Mamm. p. 2.

Preliminary List of Works and Papers relating to the Mammalian Orders Cete and Sirenia.<*Bull. U. S. Geol. and Geogr. Surv.*, VI, No. 3, pp. 399–562, Aug. 30, 1882.

Covers the period from Albertus Magnus (1495) to the year 1840, and numbers 1013 annotated titles, the annotations in many cases amounting to a full statement of contents, so far as pertinent to the present subject, including names of species and genera and nature of treatment. All thus far published. The cause of the discontinuance of publication is explained in an insert, as follows:

"Owing to the illness of the author, which prevented his revision of the proof-sheets, it was necessary to stop the printing of the 'List' at the end of the year 1840. The present instalment comprises only a little more than one-third of the article; the remainder will be published as soon as the author's health renders it practicable.—J. A. Allen, Cambridge, Sept., 1882."

See further, a "Personal Note" in *Bull. Amer. Mus. Nat. Hist.*, XXIV, 1908, pp. 279–280.

The West Indian Seal (*Monachus tropicalis*). <*Bull. Amer. Mus. Nat. Hist.*, II, pp. 1–34, pls. i–iv, April 25, 1887.

Introduction, pp. 1–3; external characters, pp. 4–6; osteological characters, pp. 6–19; sexual differences, pp. 20–21; affinities of the genus *Monachus*, pp. 22–23; general history, pp. 23–26; geographical distribution, pp. 27–29; habits, pp. 29–34.

A Review of some of the North American ground Squirrels of the genus *Tamias*.<*Bull. Amer. Mus. Nat. Hist.*, III, pp. 45–116, June, 1890.

24 species and subspecies are recognized, of which 13 are here first described, as follows: (1) *Tamias obscurus*, p. 70; (2) *T. senex*, p. 83; (3) *T. speciosus* (Merriam, ined. MS.), p. 86; (4) *T. frater*, p. 88; (5) *T. amœnus*, p. 90; (6) *T. cinereicollis*, p. 94; (7) *T. umbrinus*, p. 96; (8) *T. quadrivittatus gracilis*, p. 99; (9) *T. q. luteiventris*, p. 101; (10) *T. q. affinis*, p. 103; (11) *T. q. neglectus*, p. 106; (12) *T. minimus consobrinus*, p. 112; (13) *T. m. pictus*, p. 115.

The Geographical Distribution of North American Mammals.<*Bull. Amer. Mus. Nat. Hist.*, IV, pp. 199–244, pls. v–viii (maps), 1892.

Influences determining the geographic distribution of life (climatic), pp. 199–200; interrelation of land areas, pp. 200–201; mammals as the basis for the classification of life areas, pp. 202–203; systematic classification of life areas, pp. 203–211; the Sclaterian system, pp. 211–212; the mammals of North America considered in relation to the North American Region and its subdivisions, pp. 213–240, with 3 maps.

A synopsis of the Pinnipeds, or Seals and Walruses, in relation to their commercial history and products.<Fur-seal Arbitration. Appendix to the case of the United States before the Tribunal of Arbitration, etc., I, 1892, pp. 367–391.

Revision of the Chickarees, or North American Red Squirrels (subgenus *Tamiasciurus*).<*Bull. Amer. Mus. Nat. Hist.*, X, pp. 249–298, August 31, 1898.

Monographic revision. Subspp. nov.: (1) *Sciurus hudsonicus baileyi*, p. 261; (2) *Sciurus hudsonicus ventorum*, p. 263; (3) *Sciurus hudsonicus streatori*, p. 267; (4) *Sciurus douglasii cascadensis*, p. 277; (5) *Sciurus fremonti neomexicanus*, p. 291.

Sciurus douglasii mollipilosus Aud. & Bachm. revived to replace *Sciurus hudsonicus orarius* Bangs, 1897, p. 277.

The Musk-oxen of Arctic America and Greenland.<*Bull. Amer. Mus. Nat. Hist.*, XIV, pp. 69–86, pls. xiii–xvii, and 7 text figures, March 27, 1901.

Principally on the Ellesmere Land form, here referred to *Ovibos wardi* Lydekker (=*O. pearyi* Allen, Ms.).

A Preliminary Study of the South American Opossums of the Genus *Didelphis*.<*Bull. Amer. Mus. Nat. Hist.*, XVI, pp. 249–279, Aug. 18, 1902.

A monographic revision, with copious tables of measurements. Subspp. nov.: (1) *Didelphis marsupialis insularis*, p. 259; (2) *D. m. etensis*, p. 262; (3) *D. paraguayensis ondina*, p. 272; (4) *D. p. meridensis*, p. 274. *D. paraguayensis* Oken (1816) replaces *D. aurita* Temminck (1825).

The Hair Seals (Family Phocidæ) of the North Pacific Ocean and Bering Sea.<*Bull. Amer. Mus. Nat. Hist.*, XVI, pp. 459–499, with 10 text figures, Dec. 12, 1902.

Nomenclature; sexual differences in dentition; revision of the North Pacific species (11 species and subspecies are recognized), the following new; (1) *Phoca hispida gichigensis*, p. 488; (2) *Phoca ochotensis macrodens*, p. 483; (3) *Phoca stejnegeri*, p. 485; (4) *Phoca richardii pribilofensis;* (5) *Phoca richardii geronimensis*, p. 495.

Phoca nigra Pallas (p. 483 footnote) suggested as apparently available for *Callorhinus curilensis* (see *infra*, No. 192).

Mammalia of Southern Patagonia. Reports of the Princeton University Expeditions to Patagonia, 1896–1899, Vol. III, 1905, Part I, pp. 1–120, pls. i–xxix.

Detailed treatment of 55 species, with special reference to nomenclature, that of the genera as well as the species discussed historically; full tables of references under the genera and species, and an annotated bibliography (pp. 192–210) of 65 titles.

Eunothocyon, gen. nov., p. 153 (in text); *Carcinocyon*, gen. nov., p. 153 (in text); *Ctenomys osgoodi*, nom. nov., to replace *C. robustus* Allen, preoccupied, p. 191; *Conis sclateri* nom. nov. (p. 153) to replace *Canis microtis* Sclater, preoccupied.

Species figured: *Zaedyus ciliatus*, Pls. i–iii (animal, skeleton, and three skulls); *Kerodon australis* and *Ctenomys osgoodi*, pl. vii (skulls); *Ctenomys sericeus* and *C. colburni*, pl. viii (skulls); *Eligmodontia*, *Oryzomys* and *Oxymycterus*, pls. ix and x (skulls and dentition of various species); *Akodon*, pls. xi and xii (skulls and dentition of 6 species); *Phyllotis*, *Euneomys*, and *Reithrodon*, pls. xiii and xiv (skulls and dentition); *Arctocephalus australis* and *A. philippii*, pls. xv–xvii (skulls, three views of each); *Arctocephalus townsendi*, pls. xviii–xx (skull, three views); *Otaria byronia*, pl. xxi (skeleton); *Conepatus humboldti*, pl. xxii (skulls and dentition); *Cerdocyon griseus*, pl. xxiii (skull, three views); *Lynchailurus pajeros crucina*, pl. xxiv (skull and dentition); *Puma pearsoni*, pls. xxv and xxvi (colored figures of animal, red and gray phases); *Puma pearsoni*, pls. xxvii–xxix (three views of skull).

The North Atlantic Right Whale and its near Allies.<*Bull. Amer. Mus. Nat. Hist.*, XXIV, pp. 277–329, pls. xix–xxiv, and 1 text figure, April 8, 1908.

History, relationships, nomenclature, geographical distribution, and external and osteological characters of *Eubalæna glacialis* (Bonnaterre).

Ontogenetic and other Variations in Muskoxen, with a systematic Review of the Muskox Group, recent and extinct.<*Mem. Amer. Mus. Nat. Hist.*, New Series, I, Pt. 4, 1913, pp. 101–226, pls, xi–xviii, 1 map, and 45 text figs., March, 1913.

Ontogenesis of the horns, teeth, skull and pelage, pp. 107–143; individual differentiation as indicated by the skull, pp. 143–157; systematic review, including historical summary, pp. 157–160; geographic distribution, past and present, pp. 160–164; classification and relationship. 164–171; *Ovibos*, characters, alleged species and subspecies, pp. 171–179; geographic variation, pp. 179–180; synopsis of species and subspecies, pp. 180–182; systematic description, habits, and distribution: *Ovibos moschatus moschatus*, pp. 183–189; *O. m. niphœcus*, pp. 189–191; *O. m. wardi*, pp. 191–201; *O. yukonensis* (extinct), pp. 201–203; *O. pallantis* (extinct), pp. 203–205; extermination, pp. 205–207; Muskoxen in Zoölogical Gardens, pp. 207–208; *Bootherium*, pp. 209–213; *Symbos*, pp. 213–215; *Liops*, p. 216; bibliography, pp. 221–226.

Plates xi–xv, *O. m. wardi* as follows: pl. xi, horncores; pl. xii, transverse sections of horncores; pl. xiii, longitudinal sections of horncores; pl. xiv, sections of horncores; pl. xv, maxillary toothrow at different ages; pl. xvi, mandibular toothrow at different ages. Plates xvii and xviii, skull of *Symbos cavifrons*.

Text figures 1–26, skulls and dentition of *O. m. wardi* from fœtal age to senescence; text fig. 27, map of distribution, present and recent, of Muskoxen in North America and Greenland, text figs. 28–31, skulls of *O. m. moschatus* and *O. m. wardi;* text figs. 32–36, mounted specimens of same; text figs. 38–44, photographs of calves of *O. m. wardi* in New York Zoölogical Park; text fig. 45, type skull of *Bootherium bombifrons*.

Review of the South American Sciuridæ. <*Bull. Amer. Mus. Nat. Hist.*, XXXIV, pp. 147–309, pls. i–xiv, and 25 text figs., May 17, 1915.

Historical outline, pp. 151–158; general considerations, pp. 158–168; genera and subgenera of American squirrels, pp. 169–186; systematic review of the South American squirrels.

271.[1] The Whalebone Whales of New England. *Science*, XLV, pp. 89–90, Jan. 26, 1917.

Review of Glover M. Allen's paper of this title (*Mem. Boston Soc. Nat. Hist.*, VIII, No. 2, pp. 107–322, pls. 8–15, text figs. 1–12, Sept., 1916).

272. The American Museum Congo Expedition Collection of Bats. (With Herbert Lang and James P. Chapin.) *Bull. Amer. Mus. Nat. Hist.*, XXXVII, pp. 405–563, pls. xliv–lv, text figs. 1–26, and 1 map, Sept. 29, 1917.

Systematic list, 68 species and subspecies, with much technical comment, pp. 405–478; notes on the distribution and ecology of Central African Chiroptera, by Herbert Lang and James P. Chapin, pp. 479–496; field notes by Herbert Lang and James P. Chapin, on 68 species, pp. 497–560.

Subgen. nov.: *Lophomops*, p. 460; *Allomops*, p. 470; spp. and subspp. nov.: (1) *Nycteris pallida*, p. 425; (2) *Nycteris avakubia*, p. 426; (3) *Rhinolophus abæ*, p. 428; (4) *Rhinolophus axillaris*, p. 429; (5) *Hipposideros caffer niapu*, p. 431; (6) *Hipposideros abæ*, p. 432; (7) *Hipposideros nanus*, p. 434; (8) *Hipposideros langi*, p. 434; (9) *Hipposideros gigas niangaræ*, p. 438; (10) *Pipistrellus abaensis*, p. 442, (11) *Eptesicus ater*, p. 443; (12) *Eptesicus faradjius*, p. 444; (13) *Eptesicus garambæ*, p. 445; (14) *Glauconycteris humeralis*, p. 448; (15) *Glauconycteris alboguttatus*, p. 449; (16) *Miniopterus breyeri vicinior*, p. 450; (17) *Nyctinomus ochraceus*, p. 455; (18) *Chærephon frater*, p. 456; (19) *Chærephon russatus*, p. 458; (20) *Chærephon (Lophomops) chapini*, p. 461; (21) *Chærephon (Lophomops) cristatus*, p. 463; (22) *Chærephon (Lophomops) abæ*, p. 464; (23) *Mops congicus*, p. 467; (24) *Mops niangaræ*, p. 468; (25) *Mops trevori*, p. 469; (26) *Mops (Allomops) osborni*, p. 473; (27) *Mops (Allomops) occipitalis*, p. 474; (28) *Mops (Allomops) faradjius*, p. 476; (29) *Mops (Allomops) nanulus*, p. 477.

273. The Skeletal Characters of *Scutisorex* Thomas. *Bull. Amer. Mus. Nat. Hist.*, XXXVII, pp. 769–784, pls. lxxxix–xcii, text figs. 1–8, Nov. 26, 1917.

The extraordinary skeletal characters of *Scutisorex congicus* serve as a basis for raising the *Scutisorex* group to the rank of a subfamily of the Soricidæ under the name Scutisoricinæ, p. 781; with field notes by Herbert Lang, pp. 781–783.

274. The Laysan Seal. *Natural History*, Journ. Amer. Mus. Nat. Hist., XVIII, pp. 399–400, May, 1918.

Remarks on *Monachus schauinslandi* Matschie from Laysan Island and on the widely interrupted distribution of the genus *Monachus* in warm temperate and subtropical latitudes, now known only from the leeward group of Hawaiian Islands, and the Caribbean and Mediterranean seas.

275. Nelson's Wild Animals of North America. A Review. *Natural History*, Journ. Amer. Mus. Nat. Hist., XIX, pp. 331–333, 2 photographs, March, 1919.

Review of the paper of this title (Published by National Geographic Society, Washington, D. C., 1918).

276. Severtzow's Classification of the Felidæ. *Bull. Amer. Mus. Nat. Hist.*, XLI, pp. 335–340, Sept. 22, 1919.

A critical review of Severtzow's classification, with an annotated list of his genera and subgenera and comment on their validity.

277. Notes on the Synonymy and Nomenclature of the Smaller Spotted Cats of Tropical America. *Bull. Amer. Mus. Nat. Hist.*, XLI, pp. 341–419, figs. 1–31, Oct. 3, 1919.

Introduction, p. 343; list of currently recognized forms of smaller cats of Tropical America arranged in groups according to their obvious alliances, pp. 345–384; 37 forms recognized, of which 12 are rated as species and (excluding the typical races) 25 as subspecies, referred to 7 superspecific groups.

Gen. nov.: *Oncilla*, p. 358; subspp. nov.: *Margay glaucula nicaraguæ*, p. 357.

278. Preliminary Notes on African Carnivora. *Journ. Mammalogy*, I, pp. 23–31, Nov., 1919.

Preliminary report on some 600 specimens collected by The American Museum Congo Expedition, 1909–1915. Discussion of the generic names *Mungos* and *Herpestes*.

Nomen nov.: *Micraonyx* (for *Leptonyx*, preoccupied), p. 24; gen. and spp. nov.: *Osbornictis*, p. 25; *Osbornictis piscivora*, p. 25; *Xenogale*, p. 26; *Xenogale microdon*, p. 27.

279. Note on Gueldenstaedt's Names of Certain Species of Felidæ. *Journ. Mammalogy*, I, pp. 90–91, Feb., 1920.

Comments on the nomenclature of the North American bay lynx, whose correct technical name should be *Lynx rufa* Schreber.

280. The Technical Names of Two *Colobus* Monkeys. *Journ. Mammalogy*, I, pp. 96–97, Feb., 1920.

Reference to *Simia polycomos* Schreber as the genotype of *Colobus* Illiger and designation of *Simia badius* Kerr as the genotype of *Piliocolobus* Rochebrune.

281. Mammals of Panama. *Journ. Mammalogy*, I, pp. 188–189, August, 1920.

Review of Edward A. Goldman's work of that title (*Smithsonian Misc. Coll.*, LXIX, No. 5, pp. 1–309, pls. 1–39, text figs. 1–24, 1920).

POSTHUMOUS PUBLICATIONS

282. The American Museum Congo Expedition Collection of Insectivora. *Bull. Amer. Mus. Nat. Hist.*, XLVII, Art. 1, pp. 1–38, pls. 1–4, text fig. 1, July 20, 1922.

Deals with the Potamogalidæ, Erinaceidæ, Macroscelididæ, and Soricidæ collected by the Congo Expedition. Twenty one species and subspecies are listed, with much technical comment. Based on a collection of 377 specimens.

283. Sciuridæ, Anomaluridæ and Idiuridæ Collected by The American Museum Congo Expedition. *Bull. Amer. Mus. Nat. Hist.*, XLVII, Art. 2, pp. 39–71, pl. 5, Oct. 27, 1922.

These three families are represented by 480 specimens belonging to 20 forms, all discussed with considerable detail.

284. Carnivora Collected by The American Museum Congo Expedition. *Bull. Amer. Mus. Nat. Hist.*, XLVII, Art. 3, pp. 73–281, pls. 6–78, text figs. 1–67, 1 map, April 11, 1924.

The collection consists of 588 specimens representing 24 genera and 33 species distributed among the Canidæ, Mustelidæ, Viverridæ, Hyænidæ, and Felidæ. Much technical comment renders the report one of the most valuable contributions to African mammalogy.

[1] The following titles are additions to the original Bibliography. The numbers preceding them continue its system of enumeration.

285. Primates Collected by The American Museum Congo Expedition. *Bull. Amer. Mus. Nat. Hist.*, XLVII, Art. 4, pp. 283–499, pls. 79–167, text figs. 1–3, 1 map, Feb. 6, 1925.

The specimens numher 645, of which 66 represent the Lemuridæ, 549 the Lasiopygidæ, end 30 the Pongidæ. Among them ere 28 forms with one species new to science. There is one new genus, end one new generic name is proposed. A critical discussion of nomenclature, and other remarks give the report en euthoritative place among pepers on this group.

IV. ZOOGEOGRAPHY

On the Mammals and Winter Birds of East Florida . . . . and a Sketch of the Bird Faunæ of Eastern North America.<*Bull. Mus. Comp. Zoöl.*, II, No. 3, April, 1871, pp. 161–450.

Part V. On the Geogrephical Distrihution of the Birds of Eastern North Americe, with special reference to the Numher and Circumscription of the Ornithological Faunæ, pp. 375–425; List of Authorities, pp. 426–450.

"In accordance with the facts stated ahove respecting the mode of the distrihution of enimals end plants over the earth's surface, end the zoölogical and hotenical laws of the differentiation and mutual relations of the different regions, the following primary natural history divisions may he recognized: I, an Arctic Realm;[1] II, a North Temperete Realm; III, an American Tropical Realm; IV, an Indo-African Tropical Realm; V, a South American Temperete Realm; VI, an African Temperete Realm; VII, an Antarctic Realm; VIII, en Australien Realm" (p. 380).

For eastern North America are recognized the following seven faunæ: (1) Floridian, (2) Louisianian, (3) Carolinien, (4) Alleghanien, (5) Canadian, (6) Hudsonian, (7) American Arctic. Their houndaries end their characteristic species of hirds are given (pp. 387–404), and they are further considered with reference to memmals end reptiles (pp. 404–406).

The species of North American hirds are considered end tehulated with reference to their geogrephical renges (pp. 407–418). General remarks on the distrihution end migration of the hirds of the Eastern Province (pp. 418–425). A Bihliogrephy of ornithological works end pepers, or "List of Authorities," relating to North America occupies pages 426–450, geographically arrenged hy States and countries and numbering 346 titles—much the lergest list of papers relating to North American ornithology that eppeered prior to 1878.

The Geographical Distribution of the Mammalia, considered in relation to the principal Ontological Regions of the Earth, and the Laws that govern the Distribution of Animal Life.<*Bull. U. S. Geol. and Geogr. Surv. Terr.*, IV, No. 2, pp. 313–377, May 3, 1878.

I. General considerations, with criticism of the life-regions proposed hy Dr. P. L. Scleter end supported by Mr. Alfred R. Wallece, pp. 313–329; II. Mammalian Regions of the Globe; pp. 329–373; III. Generel Summary, pp. 373–377.

The primary divisions are essentially as laid down in 1871 (see ahove), except that a South African Temperate Realm is admitted, end Madagascar is recognized as an edditional Realm, designated as the Lemurian Realm. Under these are defined regions of secondary and tertiary rank where such subdivisions seemed to he required.

The Geographical Distribution of North American Mammals.<*Bull. Amer. Mus. Nat. Hist.*, IV, pp. 199–243, pls. v–viii (colored maps), Dec. 29, 1892.

Influences determining the Geogrephical Distrihution of Life, pp. 199–203; Systematic Classification of Life Areas, pp. 203–206; Primary Life Regions, pp. 206–207 (same as in No. 2 *supra*); North Temperate Realm, pp. 207–211; The Sclaterian System, pp. 211–212; The Mammals of North America considered in reletion to the North American Region end its Suhdivisions, pp. 213–240; Tropical North America, pp. 240–243; Tahular Synopsis, p. 243.

The Geographical Origin and Distribution of North American Birds, considered in relation to Faunal Areas of North America.<*Auk*, X, pp. 97–150, pls. iii, iv (colored maps), July, 1893.

I. The Geographical Origin and Distrihution of North American Birds, pp. 98–117; II. The Feunal Suhdivisions of North America, considered with reference to their Relationships, Classification, end Nomenclature, pp. 117–150 (tehular synopsis, p. 150).

The classification edopted is essentially the same as that recognized in 1892 (see *supra*, No. 5).

V. EVOLUTION

Mammals and Winter Birds of East Florida, etc.<*Bull. Mus. Comp. Zoöl.*, II, No.3, pp. 161–450, April, 1871.

Part III. On Individual end Geographical Variation among Birds, considered in respect to its hearing upon the Value of certain assumed Specific Charecters, pp. 186–250.

Wide renge of individual variation shown to occur in a considerahle numher of species, with extensive tables of measurements, pp. 186–226; correlation of variations in general size, size of hill, etc., end in coloration, with differences in climatic end geographic conditions, pp. 229–242; species, varieties, end geographical races, pp. 242–250. A presentetion of facts, without discussion of eny theories of evolution, which eppeared later.

Geographical Variation in North American Birds.<*Proc. Boston, Soc. Nat. Hist.*, XV, pp. 212–219, Dec., 1872.

A general résumé of the euthor's studies of the suhject, to that date. (Repuhlished in *Amer. Nat.*, VIII, pp. 534–541, Sept., 1874.)

Geographical Variation among North American Mammals.<*Bull. Geogr. and Geol. Surv. Terr.*, II, No. 4, July 1, 1876, pp. 309–344.

The correlation of size with geogrephical verietion is formuleted (p. 310) under the three propositions:

"(1) Maximum physical development of the individual is ettained where the conditions of environment are most favorahle to the life of the species . . . ."

The influence of Physical Conditions in the Genesis of Species.<*Radical Review*, I, No. 1, pp. 108–140, May, 1877. (Republished by request, in the Ann. Report of the Smithsonian Institution for 1905 (1906), pp. 375–402.)

"The doctrine of netural selection, or the survival of the fittest, has recently heen hrought forward as the key to this complex problem and is upheld hy a large cless of enthusiastic adherents, who accept it as the full solution of the whole question. By others the conditions of environment are helieved to he far more influential in effecting a certain class of modifications, et least, than the necessarily precerious influence of natural selection," etc.

The direct modifying influence of environment as a fector in evolution is regarded as more potent than naturel selection taken in the narrow sense of the "survivel of the fittest."

Sexual Selection and the Nesting of Birds.<*Auk*, II, pp. 129–139, April, 1885.

In reference to Wallece's "Theory of Birds' Nests" (*Intellectual Observer*, July, 1867), and Dixon's "On the Protective Colour of Eggs" (in Seebohm's Hist. Brit. Birds, Introd., pp. x–xxxvii).

George F. Becker

NATIONAL ACADEMY OF SCIENCES

Volume XXI
SECOND MEMOIR

BIOGRAPHICAL MEMOIR GEORGE FERDINAND BECKER

1847-1919

BY

GEORGE P. MERRILL

Presented to the Academy at the Annual Meeting, 1924

GEORGE FERDINAND BECKER

By George P. Merrill

Dr. George F. Becker was born in New York City, January 5, 1847. His father was Alexander Christian Becker, of a Danish family, settled in Archangel, Russia, where it is stated the head of the house (Samuel Becker) held for a time the office of Danish consul, holding his commission direct from the King of Denmark. Samuel Becker was a man of considerable wealth and given to lavish entertaining, but lost his property through absorption in science, particularly the new chemistry. On account of these financial troubles, Alexander Becker (the father of George F.) came to the United States and settled in New York. His first venture was in mercantile life, but finding this distasteful he studied medicine and entered upon the practice of his profession, but died when the subject of this sketch was 2 years of age.

"My mother's maiden name" writes Doctor Becker, "was Sarah Cary Tuckerman, a daughter of the Rev. Joseph Tuckerman known in Boston as a philanthropist and the first Minister-at-Large. He was a graduate of Harvard and an Overseer, and intimate friend of William Ellory Channing, Joseph Story and other prominent men of the day. The only scientific Tuckerman was my mother's first cousin, Edward, a member of this Academy [i. e., the National Academy of Sciences].

"Cambridge was selected as a residence by my parents with a view to the education of their two children Alexander Rudolph and myself. My mother's Cambridge friends were for the most part in the University set. Benjamin A. Gould was a very constant visitor. C. C. Fulton and Louis Agassiz married Mary Cary and Elizabeth Cary, first cousins of my mother. Charles Henry Davis, the founder of the Nautical Almanach and later Rear Admiral, was a frequent and welcome caller. Benjamin Peirce and Jeffries Wyman were likewise good neighbors. We knew Asa Gray more slightly but saw little of the Bonds. Longfellow, Lowell, Richard H. Dana, Holmes and Wolcott Gibbs and Charles Eliot Norton were valued acquaintances.

"Most of the scientific men took some little interest in me as a child but I owe most to Agassiz, Peirce and Wyman who seemed to like to encourage me in scientific curiosity.

"I was flying my kite in a field one day about 1856, when Peirce joined me to ask if I knew why it stayed up in the air. Of course I had no definite idea and he was at much pains to explain as much as I could understand of a distinctly difficult subject; he then and there excited an interest in my mind which has never yet wavered."

There is abundant evidence in the correspondence to which the present writer has had access that between mother and son there was early developed a strong bond of affection which was to continue throughout their entire lives.

It is told of George that when a boy of 4 years he quietly listened to his widowed mother all the evening as she read the history of the "Commonwealth of Massachusetts." After he had been tucked into bed, and sleep had come to him, his lonely mother was overcome with memories. Going to the child's bed, she knelt down; the little fellow must have felt her presence, for he reached out and touched her. "Oh, my child, who do you love?" said the mother. The sleepy little childish voice answered: "Anyone who will do good to the Commonwealth of Massachusetts."

In early youth Doctor Becker showed a decided taste for natural history and was of a studious and quiet nature, caring little for the talk and games of other boys.

"Games and sports interested me but little," he wrote, "and my mother had often to send me to the Delta, now occupied by Memorial Hall, to play with the other boys. I could play rounders and pre-Rugbian football decently well but I preferred gymnastics to these games because I could do them alone. The boys' talk did not interest me and I had sense enough to make no reference to natural history in their presence.

"Then (as ever since) the only society I cared for was such as I found mentally stimulating and the only pleasures for me were those involving some mental exertion. Chess I liked, but cards I found dull, the element of chance spoiling the fun.

"When I could not be at the Museum or in the woods and marshes I liked best to spend my time on the lounge in my mother's library with Audubon's text or Nuttall or Carpenter's animal physiology in my hands. I found them very stimulating to the imagination, as much so as Scott's novels, though these too I enjoyed."

Concerning his boyhood he also wrote:

It was a delight to me to roam alone about the woods especially those near Fresh Pond and the marshes not far away. Not a pool or a grove lacked inhabitants I knew something about. In fact, I believe I could recognize each New England bird and I was familiar with many of the reptiles. I knew not only the frogs and the toads but the spawn of most of the species and having heard so much of embryology from Agassiz it was delightful to follow the development of the translucent eggs from day to day, as well as to watch the fascinating transformation of the tadpoles.[1] I had a small shot gun which I generally carried on these expeditions but I rarely fired it excepting when some bird or rodent seemed to display characters to make him a candidate for admission to the museum. Sometimes, however, I would practice a bit at a mark to keep my hand in. One such day I was on the way home when a gentleman overtook me and seeing a museum alcoholic collecting case in my hands asked me what I had. I was tired and I didn't recognize my interlocutor so I merely replied the Latin names of my specimens, meaning to choke him off! But with these he was evidently familiar. He proceeded to ask me some questions I could answer, others much beyond me and then launched out into a most delightful 15 minutes disquisition suitable to my small capacity. I was ashamed, charmed and instructed as well I might be for it was no less than Jeffries Wyman who had thus honoured a little boy. I never met him afterwards without carrying away ideas and an improved sense of method.

Becker was fitted for college in Latin and Greek under the tutelage of Prof. Wm. B. Atkinson and was admitted to Harvard in 1864, only "with several partial conditions which did not trouble me. I was glad to be an undergraduate and was confident I could keep up with my class. I had no trouble in doing so excepting when colds and sore throats kept me out of the class room as they sometimes did for weeks together. Indeed, I had plenty of time for desultory study, looking up anything I did not understand and always finding that it led to something else I did not comprehend, which needed new search."

Concerning Agassiz and his teachings he wrote:

Very clearly impressed upon my memory is a passage from one of his lectures delivered about this time [probably 1859]. Substantially it is as follows though I cannot of course guarantee its literal accuracy. "As investigators we are necessarily open to new ideas whether they arise in our own minds or those of our colleagues. Yet it must not be supposed that this habit is sufficient to assure our receptivity without making a conscious effort to take broad views. Fundamental discoveries occur only at comparatively long intervals and they require a mental readjustment which is difficult, especially after youth is passed. Thus when Harvey made his great discovery of the cellular structure of tissues, the scientific life of all the anatomists over forty years old was arrested. Some accepted the truth but confessed themselves unable to revolutionize the conceptions which had been the basis of their work, others vainly contended against the truth."

Good precepts these but difficult to follow as the dear man himself soon exemplified. Not long afterwards the *Origin of Species* appeared and I well remember his next lecture to his class. He was almost overcome with pain, indignation and horror. This hypothesis, he told us, is a revival in a more insidious form of the thoroughly exploded heresy of Lamark. He could not express to us the pain it gave him to see the great learning and ability of Charles Darwin applied to sophistical and almost blasphemous reasoning. The book could not but do vast harm and wrong though it is, let no one suppose it easy to refute, unsound. On the contrary the vast information and acute though misdirected arguments make a reply most difficult. The safest way would be not to read it at all. Fancy such an injunction addressed to that set of youths. After the lecture they adjourned to Putnam's room and I think I must have been invited to go along too, at any rate I went without receiving any rebuff. Of course it was an indignation meeting though nothing abusive was said of the master. It was a great day, a critical epoch in the lives of most of the young men present.

About the middle of his college course it would seem his interest in natural history had become considerably cooled, and—

I was somewhat adrift when a single lecture on mathematics opened my eyes to the fact that this is a science of great principles and ideas, not a mere jumble of tiresome computations and unrelated Chinese puzzles. That was a happy discovery to me. Chemistry too, attracted me though the instructions we received were distinctly

[1] Drawings, essays, and even poems on these various subjects, found among his papers and evidently prepared at about this time, are of quite exceptional merit and show a trend of mind little dreamed of by those who knew him only in his later years.

bad. In the senior year I took analytical and celestial mechanics under Benj. Peirce, with a single other classmate. Peirce went too fast but he was inspiring and we were thus brought into contact with a really original thinker. I came out at graduation well up in my class—thanks to high marks in mathematics and English.

At the close of that year (i. e., senior) my mother offered to let me study in Germany after graduating at Harvard. I gratefully and eagerly accepted. In the winter of 1867–8 I had the benefit of intimate association with Dr. Herman Hagen the well known entomologist. He had just come from Koenigsburg accompanied by his wife to take a position in the Museum, and Agassiz, knowing of our plans to spend some years in Germany, persuaded my mother to take them in for the winter. They had the task of wintering themselves in a new country while I had more than plenty to learn from a German Savant. Hagen and I soon had a *lingua franka* in which we could converse and we spent many of our evenings in high debate upon an endless variety of topics. He was a somewhat eccentric but highminded scholar of enormous learning and the best standards. Dilettantism and philistinism were to him anathema. His was just the companionship I needed and the opportunity was not thrown away. Though he was no better scholar than other of the men mentioned above, he had a greater effect upon me than they did because our intercourse was so constant and so familiar. My debt to Hagen is a great one. But for his instructions I should have been ill prepared to grasp the spirit of the German universities. By reason of his coaching I found myself at home in a very short time in German academic circles.

That Becker was a diligent student while in Germany is shown by the following translation of a certificate from the well-known chemist Bunsen:

TRANSLATION OF PROF. BUNSEN'S LETTER

It gives me especial pleasure to certify that Mr. G. F. Becker, Dr. Phil., from America has taken part in the practical chemical exercises in my laboratory with extraordinary zeal from Oct. 1868 to Nov. 1869; and has not only acquired a thorough knowledge of analytical chemistry, but also especial skill in the exercises of analytic work.

Inasmuch as I count Mr. Becker among my most active and most gifted pupils, I cannot suppress the wish that he may right soon find a sphere of action corresponding to his wishes and to his remarkable abilities.

Pride in his work and a determination to master its practical details led Becker to undertake while in Germany the somewhat unusual task of a puddler in the royal iron works. With what success he met is shown by the following:

CERTIFICATE AS PRACTICAL PUDDLER

It is herewith certified, that G. F. Becker of Boston has been at work in these works since Sept. 16 as puddler, and during this time has performed the severe labours of this branch of industry with extraordinary industry and most praiseworthy endurance.

The trial charges, which Mr. Becker made, before leaving, without assistance, gave a most excellent result, and furnished the proof of the practical thoroughness as well as of the intelligent grasp, of the whole puddling process on the part of the aforenamed gentleman.

He also took means to instruct himself with zeal and success in furnace construction, as far as the customs here present anything peculiar or unusual—put a helping hand in the building, repair and preservation of the furnaces, and made himself acquainted with the whole working of the shop.

Konigshütte, Nov. 1, 1870.

The Director,
(Signed) Richter.

During the Franco-Prussian war, Doctor Becker served as a correspondent for the New York Herald, being attached to the Crown Prince's staff, but says of himself that he was not very successful. He was present at the battle of Woerth and the siege of Strassburg, and was once captured by the French and held prisoner for a few hours. Even at this time he foresaw the coming of the great war, though anticipating its date. He wrote, "I bet a bottle of the best champagne that Germany will be at war with Europe within 10 years."

Returning to America, Doctor Becker was employed during 1872–73 as a construction engineer by steel works at Joliet, Ill. While here he invented, but did not patent, an improved puddling process which is stated to have been in use both in Joliet and Youngstown, Ohio, up to within the last five years (i. e., about 1918). Shortly after this he went to California, partly, it is said, on account of his health, though it seems probable that the field seemed most inviting to one of his profession. Here during 1875–1893 he was instructor in mining and metallurgy in the State university. Among his pupils was the now well-known mining engineer, John Hays Hammond. While here he fell under the influence of Clarence King, who was then engaged upon the survey of the fortieth parallel. In 1879, when King became director of the consolidated geological surveys, Becker was among the first to receive appointment, and it is here that his scientific career as a physicist and geologist may well be said to have begun.

20154°—26——3

Soon after his appointment Doctor Becker made a reconnaissance of the San Francisco, Utah, Eureka, Nevada, and Bodin, Calif., mining districts, with a view to laying out the work rather than of completing the examination of any single district. Owing to a change in the plans of the director, Becker personally undertook the study of the mines of the region, though at a later date (1884) those of Eureka were examined and reported upon by Mr. J. S. Curtis, acting as his assistant.

Early in 1880 Becker was instructed by the director of the survey to take, in addition to his survey duties, the office of a special agent of the Tenth Census (this without extra remuneration) and to assist in the compilation of statistics and technical information as to the precious metal industries of the country. The district assigned included Idaho, Utah, Arizona, and the country to the west of them. To S. F. Emmons was assigned the area of the Rocky Mountains. This investigation extended throughout the census year.

The results of this work were published as Volume XIII of the census reports, entitled "Statistics and Technology of the Precious Metals," by Geo. F. Becker and S. F. Emmons. It contained chapters on the geology of the Western States and Territories, statistics of production, and a very large amount of information on most of the important features of hydraulic mining, deep mining, quartz milling, etc., with brief but adequate discussions. This report, so far as statistics of production are concerned, is now out of date, but the technical information for the most part retains its value.

Becker here took occasion to discuss the four ore belts of the Pacific slopes and thought to show that each coincides with a well-marked zone along which relative upheaval has occurred. This fact by itself he thought demonstrated that a relation existed between the great movements which interest geologists and the accumulation of ore deposits which form the basis of the mining industry.

In March of 1880 Becker had been also instructed on the part of the survey to make a reexamination of the Comstock lode. Messrs. F. von Richtofen, Clarence King, and J. A. Church had each written reports upon the extraordinary deposit, but many features of the occurrence were not satisfactorily elucidated. The importance of the occurrence and the fine opportunity for study seemed to justify a fresh and more elaborate investigation.

The work, which was carried on at the same time with the census investigation, was completed in two years, the report forming Monograph III (1882) of the survey series. Besides full descriptions of the ore bodies and their occurrence, it contained a discussion of the rocks, showing that the lava propylite, supposed to be the chief rock of the district, consisted in reality of better-known eruptives in a peculiar state of decomposition. This, at the time, was an important petrographic discovery, and particularly so in view of the work of Zirkel on the rocks of the fortieth parallel survey. Examination of supposed propylites from all the other regions from which geologists had collected them in the United States showed that they were all of a character similar to that of the Comstock. Trachyte was shown to be absent, contrary to previously expressed opinions. The heat of the lode was shown to be due to hot water, rising along the vein from great depths, and deriving its heat from a source not less than 5 miles from the surface. The complex "distributed faults" or "step faults" of the locality were elucidated, and it was shown on mechanical principles how such were formed. Suggestions were given as to the most probable position of undetected ore bodies.

While many of the ideas put forward in this report have been the subject of controversy, it may safely be said to mark a new era in geological investigations in America. No previous investigations, by any survey, had been undertaken on so broad a basis. No known and available means of investigation were left untouched and untried. That Becker fully realized the possibilities of the opportunity offered is shown by the selection of assistants, among whom were the physicists, Carl Barus and William Hallock, and the then young and rising petrologist, J. P. Iddings. The appearance of this monograph placed Becker in the front rank among American geological investigators.

Doctor Becker's next field of investigation comprised, under the administration of J. W. Powell,[2] a study of the quicksilver deposits of the Pacific coast. This proved a somewhat prolonged investigation and for its satisfactory completion necessitated a study of the deposits of similar nature in Italy and Spain. Though under governmental authority, he was obliged to carry on this portion of the work at his own expense. The results of these studies appeared in 1888, as Monograph XIII of the survey series, under the caption of "Geology of the Quicksilver Deposits of the Pacific Slope."

In this report it was shown that all of the ores were deposited in preexisting openings and not by substitution. Where these openings were fissures the deposits were veins. Contrary to the generally received belief, veins of quicksilver ores, it was shown, are very common and were filled from solutions, not by sublimation, the material being deposited as black amorphous sulphide and subsequently converted into cinnabar. In connection with the deposits a new class of veins was proposed, called "chambered veins." It was shown that not only cinnabar but gold and the sulphides of iron, copper, and antimony and zinc dissolve in solutions of sodium carbonate charged with hydrogen sulphide, such as occur in great abundance as spring waters in volcanic regions. The report contained a digest of the descriptions of all the important or interesting quicksilver ore deposits of the world.

In the same volume it was shown for the first time that the Cretaceous and Tertiary formations in this part of California were continuous in life and sedimentation. Attention was also called to a great and hitherto unnoticed upheaval in the Coast Ranges, at the beginning of the Cretaceous period. Becker thought to show further that in some cases serpentine resulted from the action of mineralized solutions on sandstones.

At the close of the census investigation, Mr. J. S. Curtis, who had been one of Becker's assistants in that inquiry, joined the survey and was directed by him to study the Eureka mines. Mr. Curtis's volume, Silver Lead Deposits of Eureka, Nevada, Monograph VII, 1884, of the survey series, was well received by those interested in mining.

The above completes the list of Doctor Becker's monographic studies, but by no means closes or limits the period of his activity. In fact, they were but the beginning. While finishing the writing and attending to the publication of the report on quicksilver, he elaborated a plan to be carried out by his assistants, Messrs. H. W. Turner and W. Lindgren. It was intended that this investigation should cover the west slope of the Sierras for the entire length marked by frequent or important gold mines. The plan included four memoirs on the lithology, descriptive geology, mining geology, and systematic geology of the region. He proposed that the mapping and the descriptive memoir should be done by assistants under his supervision, while his own attention was to be given to the investigation of the questions arising in the work, such as the nature and origin of the structure, the character and effect of the glaciation, the origin and history of the ore deposits, the relation of the eruptive rocks to one another, and the like.

This investigation was never completed as planned. About 7,600 square miles were mapped and much preliminary work was accomplished in areas of which the topographic basis has been only lately completed.

Commenting upon this, Doctor Becker wrote:

> It might seem more expedient to have begun this work with the ore deposits, but a little consideration shows that this would have involved loss of time and labour. If one were to begin with the gold bearing gravels, questions which would arise immediately are: What is the source of the pebbles? What are the relation of the channels to one another and to the present drainage system? What is the age of the deposits? These questions can be solved only by a study of the general geology of the country. If one were to begin with the quartz veins more numerous problems of a similar character would compel a return to a preliminary study of the geological history of the region.

Becker's personal studies of the gold belt led to seemingly definite solutions of a number of questions. Some of his conclusions were as follows: There is strong evidence of a post-

[2] King resigned on Mar. 11, 1881, and Powell was appointed to his position three days later. Doctor Becker's position on the survey does not seem to have been affected by the change.

Triassic upheavel in the Sierra probably identical with a disturbance which has been recognized in British Columbia. Two sets of earlier Cretaceous beds (divisions of the Shasta group) are contemporaneous or continuous. Authentic information not previously published shows that California was inhabited by men in the Neolithic stage of development, before the main glaciation of the Sierra. The glacial period of the Sierra probably began and ended much later than that of northeastern America. The deep canyons of the modern rivers of the Sierra were due to the protecting action of the glaciers on the higher part of the range.

A large portion of the Sierra, he thought, was affected by systematically disposed fissures or joints. A study of their peculiarities showed to him that this network of divisions was produced by a pressure acting on the range downward from the south-southwest. This pressure, he thought, could be accounted for by the weight of the sediments in the great valley of California, provided that the earth is a solid, highly viscous mass, but not if the interior is fluid.

The fissure systems, he argued, controlled to a large extent the emission of eruptive rocks. They affected the modeling of the country and also indirectly explained the formation of the canyons, among them the Yosemite Valley and the great domes of that region.

During the winter of 1892 Doctor Becker made his first systematic studies of the deforming effects of great pressures. This he regarded as essential to a comprehension of the structure (announced later) and showing that relative elevation must attend the formation of slate. The theory, to him, accounted for the distribution and spacing of fissures or cracks when the action is slow. In the same connection he accounted for the columnar structure of many lavas and gave a simple proof of the fact that the pebbles in auriferous river channels and other watercourses "shingle upstream."

A reconnaissance of the gold fields of southern Alaska by Becker in 1895 afforded incidentally material for an interesting theoretical discussion of vulcanism and the shape of volcanic cones, in which it was shown that such tend to approach definite geometrical form almost exactly coinciding with that of Fujisan, Japan, but that steeper shapes will not form on a large scale by ejection from a central vent.

In 1896, under the auspices of an English company, Doctor Becker visited the Wittwaterstrand of South Africa for the purpose of studying the gold fields. Aside from whatever report he may have made to the company, his observations found their way into print in the Eighteenth Annual Report of the United States Geological Survey (1896–97) and the Zeitschrift für Geologie Praktique, besides less technical accounts in the National Geographic Magazine and the London Economist. In all these publications Doctor Becker held the ground of the marine origin of the gold-bearing gravels and the alluvial origin of the gold itself, in opposition to De Launay and others, who thought it precipitated from a saturated solution of gold and pyrite in sea water. He noted that "in the pre-Tertiary rocks only those gravels remain which were protected by superjacent beds and allowed to indurate. River gravels, as such, could escape dispersion only when during subsidence they were immediately covered by fresh deposits, without undergoing any notable wave action." Hence the extreme rarity of pre-Tertiary river gravels. Such an origin—i. e., as marine, rather than as river gravels—he felt furnished a strong reason for the belief of their prolonged productiveness.

It was while employed in this work that Doctor Becker became conversant in some detail with matters relating to the Jameson raid and the Boer war and led to the preparation of an article on "The revolt of the Uitlanders," published in the National Geographic Magazine of that year. In this he set forth in a dispassionate and impartial way the prevailing conditions as they appeared to an outsider, and through his personal influence with President Krueger he is said to have been instrumental in bringing about an amicable settlement of certain difficulties that threatened to lead to international complications.

The most original, outstanding, and valuable of Doctor Becker's work was not, however, along the lines of descriptive geology. His interests lay largely in the more abstruse chemico-physical problems and concerning which he had almost from the start taken advanced grounds, not merely in relation to the problems to be solved, but, as well, to the methods of their solution. Along these lines he was a pioneer, and it was not too much to claim that the present

Geophysical Laboratory is the outgrowth of his work more than that of any one man.[3] His published shorter papers showed an ever-increasing tendency to delve into physical and chemical problems and theories and to devise methods for their solution. This is evident in his Finite Homogeneous Strain, Flow, and Rupture of Rocks (1893), Rock Differentiation (1896–97), Experiments on Schistosity and Slaty Cleavage (1904), Torsional Theory of Joints (1913), and numerous other papers.

Concerning the paper of 1893 Day remarks:

In this we recognize a splendid attempt to define and formulate in precise terms, some of the relations in the science of rock mechanics. This was a magnificent task of pioneer quality and of extraordinary difficulty, but was not immediately fruitful because clothed in somewhat abstruse form.

In his discussion of rock differentiation an attempt was made for the first time in America to apply physiochemical laws along the experimental lines of Van't Hoff and others. The conclusions reached can be made clear by quoting his own abstract.[4]

All known processes by which the segregation or differentiation of a fluid magma could take place involve molecular flow. This is demonstrably an excessively slow process excepting for distances not exceeding a few centimeters. Soret's method of segregation, even if it were not too slow, seems inapplicable because it involves a temperature unaccountably decreasing with depth. The normal variation of temperature, an increase with distance from the surface, would be fatal to such segregation. The least objectionable method of segregation would be the separation of a magma into immiscible fractions; but this seems to involve a superheated, very fluid magma, while the law of fusion and the distribution of phenocrysts in rocks indicate that magmas prior to eruption are not superheated to any considerable extent and are very viscous.

The homogeneity of vast subterranean masses called for by the hypothesis of differentiation is unproved and improbable. The differences between well-defined rock types are more probably due to original and persistent heterogeneity in the composition of the globe. Hypogeal fusion and eruption tend rather to mingling than to segregation, and transitional rock varieties are not improbably mere fortuitous mixtures of the diverse primitive relatively small masses of which the lithoid shell of the earth was built up.

This paper was subjected to a critical review, with only partial agreement by C. F. Tolman, in the Journal of Geology for May–June, 1897.

In the paper on schistosity and slaty cleavage published in 1896 and already referred to, Becker took issue with the general idea to the effect that a secondary cleavage may be induced under pressure, but argued that "deformation of a solid, homogeneous, viscous, isotropic, not infinitely brittle, mass will develop structure in it on not less than one surface nor on more than four surfaces simultaneously." This he thought to show both mathematically and by experiment. In summing up his results he stated:

In view of the evidence merely outlined above, it appears to me utterly impossible to deny that solid flow does as a matter of fact induce a true cleavage which is parallel to the lines of relative tangential motion or gliding, this cleavage not necessarily being accompanied by any actual ruptures however microscopic.

Again in his paper on Current Theories of Slaty Cleavage (1907), which was largely controversial, Becker referred to the prevalent ideas as found in the literature, and then he stated his own views as follows:

Like Tyndall and Daubrée, I consider a parallel arrangement of flattened grains unessential to cleavage. Rupture takes place on planes of maximum slide or maximum tangential strain. Rupture is a gradual process and cohesion is impaired through flow before it is destroyed. Impaired cohesion in my theory is cleavage. Cleavage develops most perfectly when the stress tending to produce it is persistent in direction, because viscous resistance is then small. In a rotational strain there are two sets of mathematical planes on which maximum slide takes place and both sets are parallel to the axis of rotation. They make with the greatest axis of the strain ellipsoid angles given by

$$\tan = \frac{B}{(ABC)\ 1/3}$$

A being the greatest axis, B the least and C the axis of rotation. The planes of maximum slide contain the circular sections of the ellipsoid only in a limiting case. During the progress of strain these mathematical planes sweep through wedges of the mass, but the two sets of planes sweep at different rates, one set having a relative angular velocity from, say, 20 to an infinite number of times as great as the other. On the planes which sweep rapidly viscosity reinforces rigidity, there is no time for considerable flow to take place, and unless actual rupture

[3] See his Project for a Geophysical Laboratory and construction of a geophysical laboratory. Year Book, Carnegie Institution, 1902 and 1904.
[4] Amer. Jour. Sci., vol. III, 1897, p. 40.

occurs, so that joints form, the effect will be small. On the other set of planes viscosity is small, the mass has time to yield by flow, cohesion is weakened and cleavage results. In a word, the theory is that slaty cleavage is due to solid flow attendant upon rotational strains. So much of the energy of the system as is not potentialized is dissipated on the plane of maximum slide, and this may or may not lead to the alteration of mineral constituents, e. g., the transformation of feldspar into biotite.

In his paper on the Age of the Earth (1910), Becker pointed out the probable errors in the methods previously employed, with particular reference to the works of Kelvin, Darwin, Joly, and F. W. Clarke. He thought also to show that "radioactive minerals cannot have the great age attributed to them. Only something like a tenth of the heat emitted by the earth can be ascribed to radioactivity plus all other exothermic chemical transformations, the remaining nine-tenths of the heat being due to compression." Barrell, in a more recent paper (1917), subjected this conclusion to severe criticism, but as to which of the two credit is to be given for nearest approach to actual facts it is as yet impossible to say. Although not so shown in his correspondence, Doctor Becker was greatly interested in Chamberlin's planetessimal hypothesis, though by no means in agreement therewith.

In accordance with an arrangement made with the War Department, Doctor Becker, under orders of July 8, 1898, visited the Philippines for the purpose of investigating and reporting on the mineral resources of these islands. He sailed from San Francisco on the transport *Pueblo* July 15, under General Otis. After reaching Manila, he devoted some time to the preparation of a brief paper entitled "Memorandum on the Mineral Resources of the Philippine Islands," compiled from various unpublished records and published memoirs available in Manila, and from verbal information furnished by mining men, capitalists, and others. This memorandum was published in Part VI of the Nineteenth Annual Report of the United States Geological Survey.

In September of that year, Doctor Becker spent some days in field work on the island of Corregidor and about Mariveles. On his return to Manila he prepared, at General Otis's request, a memorandum on the agricultural resources of the archipelago. Shortly afterwards, finding it impracticable on account of the native rebellion to pursue further his geologic investigations, he attached himself to the Bureau of Military Information, Eighth Army Corps, under Major Bell. In this position he rendered valuable service, translating from the Manila newspapers articles of importance or interest to the Government and the military authorities, and endeavoring to enlighten public opinion in the islands by published articles correcting Spanish misrepresentations and setting forth the real conditions in the United States so far as those conditions were likely to become of importance to the Philippines.

Doctor Becker's own account of his services, as given in a report to Major Bell, are worthy of reproduction in full.

MANILA, P. I., *March 1, 1899.*

SIR: Pursuant to your verbal request, I submit the following report of work done by me in connection with the Information Bureau of the 8th Army Corps, during the three months just passed, viz: December, January and February.

I made voluntary tender of my services to you at the beginning of December, because the military situation made geological field work impracticable and I was unwilling to occupy the position of United States Geologist, in Charge, without rendering any services to the Government.

The first duties assigned me in the Bureau under your direction were to keep watch on the Philippine newspaper, "La Independencia," controlled by Mr. Antinio Luna, and to make translation of articles which seemed of importance or interest to the Military Governor. Another similar newspaper, the "Republica Filipina," controlled by Mr. Paterno, was afterwards included in this commission, and a variety of written and printed documents were submitted to me for translation. Your files contain copies of these translations. The following is a list of the more important of them. "America and Ourselves" a leader in the Independencia of Dec. 2, 1898, "Let us Wait" a leader in the Independencia of Dec. 3rd. "Philippino Yearnings for Spain" being extracts from the Independencia of Dec. 10th, threatening extracts from the Independencia of Dec. 19th. Passages from Republica Filipina of Dec. 25th complaining of the delay by the United States in announcing its policy. "Let there be Confidence" a conciliatory leader in the Independencia of Dec. 26th. Interview with Malolos Cabinet, Buencamino spokesman (a letter to Mr. Rickards, dated December 27th).

Primo de Rivera on the Paterno negotiation, (extracts from a memorial of the ex-captain General to the Spanish Senate in August last with some explanations and comments. Handed in December 31.) Primo de Rivera on ecclesiastical reform. (This from the same memorial as the last. Comments were added. Handed

in Jan. 1, 1899.) Letter of Buencamino to Republica Filipina, January 1, being a reply to a letter of mine. Manifesto by Aguinaldo January 5th. Second Manifesto by Auginaldo January 5th. "Aguinaldo pleading with his brother Philippinos," a rare pamphlet by Emilio Auginaldo, issued in December 1898 and immediately withdrawn. The translation is accompanied by an analysis. After January 4th, the Philippino newspapers removed from Manila and became openly hostile to the United States. It then ceased to be a matter of importance to keep track of their sentiments or expressions.

In December 1898 it appeared to be desirable to try influencing public opinion among the Natives by published statements correcting Spanish misrepresentations and setting forth soberly the real conditions in the United States so far as they are likely to become important to the Philippinos. In order to give such explanations more weight, it was expedient that they should be signed by a responsible person, and, if possible, by someone not in the military service. As I fulfilled these conditions, the task was assigned to me and several articles were written. All of them were carefully scrutinized by the Chief of the Bureau of Information, and by him submitted to higher authority, but I alone assumed responsibility for them to the public, in order that the Military Government might not be committed in any way by these utterances. Nothing was added by my superiors to these essays, but some sentences were struck out as impolitic. The articles were translated into intelligible Spanish under my supervision, and were published both in that language in the Philippino papers and in English in the Manila Times. The following thus appeared: "The Future of the Philippines" printed in Independencia, December 10, and replied to in the same issue. This appeared in the Manila Times on December 7th. "Treatment of North American Indians," Republica and Times Dec. 17. "Territorial Government in the United States," Republica and Times of December 23. "Free Education in the United States," Independencia of Dec. 24, and Times of Dec. 27th. Letter to the Republica on the delay by the United States in anuouncing its policy, Dec. 28. A further paper on "Religious American and the Catholic" was prepared, and the Republica promised to print it, but failed to do so. At the suggestion of a General Officer and with your consent, I also wrote an unsigned editorial for the Manila Times entitled "An Important Step." It appeared on Tuesday, Feb. 21.

During the month of January, at your instance, I took up the matter of the reestablishment of the Cedula Personal, gathering arguments for and against its reestablishment on any basis and opinions concerning the most expedient fees for such a personal certificate. It was found that the inhabitants of Manila wished for a Cedula and (although there was some opposition on this point) that it would be best to make only one class, putting the fee very low. I wrote an argument on the general question and directed especially to the desirability of making the Cedula compulsory on all men, in Manila, excepting U. S. soldiers, between the ages of 18 and 60, leaving it optional for women. General Otis was at first disinclined to the compulsory feature but yielded to my representations. I have the satisfaction of believing that the compulsory cedula will be an important aid in the preservation of order under American control in this Archipeligo.

I also undertook an examination of the matter of licenses which, however, was interrupted by the outbreak of the Insurrection.

In addition to the matters detailed above, many others have fallen to my share which are not important enough for special mention, such as deciphering telegrams, procuring secret agents, examining into rumours, collecting information, seeing to the printing and posting of the Military Governor's proclamation and making myself generally useful in the Bureau so far as my lack of military education permitted. You have also done me the honor to consult me on most of the matters in which you have been engaged.

When the Insurrection broke out it became impossible for me to sever my connection with the Bureau without loss of self respect. Inclination drew me in the same direction as loyalty and I have accompanied you on most of your strictly military duties as well as upon quests for information. Without specific instructions I have understood that, in the field as in the office I was to be constantly on hand, in readiness to undertake any message or commission you might see fit to entrust to me, and to render without orders any service within my capacities which circumstances clearly called for, if only by example. It has not always been easy to draw the line between officiousness and negligence, but you have been kind enough to give me reason to believe that I have not erred grossly in either direction. Your own official reports more than sufficiently cover such services as I have found an opportunity of performing at the front.

I cannot close this report without referring to the pleasure it has afforded me to assist, in a variety of extremely interesting matters and in some thrilling situations, an officer so ingenuously and intelligently devoted to his duty, who is as intent on correcting abuses and ameliorating the lot of the wretched as he is strenuous in the quest for military information and fearlessly aggressive on the field of battle.

How well his services were appreciated is shown by the following extract, re George F. Becker, from official report of Maj. J. F. Bell, in charge of the Office of Military Information of the Department of the Pacific, to Maj. Gen. Arthur MacArthur, commanding Second Division, Eighth Army Corps, under date of February 11, 1899, on the fight at Caloocan of February 10:

I have reserved for the end of this letter, meution of the exceedingly gallant and courageous conduct of Professor George F. Becker, U. S. Geologist, because in accordance with his idea of his duty he insisted on accompanying me into this fight and remained with the Company, much of the time mounted, throughout the entire

engagement. He was as cool and collected as if he were pursuing geological investigations in his study, encouraged the men behind whom he was standing and rendered other valuable services which required him to pass mounted immediately in rear of the entire line. I am sorry that, not being a soldier, he cannot receive the reward which his courage and gallantry has entitled him to.

The view he took of his duty referred to above, arose from the fact that for sometime before this war he volunteered to assist me in the Information Bureau and the instincts of a courageous gentleman have prevented his abandonment of his self imposed task of following me wherever I go now that the expected war with the Insurgents has come about and sometimes calls me, in the line of my duty, into dangerous situations.

In justice to him however I should add that long prior to any certainty of hostilities he made me promise that if hostilities did occur I would permit him to accompany me wherever my duty called me. He has accompanied me, pursuant to his own desire, on every reconnaissance I have made and frequently against my judgment as to what was best for him.

Very respectfully,

Sgd. J. F. BELL,
Major of Engineers.

After the outbreak of hostilities, upon invitation of General McArthur, to whose staff Major Bell was transferred, Doctor Becker accompanied the latter to the front and participated in a number of military reconnaissances and engagements, rendering service that has been favorably reported to the War Department.

In May he made a journey to the island of Negros and endeavored to examine the deposits of tertiary lignite there, but the hostility of the natives prevented extended investigations.

While here he met with an adventure which gave him reputation quite unsought and along lines little expected. It seemed that he wished to examine a coal deposit near San Carlos, and for safety's sake was given an escort of a noncommissioned officer and 16 men. Notwithstanding this they were attacked by a considerable body of natives. The rest of the story is told in a clipping from the newspaper Freedom of September 29 of that year:

The men saw three lines of skirmishers surrounding the plantation. Guns were grabbed, orders given, and they were soon out and ready to meet the enemy; now here is where the "Old Professor" as the boys called him [Becker] shone. Emerging from the owner's house in his shirt sleeves, with a little popgun, a 32-caliber revolver, which he had borrowed from one of the boys, he took his position as commander on the right, accompanied by four comrades.

There was a hot fight for a time. Finally the rebels closed in on the right. The five men protecting that position awaited the charge calmly. When the insurrectos were within about 10 yards a volley rang out, and four blacks dropped. But one kept coming, and straight for the professor. Up went his little popgun and never a tremor in his arm. He fired and missed. The black was now almost upon him. Again he fired and this time he caught his man in the right arm shattering it, and causing him to drop the murderous looking bolo. A volley rang out and the black dropped at the professor's feet. When it was all over 12 dead insurrectos were counted.

"Well it's the first time I ever fired a pistol at a human being, boys, but I had to do it, and I did it." [5]

Incidental to this it should be stated that Doctor Becker was twice "cited" for bravery in the field during his stay in the islands.

On December 26, 1902, President Roosevelt called upon the National Academy of Sciences for a report on the desirability of instituting scientific explorations of the Philippine Islands and on the scope to such an undertaking, expressing his hope that such a plan might be adopted by Congress.

A committee of the academy, consisting of Messrs. G. F. Becker, W. G. Brewer, C. Hart Merriam, F. W. Putnam, and R. S. Woodward, representing Harvard, Yale, Columbia, the Department of Agriculture, and the Geological Survey, was appointed on January 14 following. Becker was elected secretary and under conditions usually prevailing may be assumed to have done his full share of the work.

The committee was unanimous in the opinion that scientific explorations of the Philippines were most desirable, both for the good of science and for the benefit of the inhabitants of the islands, and so reported on February 7, 1903.

On March 9 following, the President constituted a board of scientific surveys of the islands, consisting of Mr. C. D. Walcott, chairman, and Messrs. F. V. Coville, Barton Evermann,

[5] "A day or two since a half tipsy soldier called on Bell to speak about the reconnaissance at Gaudalupe of February 20, and 'to shake the hand of a brave man.' Then he added, 'Say, Major, who was that old man [Doctor Becker] along with you?' and when he had heard, remarked 'Well he was a crackerjack, too.' "

W. H. Holmes, C. H. Merriam, Gifford Pinchot, and O. H. Tittmann, selected from the Government bureaus to consider the cost and other features of the plan proposed by the National Academy. The board made a report in harmony with the plan of the academy. It estimated the total expense for the first of the 10 years (including an item of $250,000 for the purchase of three small vessels) at $761,950. It also submitted the draft of a bill to provide for these surveys and memoranda as to their administrative conduct.

For reasons which it is not necessary here to discuss, even were they known, these recommendations were never carried out.

Doctor Becker was also a member of the committee of the National Academy of Sciences appointed in 1915 by President Wilson to consider and report upon the possibility of controlling the slides in the Panama Canal, which then threatened seriously to interfere with its usefulness, but was unable on account of ill health to visit the canal and participate in its deliberations. He had, however, been over the ground in 1913 in company with Geologist D. F. MacDonald, and rendered important service in the preparation of the final report.

With all his close attention to details in matters of science, Doctor Becker was by no means oblivious to the duties of citizenship. This appears in his correspondence relative to the Philippines, already referred to, and in numerous letters I find in his files. While not obtrusive in his manner, it would seem that he was by no means diffident. Convinced of the soundness of his own opinion or views on any subject, he did not hesitate to make them known wherever he felt they might be useful. The following letters are of interest and self-explanatory:

Newbury, N. H., *Sept. 26, 1901.*

My Dear Becker: I thank you for your kind letter of the 21st and the speech you made at New York. Roosevelt's hard task would be easy if all men would give him the wise and reasonable consideration which you express in this speech.

As for myself, I can only thank you for what you say. Nothing can bring me back to where I stood last June. But I must "fight my course" being chained to the stake.

Yours faithfully,

(Signed) John Hay.

March 4, 1905.

Dear Mr. President: I do not feel able to allow this day to go by without expressing my congratulations on your past administration and cordial good wishes for that which begins to-day. You have justified the predictions which were made by some of your friends, including myself, at the time of Mr. McKinley's death, and have earned the confidence which the nation has lately expressed in you.

May nature continue to smile upon you and may the country continue to sustain you in raising the standard of national life.

Very respectfully, your obedient servant,

(Signed) George F. Becker

Nov. 15, 1912.

Dear Darwin: ——— ——— has informed me of your illness, bad news which at once recalled the details of many pleasant hours I owe to you.

Illness and suffering sometimes make a body lonely; and that is why I write to say how much I wish I could cheer you now as you cheered me when I lay ill at McKinney Hughes house. You have a host of willing friends who hope all good things for you and know that you have deserved them. May our wishes be efficacious.

Pray do not dream of answering this greeting or of asking any one to do so. I sympathize too keenly with Lady Darwin and your children to burden them with needless letters.

Most cordially yours,

(Signed) G. F. Becker.

So. Lee, *July 16, 1915.*

Doctor Otis Smith.

Dear Doctor Smith: I have read your Greek Φ B K address with pleasure and I think it ought to do good.

There is an argument for public spirit on the part of university men which I have never heard emphasized. Perhaps you might like to use it in some future address. University education is to a large extent gratuitous; for the undergraduates at Harvard or the Johns Hopkins do not pay fees covering more than a fraction of the expenses of their education. Men who seek or use their university training solely for their personal service are almstakers. Only by public service can educated men repay the debt they incur and thus fulfill the designs of the founders.

Cordially yours,

(Signed) George F. Becker.

A striking feature of Doctor Becker's career was his versatility, which he seems ever to have cultivated, rather than held in check. An interesting illustration of this is afforded in a paper (lecture) prepared by him in 1904, entitled: "How small an Army we need." The purport of the paper can not here be further elucidated than to state that a copy of the same being sent Brig. Gen. J. F. Bell, then at Fort Leavenworth, Kans., received the following indorsement:

INFANTRY AND CAVALRY SCHOOL AND STAFF COLLEGE,
OFFICE OF THE COMMANDANT,
Fort Leavenworth, Kansas, Sept. 17, 1904.

DR. GEORGE F. BECKER,
Washington, D. C.

MY DEAR DOCTOR: I have received the article entitled: "How small an Army we need," which you have kindly sent me for criticism. I not only have no criticism to make, but am astonished that a man who has never been a professional soldier could have written so soundly on the subject you have selected. I have submitted this article to many of my assistants on duty with this college, and it meets with the hearty concurrence of all of them. We find the matter of which it treats so clearly and cogently set forth that we would be greatly gratified to see it published in some form or periodical where it could reach the masses of our non-professional fellow citizens. You have not even made any technical error in the statements of fact or deductions. We only hope that there is a respectable percentage of our fellow countrymen who may be able to see this matter in the same light that you do.

Hoping you may continue your studies and efforts on behalf of the country in this line, believe me,

Truly and sincerely yours,

(Signed) J. F. BELL,
Brigadier-General, U. S. Army,
Commandant.

It is difficult to write of the purely personal side of Doctor Becker, since few of those who were associated with him and knew him at all intimately are now living. That he was of more than ordinary affectionate nature both as boy and man is evident from his correspondence; this is particularly conspicuous in his letters to his mother. Few but his most intimate friends could see in this seemingly unemotional man, absorbed in problems of science, one whose daily letters to wife and mother were concluded in language of the tenderest endearment.

"I owe him a debt," writes his one time assistant, H. W. Turner, "for his rigid requirements of exact notes on all geological matters . . . In camp we found Dr. Becker always a good sport and an interesting companion."

"In thought and manner," writes Doctor Day, "Dr. Becker was a true pioneer, absolutely fearless, impatient of limitations, quick to get at the heart of the problem, direct and vigorous in its prosecution, and with an untiring spirit even under the strain of protracted illness which clouded the closing years of his life." And again, "Like most pioneer thinkers, Dr. Becker was by necessity the master of several fields of scientific research. He possessed an excellent working knowledge of mathematics, physics, chemistry, and geology, and used all these with the greatest freedom and effectiveness throughout all his work. With the possible exception of Gilbert, there is no man of his time in the Washington geological work who possessed greater versatility in discussion or such breadth of view." And still again, in a personal note to the present writer he says: "I cherish Dr. Becker's memory as that of one of the finest men, one of the soundest scientists, and one of the best friends I ever knew."

Highly commendatory is the following extract from a letter written by General Bell to Mrs. Becker—the mother—under date of February 21, 1901:

Your son and I were strongly drawn toward each other, because we lived here together at a time of great distress, and we found our ideas accorded respecting the propriety and impropriety of matters in general. We easily became indignant at abuses committed by Americans and worked hard together for the credit of our land and nation. His strong conviction and determination was a support to me in many cases of doubt and uncertainty. His companionship on the battle-field was inspiring. Bravery is expected in a soldier. He gets no credit from having it, but great discredit if he has it not, but bravery is not expected from those whose business is other than fighting. They have nothing to gain and all to lose from being killed; whereas a soldier may gain undying fame by losing his life on the field of battle. Therefore, when a man takes his life in his own hands, as your son did, inspired solely by an interest in his nation, and the manly instinct antagonistic to cowardice, he is deserving of credit indeed. No bond of affection is so strong as that which is created by the sharing of mutual dangers. I think our satisfaction might have been more complete had we had your strong intellect and sympathy to aid us at a time when much hardship and suffering was visible on every side.

The most prominent characteristic of Doctor Becker, as viewed by the writer, was his persistent aggressive attitude toward geophysical problems and the establishment of a laboratory for the experimental work essential to their solution. This was the one dominant feature of his career and one which was ultimately crowned with success by the establishment of the Carnegie Geophysical Laboratory. That he was thoroughly in earnest in this is shown—if further evidence is needed—in his last will and testament, by which his entire residual estate is to pass to the Smithsonian Institution to be applied to "the advancement of geophysics."

His thoughtfulness and willingness to assist in matters covering a wide field was little realized by the majority of his acquaintances. It was at his suggestion that there was established in 1909, by the National Academy of Sciences, a medal to be conferred from time to time upon men "who can not be classed as eminent scientists, but who are eminent in the application of science to the public welfare." His article in the National Geographic Magazine, "Revolt of the Uitlanders," was a model of unprejudiced plain speaking and unquestionably did much toward clarifying the public mind on a subject concerning which it was at best poorly informed. His remark in that connection that "no man of ordinary virtue who does not identify himself with the country in which he lives, to whom that country is not a 'home' will use his official power . . . for the best interests of the community from which he longs to be gone" is worthy of repetition.

Becker took an ardent and decided stand in the Great War; and though there is found little on the subject in his correspondence, it is known that he was thoroughly American in thought and action.

Doctor Becker enjoyed a wide range of acquaintances both among the scientific fraternity in America and abroad and what is commonly spoken of as "society," particularly that of the higher circles of political life, and around his hospitable table there gathered not only members of the congressional delegations, but the Cabinet, Supreme Court, and foreign legations as well.

Doctor Becker received the degree of Ph.D. (summa cum lauda) from the University of Heidelberg in 1869, being, it is said, the first foreign student to attain this distinction, and was graduated with high honors from the Royal Academy of Mines of Berlin in 1871. He was an original fellow of the Geological Society of America and was president of the same in 1914. He was a member of the National Academy of Sciences, the Washington Academy of Sciences, the Geological Society of Washington, the American Institute of Mining Engineers, and an honorary member of the Geological Society of South Africa. With the exception of the two years 1892–1894, when the position was abolished for lack of appropriations, Doctor Becker held in the United States Geological Survey, for the entire period from 1880 until his death, the position of "geologist in charge."

He represented the Government in different geological congresses and in the Radioactivity Congress in Brussels of 1910.

Doctor Becker was thrice married. First to Sarah M. Barnes, from whom he was legally separated in 1879, and on June 17 of the same year to Alice Theodora Watson, who died early in the year following. On February 11, 1902, he was married to Florence Serpell Deakins, who survives him. During the later years of his life he suffered severely from asthma and its complications, but retained active interest in his work until the last. He died at his home in Washington, April 20, 1919, at the age of 72 years.

BIBLIOGRAPHY OF GEORGE FERDINAND BECKER

Compiled by Isabel P. Evans

1874

Definition of metallurgy: Min. and Sci. Press, vol. 29, p. 338, Nov. 28, 1874.

1875

Metallurgical Science: California University Bull. 7, 12 pp., Jan., 1875.

Silver; the treatment and reduction of ores: Daily Alta California, vol. 27, no. 9056, p. 1, Jan. 18, 1875; also Min. and Sci. Press, vol. 30, p. 65, Jan. 30, 1875.

Gold: Min. and Sci. Press, vol. 30, p. 78, Jan. 30, 1875.

Steel: Daily Morning Call, San Francisco, vol. 37, no. 69, p. 1, Feb. 7, 1875.

Quicksilver and fuel: Min. and Sci. Press, vol. 30, pp. 98, 102, Feb. 13, 1875

The Comstock; a new feature of its formation: Daily Morning Call, San Francisco, vol. 38, no. 141, p. 1, Oct. 19, 1875.

Notes on a new feature in the Comstock lode: Am. Jour. Sci., 3d ser., vol. 10, pp. 459–462, Dec., 1875.

1876

Public education and the Carpenter bill: Daily Evening Bulletin, San Francisco, Thursday, Mar. 9, 1876.

A costly consequence of the Carpenter bill: Daily Evening Bulletin, San Francisco, Tuesday, Mar. 14, 1876.

The future of the State superintendency: Daily Alta California, Mar. 15, 1876.

1877

Education; its relation to the State and individual, and its methods: California University Bull. 28, 72 pp., Dec., 1877.

1878

The rainfall in California: California University Bull. 31, 10 pp., Feb., 1878.

Contribution to the history of spectrum analysis: Am. Jour. Sci., 3d ser., vol. 16, pp. 392–393, Nov., 1878.

Correction for vacuum in chemical analysis: Am. Jour. Sci., 3d ser., vol. 16, pp. 265–269, 1 fig., 1878. Also, Ueber die Reduction der Wägungen auf den luftleeren Raum bei der chemischen Analyse: Liebig's Annalen, Bd. 195, pp. 222–227, pl. 2, fig. 6, 1879.

1880

[Reconnaissance of the San Francisco, Eureka, and Bodie districts, Nevada]: U. S. Geol. Survey, Clarence King, Director, 1st Ann. Rept., 1879–80, pp. 37–47, Washington, 1880.

Atomic weight determinations; a digest of the investigations published since 1814: Smithsonian Misc. Coll., no. 358, 149 pp., 1880.

——— (and Hague, J. D.). Mining industries at the Paris exposition of 1878. Report of the Commissioners, vol. 4, pp. 165–361, 1880.

1882

A summary of the geology of the Comstock lode and the Washoe district: U. S. Geol. Survey, J. W. Powell, Director, 2d Ann. Rept., 1880–1881, pp. 291–330, 1882. Abstract, Science, vol. 4, pp. 66–67, 1884.

Geology of the Comstock lode and Washoe district: U. S. Geol. Survey, Mon., vol. 3, xv, 422 pp., 7 pls., folio atlas of 21 sheets, Washington, 1882.

1883

On the relations of temperature to glaciation: Am. Jour. Sci., 3d ser., vol. 26, pp. 167–175, Sept., 1883.

1884

The influence of convection on glaciation: Am. Jour. Sci., 3d ser., vol. 27, pp. 473–476, June, 1884.

The relations of the mineral belts of the Pacific slope to the great upheavals: Am. Jour. Sci., 3d ser., vol. 28, pp. 209–212, Sept., 1884. Abstract, Am. Nat., vol. 19, p. 1210, 1885.

[Notes on California]: Science, vol. 3, p. 665, 1884.

Geology of quicksilver: U. S. Geol. Survey, Fourth Ann. Rept., pp. 40–41, 1884.

[U. S. Geological Survey work in California]: Science, vol. 3, p. 665, 1884.

1885

Notes on the stratigraphy of California: U. S. Geol. Survey, J. W. Powell, Director, Bull. 19, vol. 3, pp. 193–218, 1885. Abstracts, Am. Nat., vol. 19, pp. 1211–1212, 1885. Am. Jour., Sci. 3d ser., vol. 30, pp. 399–401, 1885.

Report, division of the Pacific: U. S. Geol. Survey, J. W. Powell, Director, 6th Ann. Rept., 1884–85, pp. 67–70, 1885.

Geological sketch of the Pacific division: 10th Census U. S., vol. 13, statistics and technology of the precious metals, pp. 5–59, 1885.

Impact friction and faulting: Am. Jour. Sci., 3d ser., vol. 30, pp. 116–128, 194–209, 244–245, Aug.–Sept., 1885.

The geometrical form of volcanic cones and the elastic limit of lava: Am. Jour. Sci., 3d ser., vol. 30, pp. 283–293, Oct., 1885.

1886

A theorem of maximum dissipativity: Am. Jour. Sci., 3d ser., vol. 31, pp. 115–120, Feb., 1886.

A new law of thermo-chemistry: Am. Jour. Sci., 3d ser., vol. 31, pp. 120–125, Feb., 1886; Sci. Am. Suppl., vol. 21, no. 537, pp. 8575–8576, 1886.

Cretaceous metamorphic rocks of California: Am. Jour. Sci., 3d ser., vol. 31, pp. 348–357, May, 1886. Abstract, Am. Nat., vol. 20, pp. 724–725, 1886.

1887

The Washoe rocks: California Acad. Sci. Bull., vol. 2, no. 6, pp. 93–120, Jan., 1887. Abstracts, Am. Nat., vol. 20, p. 1050, 1886; vol. 22, pp. 639–640, 1888; Am. Jour. Sci., 3d ser., vol. 33, pp. 75–76, Jan., 1887.

The texture of massive rocks: Am. Jour. Sci., 3d ser., vol. 33, pp. 50–58, Jan., 1887. Abstract, Pop. Sci. Monthly, vol. 31, pp. 425–426, 1887.

Natural solutions of cinnabar, gold, and associated sulphides: Am. Jour. Sci., 3d ser., vol. 33, pp. 199–210, Mar., 1887.

Notes on the relative production and relative value of the precious metals: In E. Atkinson's report upon the present status of bimetallism in Europe: U. S. 50th Cong., 1st sess., Sen. Ex. Doc. no. 34, vol. 1, pp. 47–56, 1887; also report from Consuls of U. S. Dept. State, no. 87, pp. 424–433, Dec., 1887.

A sketch of the geological development of the Pacific slope: Newport Nat. Hist. Soc., Proc., vol. 5, pp. 3–11, 1887.

1888

Report, California division of geology: U. S. Geol. Survey, J. W. Powell, Director, 7th Rept., 1885–86, pp. 93–97, 1888.

Geology of the quicksilver deposits of the Pacific slope: U. S. Geol. Survey, Mon. 13, xix, 486 pp., 7 pls., atlas of 14 sheets, folio, 1888. Abstracts, Am. Geol., vol. 5, pp. 178–180, 1890; Am. Nat., vol. 24, pp. 850–851, 1890; Am. Jour. Sci., 3d ser., vol. 39, pp. 68–69, 1890; Eng. and Min. Jour., vol. 49, pp. 137–138, 1890.

1889

Report, United States Geological Survey, California division of geology: U. S. Geol. Survey, Eighth Report, J. W. Powell, pp. 153–155, 1889.

Report, California division: U. S. Geol. Survey, Ninth Report, J. W. Powell, pp. 100–102, 1889.

Silicic acids: Am. Jour. Sci., 3d ser., vol. 38, pp. 154–157, 1889.

Summary of the geology of the quicksilver deposits of the Pacific slope: U. S. Geol. Survey, Eighth Rept., J. W. Powell, pp. 961–985, Plates 62, 63, 1889.

1890

An elementary proof of the earth's rigidity: Am. Jour. Sci., 3d ser., vol. 39, pp. 336–352, May, 1890.

Report, California division: U. S. Geol. Survey, Tenth Report, J. W. Powell, pp. 141–144, 1890. Abstract, idem, pp. 27–28, 1890.

1891

Crystalline schists of the Coast Ranges of California: Congress géologique Int., 4th session, London, 1888, pp. 170–176, 1891.

Structure of a portion of the Sierra Nevada of California: Geol. Soc. Am., Bull., vol. 2, pp. 49–74, Jan. 10, 1891. Abstracts, Am. Geol., vol. 7, pp. 201–222, 1890; Am. Nat., vol. 24, p. 276, 1890.

Antiquities from under Tuolumne Table Mountain in California: Geol. Soc. Am., Bull., vol. 2, pp. 189–198, pl. 7, Feb., 1891. Abstracts, Am. Geol., vol. 7, pp. 132, 258, 1890; Am. Nat., vol. 25, p. 366, 1891.

Notes on the early Cretaceous of California and Oregon: Geol. Soc. Am., Bull., vol. 2, pp. 201–206, Feb. 20, 1891. Abstracts, Am. Geol., vol. 7, p. 258; Am. Nat., vol. 25, p. 365, 1891.

1893

The finite elastic stress-strain function: Am. Jour. Sci., 3d ser., vol. 46, pp. 337–356, Nov., 1893.
Quicksilver ore deposits: U. S. Geol. Survey, Min. Res., 1892, pp. 139–168, 1893.
Finite homogeneous strain, flow, and rupture of rocks: Geol. Soc. Am., Bull., vol. 4, 13–90, Jan. 3, 1893. Abstract, Am. Geol. vol. 11, p. 411, 1893.
"Potential," a Bernoullian term: Am. Jour. Sci., 3d ser., vol. 45, pp. 97–100, Feb., 1893.
The interior of the earth: North Am. Review, pp. 439–448, Apr., 1893.
Fisher's new hypothesis: Am. Jour. Sci., 3d ser., vol. 46, pp. 137–139, Aug., 1893.

1894

On certain astronomical conditions favorable to glaciation: Am. Jour. Sci., 3d ser., vol. 48, pp. 95–113, figs. 1–3, Aug., 1894. Abstract, Am. Geol., vol. 14, pp. 191–192, 1894.
Sobre la teoria de la sustitución en Almaden: Sociedad española de Historia natural, 2d ser., vol. 3 (23), pp. 1–8, Oct., 1894.
The genesis of ore deposits [discusses a paper on the same subject by F. Pošepný]: Am. Inst. Min. Eng., Trans., vol. 23, pp. 602–604, 1894.

1895

The torsional theory of joints: Am. Inst. Min. Eng. Trans., vol. 24, pp. 130–138, 865–867, 1895.
Gold fields of the southern Appalachians: U. S. Geol. Survey, 16th Ann. Rept., pt. 3, pp. 251–331, pls. 16–18, 1895. Abstract, Reconnaissance of the gold fields of the southern Appalachians: Am. Jour. Sci., 4th ser., vol. 1, pp. 57–60, Jan., 1896.
Distribution of gold deposits in Alaska [Read before the Geological Society of Washington, Nov. 13, 1895]: Jour. Geol., vol. 3, pp. 960–962, Nov.–Dec., 1895.

1896

Schistosity and slaty cleavage: Jour. Geol., vol. 4, pp. 429–448, figs. 1–5, May–June, 1896.
The Witwatersrand and the revolt of the Uitlanders: Nat. Geog. Mag., vol. 7, no. 11, pp. 349–367, 4 pls., Nov., 1896.
The gold fields of South Africa: Cosmopolitan Mag., vol. 22, no. 2, pp. 170–174, 3 figs., Dec., 1896.

1897

The present value of the Witwatersrand: The Economist, London, vol. 55, pp. 4–5, Jan. 2, 1897.
Some queries on rock differentiation: Am. Jour. Sci., 4th ser., vol. 3, pp. 21–40, Jan., 1897. Review by C. F. Tolman, jr., Jour. Geol., vol. 5, pp. 393–398, 1897. Abstract, Science, new ser., vol. 4, p. 927, Dec. 18, 1896.
Note on computing diffusion: Am. Jour. Sci., 4th ser., vol. 3, pp. 280–286, Apr., 1897.
Fractional crystallization of rocks: Am. Jour. Sci., 4th ser., vol. 4, pp. 257–261, fig. 1, Oct., 1897.
Lewis on the diamond: Science, new ser., vol. 6, pp. 664–667, Oct. 29, 1897.
New diamond find in the Transvaal: Science, new ser., vol. 6, pp. 726–727, Nov. 12, 1897.
Discussion of Dr. Don's paper on the genesis of certain auriferous lodes: Am. Inst. Min. Eng., Trans., vol. 27, pp. 998–999, 1897.
Witwatersrand banket, with notes on other gold-bearing pudding stones: U. S. Geol. Survey, 18th Ann. Rept., pt. 5, pp. 153–184, pl. 1, fig. 1, 1897.

1898

Auriferous conglomerates of the Transvaal: Am. Jour. Sci., 4th ser., vol. 5, pp. 193–208, Mar., 1898.
Kant as a natural philosopher: Am. Jour. Sci., 4th ser., vol. 5, pp. 97–112, Feb., 1898.
On the determination of plagioclase feldspars in rock sections: Am. Jour. Sci., vol. 5, pp. 349–354, pl. 3, May, 1898.
American treatment of Indians: Manila Times, Dec. 17, 1898. Also, De como tratan a los indios en la America del Norte: La República Filipina, Mandaloyon, ano 1, no. 78, Dec., 1898.
The Philippine question: Manila Times, Wednesday, Dec. 7, 1898.
Territorial government in the United States; Manila Times, Dec., 23, 1898. Also Gobierno territorial de los Estados-Unidos: República Filipina, Mandaloyon, ano 1, no. 83, Dec. 23, 1898.
Free education in the United States: Manila Times, Dec. 27, 1898. Also, Educación libre en los Estados-Unidos: La Independencia, periodico filipino, ano 1, no. 93, Dec. 24, 1898.
[Explicacion de los propositos que animan al Gobierno de Washington respecto al porvenir del pueblo filipino]: República Filipina, Mandaloyon, ano 1, no. 87 [p. 1], Dec. 28, 1898.
Memorandum on the mineral resources of the Philippine Islands: U. S. Geol. Survey, 19th Ann. Rept., pt. 6, (cont.) Mineral Resources for 1897, pp. 687–693, 1898. Reprinted in the U. S. 55th Cong., 3d sess., Sen. doc. no. 62, pt. 2, pp. 514–518, 1899.

The Witwatersrand banket, with notes on other gold-bearing pudding stones: Reviewed in Zeit. für prak. Geol., Heft. 6, pp. 212–217, 1898.

Reconnaissance of the gold fields of southern Alaska, with some notes on general geology: U. S. Geol. Survey, 18th Ann. Rept., pt. 3, pp. 1–86, pls. 1–31, figs. 1–6, 1898.

1900

Rights and wrongs in South Africa: The Forum, vol. 29, no. 1, pp. 31–37, Mar., 1900.

Are the Philippines worth having?: Scribner's Mag., vol. 27, no. 6, pp. 739–752, illus., June, 1900.

Brief memorandum on the geology of the Philippine Islands: U. S. Geol. Survey, 20th Ann. Rept., pt. 2, pp. 3–7, 1900.

1901

President McKinley's death; a memorial address: Ticonderoga Sentinel, week of Sept. 15, 1901.

Conditions requisite to our success in the Philippine Islands: Am. Geog. Soc., Bull., vol. 33, pp. 112–123, Apr., 1901.

Report on the geology of the Philippine Islands, followed by a version of "Ueber Tertiare fossilien von den Philippinen" (1895) by K. Martin: U. S. Geol. Survey, 21st Ann. Rept., pt. 3, pp. 493–625, 3 pls., 2 figs., 1901. Abstract, Am. Geol., vol. 28, pp. 126–127, 1901.

1903

Project for a geophysical laboratory; appendix I to report of Advisory Committee on Geophysics: Carnegie Inst. of Washington, Yearb. no. 1, pp. 44–58, 1903.

1904

How small an army we need. [Seen only in manuscript—written in 1904—not found in print.]

Construction of geophysical laboratory: Carnegie Inst. of Wash., Yearb. No. 2, 1903, pp. 185–194, 1904.

Experiments on schistosity and slaty cleavage: U. S. Geol. Survey, Bull. No. 241, 34 pp., 7 pls., 1904.

Present problems of geophysics: Science, new ser., vol. 20, pp. 545–556, Oct. 28, 1904; Eng. and Min. Jour., vol. 78, pp. 743–744, Nov. 10, 1904 (in part). Am. Geol., vol. 35, pp. 4–22, Jan., 1905.

1905

Simultaneous joints: Eng. and Min. Jour., vol. 79, pp. 1182–1184, 9 figs., June 22, 1905. Washington Acad. Sci., Proc., vol. 7, pp. 267–275, 1 pl., July 24, 1905.

A feature of Mayón volcano: Wash. Acad. Sci., vol. 7, pp. 277–282, pl. 13, July 24, 1905.

Introduction to Day and Allen's "Isomorphism and thermal properties of the feldspars": Carnegie Inst., Washington, Pub. No. 31, pp. 5–12, 1905.

——— (and Day, A. L.) The linear force of growing crystals: Washington Acad. Sci., Proc., vol. 7, pp. 283–288, 1 fig., July 24, 1905.

——— (and Day, A. L.) An interesting pseudosolid: Washington Acad. Sci., Proc., vol. 7, pp. 289–299, pl. 14, July 24, 1905.

1906

Present problems of geophysics: Congress of Arts and Science, Universal Exposition, St. Louis, 1904, vol. 4, pp. 508–522, 1906.

1907

Current theories of slaty cleavage: Am. Jour. Sci., 4th ser., vol. 24, pp. 1–17, 6 figs., July, 1907. Abstract: Science, new ser., vol. 25, pp. 967–968, June 21, 1907.

Methods of igneous intrusion (abstract): Science, new ser., vol. 25, p. 622, Apr. 19, 1907.

1908

Relations of radioactivity to cosmogony and geology: Geol. Soc. Am., Bull., vol. 19, pp. 113–146, June, 1908.

Age of a cooling globe in which the initial temperature increases directly as the distance from the surface: Science, new ser., vol. 27, pp. 227–233, Feb. 7, p. 392, Mar. 6, 1908.

1909

Origin of the gold of the Rand gold field: Ec. Geology, vol. 4, pp. 373–384, June, 1909.

Half a century in geology; progress made since Agassiz discovered glacial period; New York Times, Section of Astronomy and Meteorology, Sunday, Dec. 12, 1909.

——— (and Van Orstrand, C. E.) Hyperbolic functions: Smithsonian Mathematical Tables, pp. iii–li, 1–321, Washington, 1909.

Relations between local magnetic disturbances and the genesis of petroleum: U. S. Geol. Survey Bull. 401, 24 pp., 1 pl., 1909.

1910

Halley on the age of the ocean: Science, new ser., vol. 31, pp. 459–461, Mar. 25, 1910.
The age of the earth: Smithsonian Misc. Coll., vol. 56, no. 6, pp. 1–28, 1910.
Reflections on Joly's method of determining the ocean's age: Science, new ser., vol. 31, pp. 509–512, Apr. 1, 1910.

1911

Some new mechanical quadratures: Philos. Mag., vol. 22, pp. 342–353. Aug., 1911.
A new toy motor: Science, new ser., vol. 34, p. 683, Nov. 17, 1911.

1912

Biographical notice of Samuel Franklin Emmons: Am. Inst. Min. Eng., Bull. no. 57, pp. 673–691, 1 pl. (port.) Sept., 1911; idem, Trans., vol. 42, pp. 643–661, 1 pl. (port.), 1912.
Major C. E. Dutton [1841–1912]: Am. Jour. Sci., 4th ser., vol. 33, pp. 387–388, Apr., 1912.
The Gudermannian complement and imaginary geometry: Phil. Mag., vol. 24, pp. 600–608, Oct., 1912.

1913

Gold production; a discussion of its effects on prices and wages; possibilities of new fields: New York Times Annalist, p. 120, Feb. 10, 1913.
Biographical notice of Samuel Franklin Emmons [reprinted from Am. Inst. Min. Eng., Trans., vol. 42, pp. 643–661, 1 pl. (port.) 1912]. In Emmons, S. F., Ore deposits, pp. xxix–xlvii, New York, 1913.
The torsional theory of joints (with discussion by H. M. Horne, R. W. Raymond, G. F. Becker, and C. R. Boyd) [reprinted from Am. Inst. Min. Eng., Trans., vol. 24, pp. 130–138, 863–867, 1895]. In Emmons, S. F., Ore deposits, pp. 92, 104, 1 fig., New York, 1913.

1914

Note on mean density of fractured rocks: Washington Acad. Sci. Jour., vol. 4, no. 15, pp. 429–431, Sept. 19, 1914.

1915

On the earth considered as a heat engine: Nat. Acad. Sci., Proc., vol. 1, no. 2, pp. 81–86, Feb.; no. 4, pp. 257–258, Apr., 1915.
Isostasy and radioactivity: Geol. Soc. Am., Bull., vol. 26, no. 1, pp. 171–204, Mar. 31, 1915; Science, new ser., vol. 41, pp. 157–160, Jan. 29, 1915.

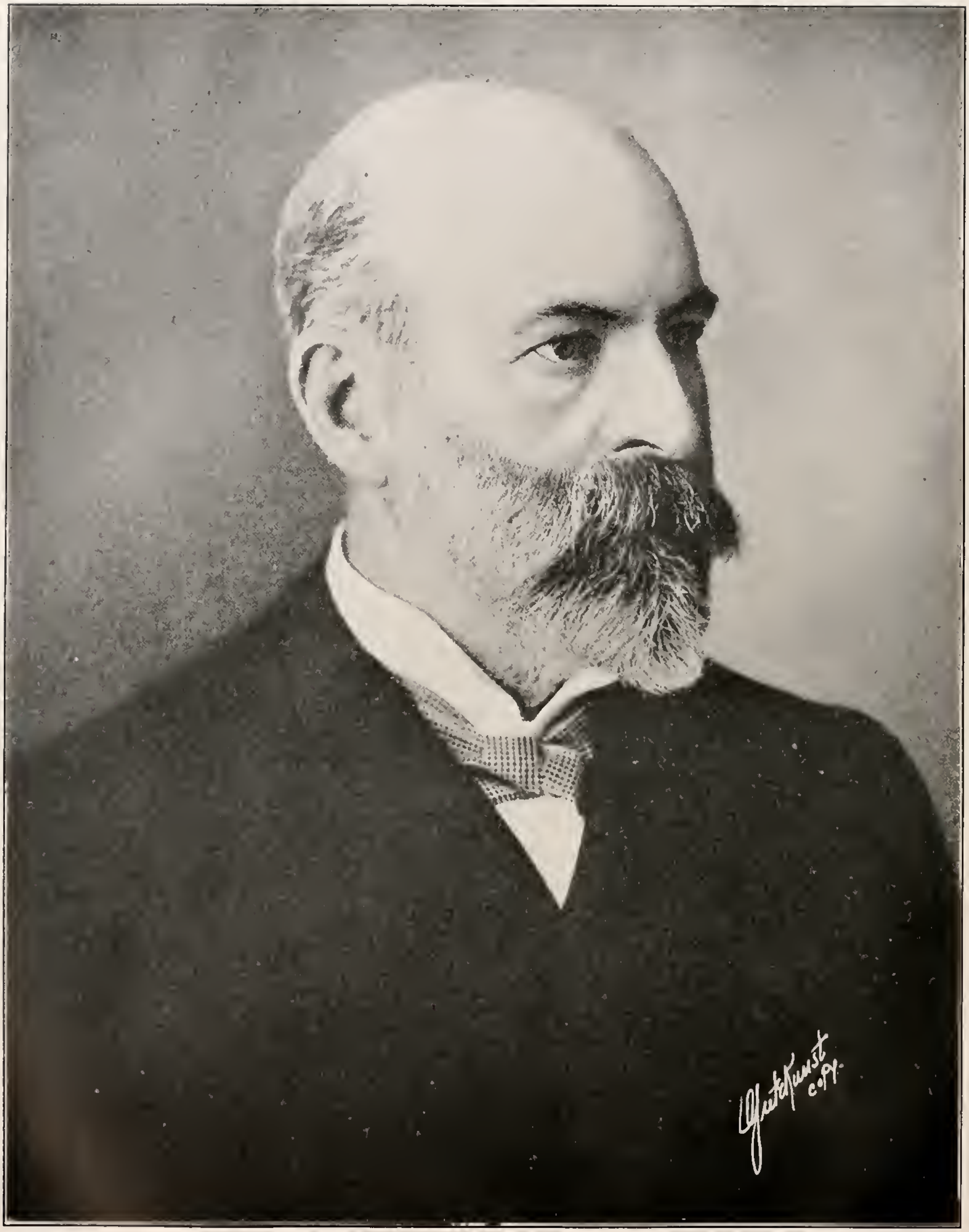
1916

A possible origin for some spiral nebulae: Nat. Acad. Sci., Proc., vol. 2, pp. 1–8, Jan., 1916.
——— (and Day, A. L.) Note on the linear force of growing crystals: Jour. Geology, vol. 24, no. 4, pp. 313–333, 3 figs., May–June, 1916.
Conservatio virium vivarum: Science, new ser., vol. 43, pp. 743–744, May 26, 1916.

1917

Mechanics of the Panama Canal slides: U. S. Geol. Survey, Prof. Paper 98, pp. 253–261, 3 figs., 1917. [Separate published July 25, 1916.] Abstract, Washington Acad. Sci., Jour., vol. 7, no. 1, p. 13, Jan. 4, 1917.

20154°—26——4



J. C. Branner

NATIONAL ACADEMY OF SCIENCES

Volume XXI
THIRD MEMOIR

BIOGRAPHICAL MEMOIR JOHN CASPER BRANNER

1850-1922

BY

R. A. F. PENROSE, Jr.

Presented to the Academy at the Annual Meeting, 1923

JOHN CASPER BRANNER

By R. A. F. Penrose, Jr.[1]

John Casper Branner was born at the town of New Market, Jefferson County, Tenn., on July 4, 1850. He was the son of Michael T. Branner and Elsie Baker Branner. His family were among the early settlers of the Shenandoah Valley of Virginia in the colonial days. They probably came originally from southern Germany or eastern Switzerland before the middle of the eighteenth century, and first settled in Pennsylvania. Somewhat later Casper Branner moved to Virginia, where in 1760 he received a grant of land in the Shenandoah Valley from Lord Fairfax, who had been given large estates in Virginia by Charles II.[2]

The family lived in this region until 1799, when Doctor Branner's great grandfather, Michael Branner, moved to Jefferson County, Tenn., and took up lands near the town of Dandridge on the Frenchbroad River. He became the progenitor of the Tennessee branch of the family, while his brother, John Branner, who remained in the Shenandoah Valley, became the progenitor of the Virginia branch of the family. Both branches have multiplied and have spread widely through many parts of the United States. Strong, active, and earnest people have been characteristic of the family, and many of them have occupied high positions in the communities in which they lived.

In the early childhood of Doctor Branner his family moved from New Market, Tenn., to the farms near Dandridge, owned by his father, some miles distant. At that time the country was sparsely settled, and books and schools were not numerous. The early education of Doctor Branner therefore was confined largely to local schools and to the reading of such books as were available. He attended the Maury Academy, about a mile from Dandridge, and later studied at what was known as the North Schoolhouse, at a school at Graham's Chapel, and still later he attended school at New Market.

Doctor Branner was naturally of an inquiring disposition, and in the scarcity of books he early developed a deep interest in the natural features of the country surrounding him. He thus rapidly became familiar with the character of the rocks and of the animals and flowers that were found in the neighborhood. This early bringing up in close contact with nature, followed later by an education in institutions of high learning, doubtless served to develop that remarkable originality and initiative which distinguished him in later life and which produced a man who became eminent among the scientists of his generation. In his early days he was intended for the ministry and was examined as to his qualifications for it, but he apparently never went further in this calling, having decided to devote himself to science.

In 1866 Doctor Branner went to Maryville College, situated near Knoxville, Tenn., where he remained for about two years. At this period the demoralization in the South which followed the Civil War reached even to Maryville College; the students became discontented and many of them left. Doctor Branner was then 18 years of age, and the new institution known as Cornell University had lately been established at Ithaca, N. Y. He was attracted by the possibilities for study there, and in 1869 went to what was known as the Ithaca Academy to prepare for the university, which he entered the next year.

At Cornell, Doctor Branner took up mostly scientific courses, particularly in geology, botany, zoology, and other branches of natural science. Here also he had the good fortune to meet Prof. Louis Agassiz and Dr. Charles F. Hartt, the latter then professor of geology at

[1] The writer desires to express his sincere appreciation of the information given to him by Mrs. J. C. Branner in the preparation of this memoir. Such assistance has been always willingly granted and has greatly helped the writer in his description of the life work of Doctor Branner.

[2] See "Casper Branner of Virginia and his descendants," by J. C. Branner, Palo Alto, Calif., 1913; also "Address at the reunion of the descendants of Casper Branner of Virginia, held at the original homestead, near Forestville, Va., Aug. 30, 1918." Published in Shenandoah Valley, New Market, Va., Sept. 5, 1918. (The "New Market," Va., mentioned here should not be confused with the "New Market," Tenn., mentioned in the text of this memoir.—Author).

Cornell. Doctor Hartt had made several trips to Brazil in previous years and had published a valuable account of its geology. He finally decided to make another trip into that country and invited Doctor Branner to accompany him. Doctor Branner had not yet completed his university course, but was much pleased at this opportunity to visit what was then a somewhat remote region, and he sailed with Doctor Hartt from New York for Rio de Janeiro in September, 1874. In later years, on his return to the United States, he received the degree of B. S. from Cornell University.

The expedition to Brazil was of much interest and importance as the beginning of the first serious attempt to start systematic geologic work in that country; and it was greatly to the gratification of the two explorers that in the following year the Brazilian Government, under the Emperor Dom Pedro II, established a department to continue this work under the name of Commissão Geologico do Imperio do Brazil. This was due largely to the efforts of Doctor Hartt and Doctor Branner, assisted by Brazilian scientists and others interested in this work. The new department was under the Ministry of Agriculture, and Doctor Hartt was appointed director, with Doctor Branner as assistant. Orville A. Derby, Richard Rathbun, and E. F. Pacheco Jordão were also on the same survey. Work was begun in the spring of 1875.

Doctor Branner's first exploration in Brazil was largely in the coastal region of the State of Pernambuco and in the States of Sergipe and Alagôas, as well as on the island of Fernando de Noronha, off the coast of Brazil. Large collections of geologic materials were rapidly assembled at the headquarters of the Commissão Geologico do Imperio do Brazil in Rio de Janeiro, including many cretaceous fossils from Sergipe and Alagôas, and Doctor Branner did much work in systematizing and arranging them. Somewhat later Dr. Charles A. White also described some of the fossils.

In 1876 Doctor Branner returned to the United States, but went to Brazil again within a few months. The work of the Commissão Geologico was carried on until the next year, when it was discontinued by the Brazilian Government. Doctor Hartt died shortly afterwards. In later years other Government organizations were instituted for geologic research and Doctor Branner in some of his subsequent trips to Brazil worked in conjunction with them.

After the discontinuance of the first survey, however, Doctor Branner accepted a position as assistant to James E. Mills, a well-known American mining engineer engaged in operating gold mines in the State of Minas Gereas. In this work Doctor Branner rapidly became familiar with the older paleozoic rocks of the region and the occurrence of gold and other ores in them; but though the scientific results of the work were of much interest, the financial results were not equally satisfactory, and in 1880 he returned to New York.

A few months later, however, he again went to Brazil at the request of Thomas A. Edison, the inventor, to search for a vegetable fiber which would add strength to incandescent lights. Doctor Branner collected and tested many kinds of bamboo and other fibrous plants throughout Brazil and the neighboring countries of Argentina, Uruguay, and Paraguay, but only a few of them seemed to possess the necessary straight-grained length and hardness desired. Moreover, when occasionally he found a fiber which might partly answer the purpose it was either too difficult to obtain or too rare in its occurrence to use practically. Doctor Branner was a very persistent man and was not easily baffled; but though he traveled many thousands of miles in this search, he eventually concluded that the bamboos of Japan and China, already known to be suitable for the use in question, were usually cheaper and could be more readily obtained than those of South America. He returned to New York again in December, 1881.

In the following year he was commissioned by the United States Department of Agriculture to go to Brazil to study the culture of cotton there, and especially the nature of the insects injurious to the cotton plant, with a view to securing information which might be useful in combatting the destructive insect common in the cotton regions of the United States. Though this investigation was the main feature of the trip, yet he also collected much data on insects injurious to sugar cane, oranges, and other fruits and plants.

This work covered a large area of the country, and Doctor Branner and his assistant, Albert Koeble, were given every assistance by the Brazilian officials to facilitate their research.

They found that the same insect which did the greatest damage to cotton in the United States existed to a greater or less extent in all cotton-growing regions of Brazil, but that it was particularly abundant in certain districts. They made large collections, and in the spring of 1883 Doctor Branner returned to Washington and presented his results to the Department of Agriculture.

After this trip Doctor Branner temporarily ceased his frequent visits to Brazil and accepted an appointment on the Geological Survey of Pennsylvania to do topographic mapping in the Lackawanna Valley and neighboring country, one of the great anthracite and industrial regions of the United States. Prof. J. P. Lesley was director of the survey at that time, and his natural genius in topography was an inspiration to Doctor Branner in accomplishing similar work not only in Pennsylvania but subsequently in other regions. Doctor Branner also made careful observations on the glacial geology of northeastern Pennsylvania, comprising the southerly extension of the great glacial region in these parts, a subject of especial scientific interest to glacial geologists.

In the spring of 1885 Doctor Branner was elected professor of geology at Indiana University, Bloomington, Ind., and in the same year received the degree of Ph. D. from that institution. The president at that time was Dr. David Starr Jordan, noted scientist and one of the foremost ichthyologists in the world. Through his efforts and those of Doctor Branner the university became a center for special instruction and research. Doctor Branner, with his wide professional experience and his knowledge of remote regions, gave great effect to this movement, particularly in his work and instruction in geology, botany, and entomology; and he created a group of enthusiastic young students who later followed him to Arkansas and California. During part of this period he was also connected with the United States Geological Survey.

In the spring of 1887, Doctor Branner was appointed State geologist of Arkansas by Governor Hughes, a position which he accepted with leave of absence from Indiana University. One of the main reasons for the creation of the geological survey of Arkansas was the great excitement over the supposed existence of gold and silver in that State, especially in the Ouachita Mountains, which run westward from Hot Springs to what was then the border of Indian Territory, but which is now the border of Oklahoma.

Many companies capitalized at millions of dollars had been formed to work the alleged mines. A thorough investigation was made by Doctor Branner and his assistants, and they were eventually forced to the conclusion that the mines then known were valueless and the few which contained a little gold and silver carried them in such small quantities as to be insignificant. This announcement of the first work of the geological survey caused great indignation among many of those financially interested in promoting the mines; the State geologist was burned in effigy, and the governor of the State was asked to remove him from office. Doctor Branner, however, stood firm, for he knew that he was correct in his conclusions, and he ignored the bitter efforts to destroy his professional reputation. Governor Hughes also supported him, and the State legislature later indorsed his work and even increased the appropriation for continuing the survey. As time went on and the views of Doctor Branner were verified, the old antagonism was changed to a feeling of remarkable confidence and respect.

Doctor Branner carried on his active survey in Arkansas for about five years, though he continued the work periodically for many years afterwards. It was doubtless the greatest accomplishment of his life; and though accompanied with innumerable difficulties and most arduous work, the result was well worth his splendid efforts. Fourteen volumes were published, and several were prepared but not published on account of lack of funds. They cover the paleontology, stratigraphy, petrology, economic geology, and other natural features of the State. The mineral resources were carefully investigated and discussed throughout the survey reports, but the purely theoretic geology was never forgotten as the economic possibilities were unfolded. The survey was thus of great importance from both purely scientific and economic standpoints, and when Doctor Branner finally left Arkansas to go to Stanford University his departure was regretted by the whole community.

In some of his later trips to Arkansas after the survey closed he carried on geologic investigations previously unfinished and produced valuable results, some of which were described in various scientific journals and in the publications of scientific societies.

Much of the geologic work on the survey was done by Doctor Branner personally, and much of it was done under his supervision by geologists whom he had gathered about him from different parts of the country and by students who had followed him from Indiana University. A remarkable spirit of enthusiasm pervaded them all, and nothing manifested their loyalty to their chief more strikingly than when in 1907, many years after the survey had closed, the surviving members who had assisted Doctor Branner presented to him a portrait of himself as "an expression of their high regard and of their appreciation of his example and inspiritaion as a geologist and as a man." In replying to this presentation Doctor Branner said: "To every member of that former organization I feel strongly attached. A more loyal and more faithful body of men can not be found anywhere. As long as the survey lasted everyone exerted himself to the utmost to do honest scientific work and faithfully to serve the legitimate interest of the people of the State; and it is a great pleasure to know that our work in Arkansas is more highly thought of by the people of that State as time passes."

Doctor Branner was offered the professorship of geology at Stanford University in 1891, and resigned as State geologist of Arkansas and as professor of geology at Indiana University to accept the appointment. The new institution had just been founded and endowed by Senator and Mrs. Leland Stanford in memory of their son and only child, Leland Stanford, jr. The president of the university at that time was Dr. David Starr Jordan, with whom Doctor Branner had formerly been associated at Indiana University; and in California, just as in Indiana, these two men worked together and gathered about them a teaching staff of distinguished scholars from all parts of the United States. The result was that Stanford University rapidly became a recognized institution of advanced learning; in fact, it never went through the condition of slow development which has marked many educational institutions, but it jumped almost immediately to the first rank, and is to-day everywhere regarded with admiration and respect.

Doctor Branner entered upon his duties as professor at Stanford University in the winter of 1892, and for over a quarter of a century, both during his official connection with it and after his retirement, he was active in its development, displaying the same energy and force that he had shown in previous work in other fields. His influence with his students was of an intellectual character which was truly astonishing and which impressed all who came in contact with him. In 1899 Doctor Branner was made vice president, and in 1913 was made president, though he still retained his position as head of the department of geology. In December, 1915, he retired from the presidency, greatly to the regret of the trustees and faculties, and was made president emeritus of Stanford University. In spite of his retirement his interest in the welfare of the university was always manifest and always sought.

In addition to Doctor Branner's educational and administrative work at Stanford, he always maintained his active interest in Brazil, and in 1899 he made a trip to that country for the purpose of studying the immense ocean reefs lying off the coast of Pernambuco, and of distinguishing those composed of sandstone from those of coral origin, a work that had never been done before. Doctor Branner had been familiar with this region ever since his early days in Brazil with Doctor Hartt, but he had not had an opportunity to study it in detail until this trip. The research covered some 1,300 miles of coast line, and a large amount of new geologic information was secured.

Doctor Branner was so deeply interested in exploration in Brazil that every time he visited it he found new material or new districts which he desired to investigate on future trips. In 1907, therefore, he again returned to Brazil in order to study the geology of the black diamond districts of the State of Bahia and adjoining regions. His work covered vast areas, not only in Bahia but in the States of Alagôas and Sergipe. Many thousands of square miles were examined, and the general geology as well as the mineral resources were carefully observed. The

Brazilian Government had followed these explorations with much interest; and realizing their important bearing on the industrial resources of the country, they employed Roderic Crandall, Doctor Branner's assistant, to continue the work after the latter had left Brazil.

Doctor Branner returned to Stanford after about six months' absence, but before long his desire to revisit Brazil returned, and in 1911 he started with a new party for the purpose of making a study of the geology and biology of the Brazilian coast in the neighborhood of the mouth of the Amazon River. Particular attention was given to the study of sea life on both sides of the vast volume of fresh water poured out by that river, and especially to its effect on the marine migration which moves along the coast from the shores of Pernambuco toward the mouth of the Amazon. The haunts and habits of the larger snakes in Brazil were also studied in detail and several specimens of boa were secured. In spite of many difficulties, various new discoveries were made on this expedition, and a number of important papers on special subjects were published.

In consequence of the numerous trips of Doctor Branner to Brazil the world to-day owes to his indefatigable efforts much of its geologic and other scientific knowledge, not only of the eastern part of the country in the States of Pernambuco, Alagôas, Sergipe, Bahia, Minas Gereas, and Rio de Janeiro, where he did a large part of his work, but also of almost every other part. On some of his trips he worked in conjunction with Dr. Orville A. Derby, an American geologist who had been engaged in geologic work under the Brazilian Government and under the government of the State of São Paulo for many years. Doctor Branner was assisted on some of his trips by H. E. Williams, who had been with him on the geological survey of Arkansas, and by Roderic Crandall, who had gone to Brazil with him and who in later years continued work which Doctor Branner had begun. Others of his own countrymen were also on occasions associated with him.

Doctor Branner was on most cordial terms with the Brazilian geologists, many of whom had done excellent scientific work and were always glad to cooperate with him. Some of his work was done jointly with them, and the noted Brazilian geologist, Dr. Miguel Arrojado R. Lisboa, was among his particular friends. Even with the officials of the Empire of Brazil and of the United States of Brazil which followed it, he was on terms of intimate good fellowship, and nothing illustrates this better than the passage of resolutions of condolence at the time of his death by the Chamber of Deputies of the Brazilian Government.

Doctor Branner throughout his whole career naturally took a great interest in the subject of earthquakes, but this interest was much stimulated after the earthquake in California in April, 1906. Soon after that calamity he was appointed by Governor Pardee a member of the State Earthquake Investigation Commission of California. In addition to this commission one of the direct results of the calamity of 1906 was the formation of the Seismological Society of America, of which Doctor Branner was one of the charter members. He was president of the society from 1910 to 1914, and was chairman of the publication committee from 1911 to 1921. In 1915, when widely divergent opinions were being expressed regarding the questions of earthquakes and landslides as affecting the Panama Canal, Doctor Branner was appointed a member of a committee of 10 which was commissioned by the United States Government to visit the Canal Zone and investigate these matters.

Most of Doctor Branner's seismological work, however, was done in California and in more or less direct connection with the Seismological Society. He was extremely active in all these investigations and accomplished important results in collecting data which could be practically applied in the limitation, and in some cases the avoidance, of the destruction caused by earthquakes and by the disastrous fires which often follow them as a consequence of broken water pipes. His work in this field was one of those remarkable accomplishments resulting from purely geologic research that characterized many of his investigations in other subjects.

Prof. Sidney D. Townley, of Stanford University, who is himself a leader in seismological research, in writing of Doctor Branner's connection with the Seismological Society, says, in its Bulletin for March, 1922, that "In the death of Doctor Branner, the Seismological Society has lost one of its staunchest supporters. He gave liberally of his time, energy, and funds in support

of seismological projects; he was the founder of the society's Bulletin, and it was he who provided the ideas and the ideals, the manuscripts, and the funds for the successful continuance of this publication through a difficult period of 10 years; it was he who obtained a gift of $5,000 for the society, and he who by never-tiring efforts trebled its membership; it was he who revived a nearly defunct society in 1910, and through 10 years of constant effort built up an organization of merit, worth, and usefulness."

After Doctor Branner had retired from the presidency of Stanford University he retained his home there and devoted much of his time to work on many scientific problems which his busy life had previously prevented him from finishing. During this period he completed a geological map of Brazil, which was published largely by the assistance of the Geological Society of America, and with the map he wrote explanatory texts both in English and in Portuguese. The great amount of geologic detail displayed over vast areas of country in this map is a mute but eloquent testimony to the research, the learning, and the untiring efforts of its author.

Doctor Branner was primarily a geologist, and his work covered a wide field in various branches of the earth sciences, including paleontology, stratigraphy, mineralogy, seismology, and economics; but he also accomplished important work in entomology, botany, and other branches of biology. He was one of the last of the old-time scientists who were learned in many branches of natural history, before the extreme specialization of modern times had made it necessary for a research worker to confine himself to narrow lines of scientific investigation.

In addition to his accomplishments as a scientist, he was a linguist of unusual ability, a remarkable educator, and a strong leader of men. As a linguist he was learned in both ancient and modern languages. Latin and Greek were thoroughly familiar to him; and in modern languages he was preeminently a scholar in Portuguese, in which he wrote a grammar for English-speaking people, a textbook of geology for the Brazilians, and an explanation of his geologic map of Brazil, as well as numerous geologic reports relating to that country. In his later years he translated from the Portuguese the History of the Origin and Establishment of the Inquisition in Portugal, by Alexandro Herculano. Other modern languages also came easily to him and assisted him greatly in his various travels.

Doctor Branner as an educator achieved remarkable success with the students who studied under him. His constant sympathy with them and his interest in their work did much to inspire that feeling of affection and loyalty preeminently observable in them. His forceful, fearless, and intensely intellectual personality, his wide experience in scientific research in many regions, his broad vision not only in his work but in his knowledge of men, gathered around him at Indiana University, on the Geological Survey of Arkansas, at Stanford University, and on numerous trips to Brazil, a group of followers which was truly wonderful, both in their numbers and in their professional success in later life. His students have spread over almost every part of the world, and an unusually large percentage of them have done honor to their instructor and chief. As he himself said in later years, in referring to certain honorary recognitions which he had received in his profession, the greatest honor of all is that which comes to one from having men "who have been his students doing good and honest work in every quarter of the globe."

Doctor Branner married, in 1883, Miss Susan D. Kennedy, of Oneida, N. Y., a graduate of Vassar College. They had three children, one a daughter, now married, and two sons. They all graduated from Stanford University; and his two sons and his son-in-law enlisted as volunteers in the American Army during the recent war with Germany. In a letter to the writer shortly afterwards Doctor Branner related how he also had tried to enlist but was not accepted on account of age. That never-failing spirit to face boldly and fearlessly whatever difficulties fell to his lot was with him to the last. He died on March 1, 1922, in his seventy-second year.

Doctor Branner was a member of numerous scientific societies and had in many cases received distinguished honors from them. He was a member of the National Academy of Sciences, the American Philosophical Society, the Geological Society of America (president, 1904), the Society of Economic Geologists; Seismological Society (president, 1910–1914); American

Institute of Mining Engineers; Washington Academy of Sciences; London Geological Society; Société Géologique de France; Société Belge de Géologie; Instituto Historico di São Paulo; Brazil Academy; Institute Historico Geographico do Brazil; and many other scientific organizations, and a Fellow of the American Association for the Advancement of Science (secretary, section E, 1888–9; vice president, 1890; president, Pacific division, 1916; chairman Cordilleran section, 1913). He was also an associate editor of the Journal of Geology.

Among the many scholastic and honorary degrees received by Doctor Branner during his career may be mentioned: B. S., Cornell, 1882; Ph. D., Indiana, 1885; LL. D., Arkansas, 1897; Maryville, 1909; California, 1915; Sc.D., Chicago, 1916. In 1911 the Hayden medal award was conferred upon him by the Academy of Natural Sciences of Philadelphia in recognition of his personal contributions to the science of geology.

The remarkably wide sphere of subjects studied by Doctor Branner, and on which he wrote to a greater or less extent, is shown by the following bibliography, comprising over 370 titles, arranged chronologically:

PUBLICATIONS OF JOHN CASPER BRANNER

1884–1921

1884

The course and growth of the fibro-vascular bundles in palms. Proceedings of the American Philosophical Society, April 18, 1884, Vol. XXI, pp. 459–483, 12 figs.

The pororóca or bore of the Amazon. Science, Nov. 28, 1884, Vol. IV, pp. 488–492. Published as separate, with additional notes, 4 figs. Boston, 1885.

Rock inscriptions in Brazil. American Naturalist, Dec., 1884, Vol. XVIII, pp. 1187–1192, 2 figs., 3 plates. The separates contain also pp. 1192a and 1192b.

Preliminary report of observations upon insects injurious to cotton, orange, and sugar-cane in Brazil. U. S. Department of Agriculture, Division of Entomology, Bulletin No. 4, pp. 63–69. Washington, 1884. The same report reprinted as a separate, Boston, 1884.

The Batrachichthys. Science, March 28, 1884, Vol. III, p. 376, 1 fig.

Flexible sandstone. American Naturalist, Sept., 1884, Vol. XVIII, p. 927.

Notes upon the glacial striae observed in Wyoming-Lackawanna region. Lackawanna Institute, Proceedings and Collections, 1884, Vol. I, pp. 19–27.

1885

Inscripçôes em rochedos do Brazil. Translated by Dr. Joao Baptista Regueira Costa and published by the Instituto Archeologico e Geographico Pernambucano. 4 plates. Pernambuco, Brazil, 1885.

Glaciation of the Lackawanna valley. Proceedings of the American Association for the Advancement of Science, August, 1885, Vol. XXXIV, pp. 212–214. Abstract, Science, 1885, Vol. VI, pp. 221–222.

The reputation of the lantern-fly. American Naturalist, Sept., 1885, Vol. XIX, pp. 835–838, 1 fig.

A Gitiranabóia. Liberal Mineiro, Ouro Preto, Brazil, Dec. 19, 1885.

Cotton in the Empire of Brazil; the antiquity, methods, and extent of its cultivation, together with statistics of exportation and home consumption. Department of Agriculture, Special Report No. 8, pp. 79. Washington, 1885.

Cotton caterpillars in Brazil. Appendix V, pp. 49–54, of the Fourth Report of the U. S. Entomological Commission . . . on cotton worm and boll worm. Washington, 1885.

Pororóca oder der Zeitstrom am Amazonas Das Ansland, Vol. 58, pp. 11–15, 1885. Zeitschrift fur Schule Geographie, Vol. VI, p. 201, 1885.

1886

Glaciation of the Wyoming and Lackawanna valleys. Proceedings of the American Philosophical Society, Feb. 19, 1886, Vol. XXIII, pp. 337–357, 2 maps. Abstract, Science, 1886, Vol. VIII, p. 422.

Geographical and geological exploration in Brazil. American Naturalist, August, 1886, Vol. XX, pp. 687–690.

Notes upon a native Brazilian language. Proceedings of the American Association for the Advancement of Science, August, 1886, Vol. XXXV, pp. 329–330.

The thickness of the ice in Northeastern Pennsylvania during the glacial epoch. American Journal of Science, Nov., 1886, Vol. CXXXII, pp. 362–366.

Geological map of Indiana, colored according to the scheme of the International Congress of Geologists, 2 x 4. Indianapolis, 1886.

Rough notes of lectures on Botany. Indiana University, 1886.

1887

The railways of Brazil; reprinted from the Railway Age, July 8, 1887, Vol. XII, pp. 470–473, with notes and additions, 26 pages, 2 maps. Chicago, 1887.

Annual report of the Geological Survey of Arkansas for 1887, 15 pages. Little Rock, 1887.

Additional notes on the lantern-fly of Brazil. Transactions of the New York Academy of Science, Nov. 21, 1887. Vol. VII, pp. 66–68.

Notes on the glacial striae observed in the Lackawanna-Wyoming region. Lackawanna Institute of History and Science, 1887, Vol. I, pp. 19–27. Scranton, 1887.

Topographical map in ten-foot contours of a portion of the Lackawanna valley between Scranton and Carbondale, Lackawanna county, in the Northern Anthracite coal field; scale 1600′ = 1″. Preliminary topographical map, Lackawanna valley sheets, Nos. I and II. Annual Report of the Second Geological Survey of Pennsylvania, 1886. Harrisburg, 1887.

1888

The so-called gold and silver mines of Arkansas; an official report to Governor S. P. Hughes. Arkansas Gazette, Little Rock, Aug., 1888. Engineering and Mining Journal, New York, Aug. 18, 1888.

Notes on the fauna of the islands of Fernando de Noronha. American Naturalist, October, 1888, Vol. XXII, pp. 861–871, 2 figs.

Notes on the Botocúdus and their ornaments. Proceedings of the American Philosophical Society, Nov. 16, 1888, Vol. XXVI, pp. 171–173, 10 figs.

The Cretaceous and Tertiary geology of the Sergipe-Alagôas basin of Brazil. Transactions of the American Philosophical Society, 1888, Vol. XVI, pp. 369–434, 5 plates, 10 figs., 4to.

Administrative report and introduction to Report upon Preliminary Examination of the "Geology of Western Central Arkansas." (Theodore B. Comstock, 320 pages.) Annual Report of the Geological Survey of Arkansas for 1888, Vol. I, pp. xv–xxxi. Little Rock, 1888.

On the manufacture of Portland cement. Chapter XXIX of Annual Report of the Geological Survey of Arkansas for 1888, Vol. II, pp. 291–302. Little Rock, 1888.

Introduction to "The Neozoic Geology of Southwestern Arkansas." (Robert T. Hill, 260 pages.) Annual Report of the Geological Survey of Arkansas for 1888, Vol. II, pp. xi–xiv. Little Rock, 1888.

Introduction to "The Northern Limits of the Mesozoic Rocks in Arkansas." (O. P. Hay, 40 pages.) Annual Report of the Geological Survey of Arkansas for 1888, Vol. II, p. xiii. Little Rock, 1888.

Preface to a Preliminary Report upon a Portion of the Coal Regions of Arkansas to "The Geology of the Coal Regions." (Arthur Winslow, 122 pages.) Annual Report of the Geological Survey of Arkansas for 1888, Vol. III, pp. vii–x. Little Rock, 1888.

Glaciation: its relation to the Lackawanna-Wyoming valley. Lackawanna Institute of History and Science, Vol. I, pp. 3–18, 4 plates. Scranton (Pa.), 1888.

Arkansas gold and silver mines; an official report to Governor S. P. Hughes in reply to certain charges. Arkansas Democrat, Oct. 18, 1888. Engineering and Mining Journal, Oct. 20, 1888, Vol. XLVI, pp. 325–327.

The age and correlation of the Mesozoic rocks of the Sergipe-Alagôas basin of Brazil. Proceedings of the American Association for the Advancement of Science, 1888, Vol. XXXVII, pp. 187–188.

1889

Arkansas State Weather Service. Appendix V of the Annual Report of the Chief Signal Officer, 1888, pp. 72–75. Washington, 1889.

A preliminary statement of the distribution of coal over the area examined by the Geological Survey (of Arkansas). Arkansas Gazette, Little Rock, Feb., 13, 1889.

The geology of Fernando de Noronha. American Journal of Science, Feb., 1889, Vol. XXXVII, pp. 145–161; map, 7 figs.

The Convict-island of Brazil, Fernando de Noronha. Popular Science Monthly, May, 1889, Vol. XXXV, pp. 33–40.

The age and correlation of the Mesozoic rocks of the Sergipe-Alagôas basin of Brazil. Proceedings of the American Association for the Advancement of Science, 1889, Vol. XXXVII, pp. 187–188.

The age of the crystalline rocks of Arkansas. Proceedings of the American Association for the Advancement of Science, 1889, Vol. XXXVII, p. 188.

(With R. N. Brackett.) The peridotite of Pike County, Arkansas. American Journal of Science, 1889. Vol. CXXXVIII, pp. 50–56, 1 fig., 1 plate. Reprinted in Annual Report of the Geological Survey of Arkansas for 1890, Vol. II, pp. 378–391, 1 fig., 1 plate. Abstract, Proceedings of the American Association for the Advancement of Science, 1889, Vol. XXXVII, pp. 188–189; Neues Jahrbuch fur Mineralogie, 1893, pp. 500–501.

Analyses of Fort Smith clay shales. Brick, Tile and Pottery Gazette, June, 1889, Vol. X, p. 114.

Building-stones of Arkansas. Stone, Oct., 1889, Vol. II, pp. 92–93.

Geology of Arkansas. Abstract of a lecture delivered at Pine Bluff, Arkansas. Minutes of the State Teachers' Association of Arkansas, pp. 34–38. Little Rock, 1889.

Clays, Kaolins, and bauxites. Annual Report of the Geological Survey of Arkansas for 1889, Vol. I, about 300 pp. (not published).

1890

Some of the mineral resources of Northwestern Arkansas. Arkansas Gazette, Little Rock, Jan. 12, 1890; Arkansas Press, Jan. 19, 1890.

Professor Hartt in Brazil. Cornell Magazine, Ithaca, N. Y., Feb., 1890, Vol. II, pp. 186–192.

The training of a geologist. American Geologist, March, 1890, Vol. V, pp. 147–160.

The aeolian sandstone of Fernando de Noronha. American Journal of Science, April, 1890, Vol. CXXXIX, pp. 247–257, 8 figs.

Geologia de Fernando de Noronha. No. 36 of the Revista do Instituto Archeologico e Geographico Pernambucano. Pernambuco, Brazil, 1890, pp. 20–21, 1 map, 7 figs.

The relations of the state and national geological surveys to each other, and to the geologists of the country. American Geologist, Nov., 1890, Vol. VI, pp. 295–309; Science, Aug. 29, 1890, Vol. XVI, pp. 120–123; Proceedings of the American Association for the Advancement of Science, 1891, Vol. XXXIX, pp. 219–237.
The pororóca, or bore, of the Amazon. Popular Science Monthly, Dec., 1890, Vol. XXXVIII, pp. 208–215.
Solar halos. Science, 1890, Vol. XV, p. 195.

1891

A preliminary report upon the bauxite deposits of Arkansas, with locations and analyses. Arkansas Gazette, Little Rock, Jan. 8, 1891; Arkansas Press, Jan. 12, 1891; Biennial Report of the State Commissioner of Mines, Manufactures, and Agriculture for 1893–94, pp. 119–126; Biennial Report of the same for 1895–96, pp. 105–112.
Bauxite in Arkansas. American Geologist, March, 1891, Vol. VII, pp. 181–183. Science, March 27, 1891, Vol. XVII, p. 171. Engineering and Mining Journal (N. Y.), 1891, Vol. LI, p. 114.
Introduction to "The Geology of Washington County." (Frederic W. Simonds, 154 pp.). Annual Report of the Geological Survey of Arkansas for 1888, Vol. IV, pp. xi–xiv. Little Rock, 1891.
(With F. V. Coville.) A list of the plants of Arkansas. Annual Report of the Geological Survey of Arkansas for 1888, Vol. IV, pp. 155–242. Little Rock, 1891.
Introduction to "Notes on the Botany of Arkansas." (F. V. Coville, 10 pp.) Annual Report of the Geological Survey of Arkansas for 1888, Vol. IV, pp. 155–156. Little Rock, 1891.
Preface to "The Geology of Crowley's Ridge." (R. Ellsworth Call, 283 pp.) Annual Report of the Geological Survey of Arkansas for 1889, Vol. II, pp. xi–xix. Little Rock, 1891. This volume also contains short articles on "The Relationship of the Pleistocene to the Pre-Pleistocene Formations of Crowley's Ridge and Adjacent Areas South of the Limit of Glaciation." (R. D. Salisbury, 24 pp.); on "Description of Fossil Woods and Lignites from Arkansas." (F. H. Knowlton, 19 pp.)
Preface to "Manganese: Its uses, Ores and Deposits." (R. A. F. Penrose, jr., 642 pp.) Annual Report of the Geological Survey of Arkansas for 1890, Vol. I, pp. xxiii–xxvii. Little Rock, 1891.
Preface to "The Igneous Rocks of Arkansas." (J. Francis Williams, 457 pp.) Report of the Geological Survey of Arkansas, Vol. II, pp. xi–xv. Little Rock, 1891. This volume also contains an article on "Tabulation of the Dikes of Igneous Rock of Arkansas." (J. F. Kemp and J. Francis Williams, 26 pp.).
Analyses of Hot Springs waters. Report of the Superintendent of the Hot Springs Reservation to the Secretary of the Interior, pp. 9–16. Washington, 1891.
David Starr Jordan, LL.D. (A biographical notice.) The Delta Upsilon Quarterly, New York, May, 1891, Vol. IX, pp. 195–198.
(With James Hall and F. French.) Rapport de la séance du 31 Août, 1891, sur les gammes coloriages générales. V. Congrès Géologique International, Washington, 1891, pp. 79–80.
(With Joseph LeConte and F. French.) Rapport de la séance de cloture du 1re Septembre relative a nomination d'une Commission internationale de bibliographie géologique. V. Congrès Géologique International, Washington, 1891, pp. 81–89.

1892

The mineral waters of Arkansas. Annual Report of the Geological Survey of Arkansas for 1891, Vol. I, 144 pp., map. Little Rock, 1892.
The cotton industry in Brazil. Popular Science Monthly, 1892, Vol. XL, pp. 666–674.
The training of a geologist. Third edition, 19 pp., San Francisco, 1892.
Preface to "Whetstones and the Novaculites of Arkansas." (L. S. Griswold, 443 pp.) Annual Report of the Geological Survey of Arkansas for 1890, Vol. III, pp. xv–xviii. Little Rock, 1892. This volume also contains short articles on "Geological Age of the Graptolite Shales of Arkansas." (R. R. Gurley, 16 pp.); "New Species of Graptolites." (R. R. Gurley, 3 pp.); "The Geological Age of the Rocks of the Novaculite Area." (Charles S. Prosser, 5 pp.); "Notes on Lower Carboniferous Plants from the Ouachita Uplift." (Charles S. Prosser, 2 pp.)
Preface to "The Iron Deposits of Arkansas." (R. A. F. Penrose, jr., 153 pp.) Annual Report of the Geological Survey of Arkansas for 1892, Vol. I, p. xi. Little Rock, 1892.
Introductions to papers in "Miscellaneous Reports." Geological Survey of Arkansas for 1891, Vol. II. Little Rock, 1892.
Introduction to "Final Report on the Coal Regions of Arkansas." (Arthur Winslow.) Annual Report of the Geological Survey of Arkansas, 1892, Vol. III. (Not published.)
Introduction to "The Lower Coal Measures of Arkansas." (J. H. Means and G. H. Ashley.) Geological Survey of Arkansas for 1892, Vol. IV. (Not published.)

1893

The lip and ear ornaments of the Botocúdus. Popular Science Monthly, Oct., 1893, Vol. XLIII, pp. 753–757, 5 figs.
The supposed glaciation of Brazil. Journal of Geology, Chicago, Vol. I, pp. 753–772, illustrated.
Preface to "Marbles and Other Limestones." (T. C. Hopkins, 443 pages.) Annual Report of the Geological Survey of Arkansas for 1890, Vol. IV, pp. xvii–xxi. Little Rock, 1893.

Observations upon the erosiou in the hydrographic basin of the Arkansas River above Little Rock. Wilder Quarter-Century Book, pp. 325–337. Ithaca, N. Y., 1893. Also separate, Ithaca, N. Y., 1893.

The coal fields of Arkansas. Mineral Resources of the United States for 1892, pp. 303–306, 1 fig. Washington, 1893.

Proverbs from the Portuguese. The Overland Monthly (San Francisco), May, 1893. Second series, Vol. XXI, pp. 501–503.

A geologica cretacea e terciaria da bacia do Brazil Sergipe-Alagôas: Traducção de Garcia Muniz, 170 pages, Aracajú, 1893. (Portuguese edition of the Cretaceous and Tertiary geology of the Sergipe-Alagôas Basin without illustratious.)

1894

Elevations in the State of Arkansas. Annual Report of the Geological Survey of Arkansas for 1891, Vol. II, pp. 77–152, 2 figs. Little Rock, 1894.

Observations upon the erosion in the hydrographic basin of the Arkansas River above Little Rock, Annual Report of the Geological Survey of Arkansas for 1891. Vol. II, pp. 153–166. Little Rock, 1894.

Magnetic observations and meridian monuments established in Arkansas. Annual Report of the Geological Survey of Arkansas for 1891, Vol. II, pp. 167–176, 10 figs. Little Rock, 1894.

Introduction to "Preliminary List of the Mollusca of Arkansas." (F. A. Sampson, 17 pp.) Annual Report of the Geological Survey of Arkansas for 1891, Vol. II, pp. 179–180. Little Rock, 1894.

Introduction to "Catalogue of the Fishes of Arkansas." (Seth E. Meek, 35 pp.) Annual Report of the Geological Survey of Arkansas for 1891, Vol. II, pp. 216–220. Little Rock, 1894.

Bibliography of the geology of Arkansas. Annual Report of the Geological Survey of Arkansas for 1891, Vol. II, pp. 319–340. Little Rock, 1894.

Introduction to and translation of the political constitutions of Brazil. The Convention Manual of the Sixth New York State Constitutional Convention, 1894. Part 2, Vol. III, Constitution of the Empire, pp. 57–105. Constitution of the United States of Brazil, pp. 107–138. Albany, 1894.

Preface to "The Tertiary Geology of Southern Arkansas." (Gilbert D. Harris, 207 pp.) Annual Report of the Geological Survey of Arkansas for 1892, Vol. II, pp. xiii–xiv. Little Rock, 1894.

Report on road-making materials in Arkansas. U. S. Department of Agriculture, Office of Road Inquiry, Bulletin No. 4, Washington, 1894. Fourth Biennial Report of the Bureau of Mines, Manufactures and Agriculture (of Arkansas) for 1895–96, pp. 90–101. Little Rock, 1896. Also in Fifth Biennial Report of that Bureau for 1897–98, pp. 131–141.

The geological surveys of Arkansas. Journal of Geology, Chicago, Vol. II, pp. 826–836.

The education of a naturalist. Commencement address at Leland Stanford Jr. University, May, 1894. Daily Palo Alto, May 30, 1894. Stanford University, 1894.

Os grés de eolios de Fernando de Noronha. Instituto Archeologico e Geographico Pernambucano, 8 figs. Pernambuco, Brazil, 1894.

Introduction to "Preliminary List of the Myriapoda of Arkansas." (C. H. Bollman, 11 pp.) Annual Report of the Geological Survey of Arkansas for 1891, Vol. II, p. 202. Little Rock, 1894.

Preface to "The Geology of Dallas County." (C. E. Siebeuthal, 42 pp.) Annual Report of the Geological Survey of Arkansas for 1891, Vol. II, p. 278. Little Rock, 1894.

"The Geology of Benton County," by Frederic W. Simonds and T. C. Hopkins, 75 pp. Annual Report of the Geological Survey of Arkansas for 1891, Vol. II. Little Rock, 1894. (J. C. Branner, Director.) Introduction missing.

1895

(With John F. Newsom.) Syllabus of lectures on economic geology. Palo Alto, May, 1895, p. 282.

(With John H. Means.) Great mountain railways. The Chautauqua, July, 1895, pp. 426–433.

Report upon the condition of the Geological Survey of Arkansas. Appendix to the Biennial Message of Governor Wm. M. Fishback to the General Assembly of the State of Arkansas, 1895, pp. 26–33.

Decomposition of rocks in Brazil. Bulletin of the Geological Society of America, 1895, Vol. VII, pp. 255–314. Plates.

1896

Our trade with South America. The Argonaut, San Francisco, Jan. 13, 1896.

Decomposition of rocks iu Brazil. Bulletin of the Geological Society of America, 1895–96, Vol. VII, pp. 255–314, 5 plates, 6 figs.

Thickness of the Paleozoic sediments in Arkansas. American Journal of Science, New Haven, Sept., 1896, Vol. II, pp. 229–236, 8 figs.

Bibliography of clays and the ceramic arts. Bulletin 143 of the U. S. Geological Survey, 114 pp. Washington, 1896.

Review of "The Soil, by F. H. King." Journal of Geology, Chicago, Vol. IV, p. 243.

The decomposition of rocks in Brazil. Editorial iu Journal of Geology, Chicago, Vol. IV, pp. 630–631.

On the size of geologic publications. Editorial in Journal of Geology, Chicago, Vol. IV, pp. 214–217.

A supposta glaciação do Brazil. Revista Brazileira, April, 1896. Vol. VI, pp. 49–55, pp. 106–113. Rio de Janeiro, Brazil, 1896.

The study of Science. (Part of a lecture delivered at the Mount Tamalpais Military Academy.) Overland Monthly, San Francisco, Oct., 1896, Educational Department, pp. 26–30.

Abstract of "Oldest Fossiliferous Beds of the Amazon Region, by F. Katzer." Journal of Geology, Vol. IV, pp. 975–976. Chicago, 1896.

Review of the proceedings of the Indiana Academy of Sciences, geological subjects. Journal of Geology, Vol. IV, p. 981. Chicago, 1896.

A preliminary report upon the bauxite deposits of Arkansas, with locations and analyses. Biennial Report State Commissioner of Mines, Manufactures and Agriculture for 1895–96, pp. 105–112.

Report on road-making materials of Arkansas. Fourth Biennial Report, Bureau of Mines, Manufactures and Agriculture (of Arkansas) for 1895–96. 1896, pp. 90–101.

1897

Note on "O fim da creação, pelo Visconde do Rio Grande." Revista Brazileira, Aug., 1897, pp. 254–255, Rio de Janeiro, Brazil; also in Annuario do Rio Grande do Sul parar o anno de 1898, pp. 261–265. Porto Alegre (Brazil), 1897.

Bacteria and the decomposition of rocks. American Journal of Science, 1897, Vol. CLIII, pp. 438–442, and as separate; abstract Neues Jahrbuch für Mineralogie, 1899, Vol. II, Referate, 84.

The bauxite deposits of Arkansas. Journal of Geology, April–May, 1897, Vol. V, pp. 263–289, 2 plates, 2 figs. Also as separate with 10 pp. additional matter, Chicago, 1897.

(With J. F. Newsom.) The Red River and Clinton monoclines. American Geologist, July, 1897, Vol. XX, pp. 1–13, 1 map and 3 figs.; and separate.

Protection for American colleges. The Nation, New York, May 27, 1897, p. 395.

The introduction of new terms in geology. Science, June 11, 1897, Vol. V, pp. 912–913; Science, July 23, 1897, Vol. VI, pp. 133–134.

Mineral Wealth of Arkansas. Engineering and Mining Journal, Aug. 7, 1897, p. 153.

Geology in its relations to topography. Proceedings of the American Society of Civil Engineers, Oct., 1897, Vol. XXIII, No. 8, pp. 473–495; 1 plate, 16 figs.

Introduction to Ashley's "Geology of the Paleozoic Area of Arkansas South of the Novaculite Region." Proceedings of the American Philosophical Society, 1897, Vol. XXXVI, pp. 217–220.

The former extension of the Appalachians across Mississippi, Louisiana, and Texas. American Journal of Science, Nov., 1897, Vol. CLIV, pp. 357–371, 2 figs. Abstract in Report of the British Association for the Advancement of Science, Toronto meeting, 1897, pp. 643–644; Annales de Géographie, 7me Annee, Sept. 15, 1898, pp. 245–246; Nature, Nov. 18, 1897, Vol. LVII, p. 70; Journal of Geology, Oct.-Nov., 1897, Vol. V, pp. 750–760.

On the reporting of values to land owners by the State Geologist of Arkansas. Hot Springs News, July 12, 1897.

Review of "The Bedford Oolitic Limestone of Indiana. By T. C. Hopkins and C. E. Siebenthal, in 21st Annual Report State Geologist of Indiana." Journal of Geology, July-Aug., 1897, Vol. V, pp. 529–531.

The lost coal report of the Arkansas survey. Letter of Aug. 21, 1897. Batesville Guard, Sept. 3, 1897.

Review of the "Unpublished Reports of the Commisão Geologica do Brazil," Published in the Boletim do Museu Paraense. Journal of Geology, Oct.-Nov., 1897, Vol. V, pp. 756–757.

Review of Katzer's "Devonian fauna of the Rio Maecurú," published in the Boletim do Museu Paraense. Journal of Geology, Vol. V, pp. 757–758. Chicago, 1897.

The phosphate deposits of Arkansas. Colliery Guardian, Vol. 73, 1897, pp. 65–66.

1898

Geology in its relation to topography (with discussion). Proceedings of the American Society of Civil Engineers, June, 1898, Vol. XXXIX, pp. 53–95, 2 plates, 16 figs.

(With O. A. Derby.) On the origin of certain siliceous rocks. Journal of Geology, May-June, 1898, Vol. VI, pp. 366–371. Abstract Neues Jahrbuch für Mineralogie, 1900, Vol. I, p. 408.

A geologist's impression (of the Grand Canyon of the Colorado, and Black Crater, Flagstaff, Arizona). Land of Sunshine Magazine, Aug., 1898, Vol. IX, pp. 149–152, illustrated.

The Spanish University of Salamanca. San Francisco Chronicle, July 17, 1898, p. 12, illustrated. Maryville College Monthly for 1898, Maryville, Tenn.

Syllabus of elementary geology. 300 pp., 18 plates and 51 figs. Stanford University, 1898.

Review of "Earth Sculpture, by James Geikie. The Science Series, G. P. Putnam's Sons, New York, 1898." Science, Dec. 30, new series, Vol. VIII, pp. 957–959.

Report on road-making materials in Arkansas. Fifth Biennial Report, Bureau of Mines, Manufactures and Agriculture (Arkansas). 1897–1898, pp. 131–141.

1899

Some old French place names in the State of Arkansas. Modern Language Notes, Feb., 1899. Vol. XIV, No. 2, pp. 65–80. (Johns Hopkins University, Baltimore, Md.)

Review of "Volcanoes (Science series), by T. G. Bonney." San Francisco Chronicle, March 19, 1899.
The recent ascent of Itambé. National Geographic Magazine, Vol. X, p. 183. Washington, 1899.
Notes upon the São Paulo sheet of the Commissão Geographica e Geologica de São Paulo, published in the Revista Brazileira, Rio de Janeiro, 1899. Vol. XIX, pp. 111–114; republished in the Cidada de Santos, Santos, Brazil. Jan. 10, 1900.
The São Paulo sheet of the topographic survey of São Paulo, Brazil. Journal of Geology, Vol. VII, pp. 788–789. Chicago, 1899.
(With C. E. Gilman.) The stone reef at the mouth of Rio Grande do Norte. American Geologist, Dec., 1899, Vol. XXIV, pp. 342–344, 2 figs.
Note upon "The Upper Silurian fauna of the Rio Trombetas, State of Pará, Brazil, and Devonian mollusca of the State of Pará, Brazil, by John M. Clarke." Archivos do Museu Nacional, Vol. X, pp. 1–48, 49–174. Journal of Geology, Nov.-Dec., 1899, Vol. VII, pp. 813–814.

1900

A recife de pedra na foz do Rio Grande do Norte. Por J. C. Branner e C. E. Gilman. Traduzido por Dr. Alfredo de Carvalho. Revista do Rio Grande do Norte, 1900, Nos. 1, 2, Natal, Jan. e Fev., 1900, pp. 267–271.
Gold in Brazil. Mineral Industry for 1899, Vol. VIII, p. 281. New York, 1900.
Diamonds in Brazil. Mineral Industry for 1899, Vol. VIII, pp. 221–222. New York, 1900.
Ants as geologic agents in the tropics. Journal of Geology, Chicago, Feb.-March, 1900, Vol. VIII, pp. 151–153, 3 figs.
(With J. F. Newsom.) Syllabus of economic geology, second edition, 368+viii pp., 141 figs. Stanford University, 1900. (March 15th.)
South America. Encyclopædia Britannica. 10th edition. Supplement, 1902. (Article written in 1900.)
Review of "A Preliminary Report on the Geology of Louisiana. By G. D. Harris and A. C. Veatch." Journal of Geology, Chicago, April-May, 1900, Vol. VIII, pp. 277–279.
Two characteristic geologic sections on the northeast coast of Brazil. Proceedings of the Washington Academy of Science, Aug. 20, 1900, Vol. II, pp. 185–201, 3 plates, 5 figs.
The origin of the beach cusps. Journal of Geology, Chicago, Sept.-Oct., 1900, Vol. VIII, pp. 481–484, 3 figs.
The zinc and lead region of North Arkansas. Annual Report of the Geological Survey of Arkansas, Vol. V, 395+xiv pp., 38 page plates, 92 figures in the text, and geologic atlas of 7 sheets. Little Rock, December, 1900. Reviewed by C. R. Keyes, Journal of Geology, Chicago, Vol. IX, pp. 634–636.
O mappa topographico do Estado de São Paulo. Revista Brazileira, Vol. XIX, pp. 111–114, 1899. Republished in the Cidade de Santos, Santos, Brazil, Jan. 10, 1900.
Results of the Branner-Agassiz expedition to Brazil, Vol. IV. Two characteristic geologic sections on northeast coast of Brazil. Proceedings of the Washington Academy of Sciences, Aug. 20, 1900, Vol. 2, pp. 185–201.

1901

Review of "A record of the geology of Texas, etc. By F. W. Simonds." Journal of Geology, Chicago, Vol. IX, p. 91.
Review of "Géologie et minéralogie appliquées. Par Henri Charpentier. Paris, 1900." Journal of Geology, Feb.-Mar., 1901, Vol. IX, pp. 198–199.
Os recifes de grés do Rio Formoso (Brazil). Revista do Instituto Archeologico e Geographico Pernambucano, No. 54, pp. 131–136, illustrated. Pernambuco, 1901.
The origin of travertine falls. Science, Aug. 2, 1901, Vol. XIV, pp. 184–185.
The zinc and lead deposits of North Arkansas. Transactions of the American Institute of Mining Engineers, 27 illustrations, 32 pages, Vol. XXXI, pp. 572–603. Republished in Lead and Zinc News of St. Louis, Mo., Vol. II, Nov. 4, 1901, pp. 4–6; Nov. 11, 1901, pp. 4–6; Nov. 18, 1901, pp. 4–6; Nov. 25, 1901, pp. 4–5. Republished in Arkansas Democrat (semi-weekly). Little Rock, Ark., Nov. 24, 1901; Dec. 8, 1901; Dec. 22, 1901; Dec. 29, 1901. Abstract: Engineering and Mining Journal, New York, Nov. 30, 1901, pp. 718–719, 1 fig.
Editorial upon giant ripples. Journal of Geology, Chicago, Sept.-Oct., 1901, Vol. IX, pp. 535–536.
The phosphate rocks of North Arkansas. Arkansas Democrat, Little Rock, Ark., Nov. 3, 1901. Harrison Times, Jan. 18, 1902.
Apontamentos sobre a fauna das Ilhas de Fernando de Noronha. Publicação do Instituto Archeologico e Geographico Pernambucano. 14 pp., 2 figs.; 8vo. Pernambuco, 1901.
The oil-bearing shales of the coast of Brazil. 1900. Transactions of the American Institute of Mining Engineers, 1901, Vol. XXX, pp. 537–554.

1902

Depressions and elevations of the southern archipelagoes of Chile. By Francisco Vidal Gormaz. From the Revista Nueva of Santiago de Chile, 1901. Translation and introduction by J. C. Branner. Scottish Geographical Magazine, Edinburgh, Scotland, January, 1902, Vol. XVIII, pp. 14–24, 1 map. Edinburgh, 1902.

Notes upon the surface geology of Rio Grande do Sul, Brazil. By James E. Mills. Edited from his letters by J. C. Branner. American Geologist, February, 1902, Vol. XXIX, pp. 126–127.

The occurrence of fossil remains of mammals in the interior of the States of Pernambuco and Alagôas, Brazil. American Journal of Science, Feb., 1902, Vol. CLXIII, pp. 133–137; 1 map, 1 half-tone plate.

Geology of the northeast coast of Brazil. Bulletin of the Geological Society of America, Rochester, Vol. XIII, pp. 41–98, 16 figs., 9 plates.

The palm trees of Brazil. Popular Science Monthly, New York, Vol. LX, pp. 386–412, 25 figs.

Discussion of Eric Hedburg's paper on "The Missouri and Arkansas Zinc Region." Transactions of the American Institute of Mining Engineers, Vol. XXXI, pp. 1013–1014.

(With J. F. Newsom.) The phosphate rocks of Arkansas. Bulletin 74, Arkansas Agricultural Experiment Station, Professor R. L. Bennett, Director, pp. 59–123. Fayetteville, Ark., Sept., 1902. 23 figures in text; 15 analyses.

Review of "The Scenery of England and the Causes to which it is due. By the Right Hon. Lord Avebury. New York. The Macmillan Co., 1902." San Francisco Chronicle, April 6, 1902.

Review of the "History of Geology and Paleontology. By Karl von Zittel. London and New York, 1902." San Francisco Chronicle, May 11, 1902.

Review of "The Earth's Beginning. By Sir Robert Stawell Ball. Appleton & Co., 1902." San Francisco Chronicle, June 22, 1902.

Syllabus of a course of lectures on elementary geology. Second edition, 370 pp., 109 figs., 25 plates. Stanford University, 1902.

The Carnegie Institution. Science, New York, Oct. 3, 1902, New series, Vol. XVI, pp. 527–528.

South America. Encyclopædia Britannica. 10th ed. Supplement. London, 1902, pp. 365–370.

1903

Geologia de Pernambuco. Traduzido do Bulletin of the Geological Society of America, Vol. XIII. Par Alfredo de Carvalho. Revista do Instituto Archeologico e Geographico Pernambucano, Vol. X, No. 58, pp. 381–402; No. 59, pp. 507–525. 1903.

Da occurrencia de restos de mammiferos fosseis no interior dos Estados de Pernambuco e Alagôas. Traduzido do American Journal of Science, Vol. XIII. Par Alfredo de Carvalho. Revista do Instituto Archeologico e Geographico Pernambucano, Vol. X, pp. 219–224. 1903.

Notes on the geology of the Hawaiian Islands. American Journal of Science, Oct., 1903, Vol. XVI, pp. 301–316.

Topographic feature of the hanging valleys of the Yosemite. Journal of Geology, 1903, Vol. XI, pp. 547–553.

From school to college. San Jose. Muirson and Wright, 1903. 21 pp.

Is the peak of Fernando de Noronha a volcanic plug like that of Mount Pelée? Illustrated. American Journal of Science, Dec., 1903, Vol. XVI, pp. 442–444.

A bibliography of the geology, mineralogy, and paleontology of Brazil. Archivos do Museu Nacional do Rio de Janeiro, 1903. Vol. XII, pp. 197–309.

Review of "Ensayo de una bibliografia histórica i Geográfica de Chile. Par Nicolas Anriqua R. i L. Ignacio Silva. A." Journal of Geology, Vol. X, p. 921. 1903.

The necessity of common roads in the zinc region of north Arkansas. The Lead and Zinc News, St. Louis, 1903. Vol. V, 3 pp.

The coal lands of western Arkansas. Fort Smith Daily News Record, Aug., 1903.

1904

Memoir of James E. Mills (with bibliography). Proceedings of the 15th Annual Meeting of the Geological Society of America, 1902–03. Reprint from Bulletin of the Geological Society of America, 1903–04, Vol. XIV, pp. 512–517.

Notas para a geologica do Rio Grande do Norte. Traduzidas pelo Alfredo de Carvalho. Revista do Instituto Historico e Geographico do Rio Grande do Norte, Vol. II, pp. 239–248. 1904.

Outline of the genealogy of the first four generations of the Branner family in Virginia. Reprint from "Shenandoah Valley," New Market, Virginia, Dec. 1, 1904. Also separate.

Science in the South: An Address before the University of Tennessee. University of Tennessee Index, Fifth series, 1904. Vol. I, 14 pp.

The stone reefs of Brazil, their geological and geographical relations, with a chapter on the coral reefs. Bulletin of Harvard College Museum of Comparative Zoology. May, 1904, Vol. XLIV, 285 pp.

Review of "Grundzuge der Geologie des unteren Amazonas gebietes, by F. Katzer." Journal of Geology, 1904, Vol. XII, p. 278.

Geology in its relation to topography, in "The Field Practice of Railway Location, by Willard Beahan." (Engineering News Publishing Company), Chapter V, pp. 116–141. New York, 1904.

Does poverty help or hinder a young man? Address to the students of Maryville College. Maryville College, No. 6, 1904, pp. 9–10.

1905

From school to college. Stanford University Press, 1905. No. II, 18 pp.

From school to college. Stanford University, 1905. Address to the students of Stanford University, Sept. 6, 1905. No. III, 23 pp.

Extract from letter to Dr. Paulo Pessoa on Notilops Brama. Jornal do Commercio, April 7, 1905.

Natural mounds of "Hog Wallows." Science, March, 1905, Vol. XXI, pp. 514–516.

Omission of titles of addresses on scientific subjects. Nature, Sept., 1905, Vol. LXXII, p. 534.

Stone reefs on the northeast coast of Brazil. Presidential Address before the Geological Society of America. Bulletin of the Geological Society of America, Jan., 1905, Vol. XVI, pp. 1–12.

Translation of "The geology of the diamond and carbonado washings of Bahia, Brazil, by O. A. Derby." Economic Geology, Nov.–Dec., 1905, Vol. I, pp. 134–142.

Abstracts of "Géosynclinaux et regions a tremblements de terre." Par F. de Montessus de Ballore. Bulletin of Société Belge de Géologie, Vol. XVIII, 1905, pp. 243–267. Journal of Geology, 1905, Vol. XIII, pp. 462–464.

Introduction to the "Miocene foraminifera of California." By Rufus M. Bagg, jr. United States Geological Survey, Bulletin No. 268, 1905.

1906

Review of Becker and Day's "Linear force of growing crystals." Journal of Geology, 1906, Vol. XIV, pp. 162–164.

Review of "A register of national bibliography. By W. P. Courtney." Journal of Geology, 1906, Vol. XIV, p. 254.

Movements on the fault line. Daily Palo Alto Times, May 1, 1906.

Address delivered on the 100th Anniversary of the founding of Maury Academy at Dandridge, Tennessee, May, 1906. Knoxville, 1906, 18 pp.

The San Francisco earthquake from the geologists point of view. The Pacific Monthly, June, 1906.

Geology and the earthquake. Out West, June, 1906, pp. 513–518.

The surface features of the State of Arkansas. University Publishing Company, N. Y., 1906, 16 pp.

Review of "Geology. By T. C. Chamberlin and R. D. Salisbury." Science, 1906, Vol. XXIV, pp. 462–465.

The relations of the drainage of the Santa Clara valley, California, to that of the Pájaro river. Abstract of a paper read before the Geological Section of the American Association for the Advancement of Science, at the Ithaca meeting. Science, 1906, Vol. XXIV, pp. 369–370.

Diamantina, Brazil. A letter. Engineering and Mining Journal, 1906, Vol. LXXXII, p. 747.

Correspondence relating to the survey of the coal fields of Arkansas. Science, 1906, Vol. XXIV, pp. 532–537.

The policy of the U. S. Geological Survey and its bearing upon science and education. Science, 1906, Vol. XXIV, pp. 722–728.

The university training of engineers in economic geology. Economic Geology, 1905–06, Vol. I, pp. 289–294.

Bibliography of clays and the ceramic arts. Second edition. American Ceramic Society, Columbus, Ohio, 1906. 451 pp.

Geologia elementar preparada con referencia especial aos estudantes brazileiros. Rio de Janeiro, 1906. 305 pp.

Review of "Les tremblements de terre et les systemes de deformation tetraedrique de l'ecorce terrestre. Par F. de Montessus de Ballore." Anales de Géographie, 1906, Vol. XV. Journal of Geology, 1906, Vol. XIV, pp. 161–162.

Portions of the report of the State earthquake commission upon "The California earthquake of April 18, 1906." Carnegie Institution, Publication No. 87. Washington, 1908.

Isoseimals: Distribution of apparent intensity. Report of the State Earthquake Investigation Commission on the California Earthquake of April 18, 1906. Vol. I, p. 255.

1907

A drainage peculiarity of the Santa Clara valley affecting fresh water faunas. Journal of Geology, Jan.–Feb.; 1907, Vol. XV, pp. 1–10. Map.

Geology and the earthquake. The California Earthquake of 1906. Edited by David Starr Jordan, 1907. pp. 63–77.

Earthquake of the 18th. Sierra Educational News and Book Review, March, 1907. Vol. III, 12 pp.

The New Geological Survey of Brazil. Science, March, 1907, Vol. XXV, pp. 510–513.

Geology in its relation to topography. Third edition of the article in Beahan's "Field Practice of Railway Location." First edition, 1904, pp. 115–141. 1907.

Una mina salgada. The salting of the São Cyriaco mine in Minas Geraes. Jornal do Commercio, Rio de Janeiro, Aug. 20, 1907.

1908

Annie Law and Fannie Law Andrews. Maryville College Monthly, Dec., 1908. Vol. XI, pp. 61–68.

Bibliography of the geology of Brazil. Bulletin of the Geological Society of America, 1908. 19 pp.

The Bogoslof Islands. Science, 1908, Vol. XXVIII, p. 480.

The clays of Arkansas. U. S. Geological Survey, Bulletin No. 351, 1908. 247 pp.

(With A. C. Lawson, G. K. Gilbert, H. F. Reid, etc.) The California earthquake of April 18, 1906. Report of the State Earthquake Investigation Commission. Carnegie Institution, Publieation No. 87, 1908. Vol. I, pp. 104–111; 255–279; 281–284; 287–290; 292–299; 306–309; 311–313; 316–319; etc.

(With David Starr Jordan.) The cretaceous fishes of Ceara, Brazil. Smithsonian Miscellaneous Collections, 1908. Vol. LII, Part I, pp. 1–29.

The geography of Bahia. Boletim da sociedade geographica de Rio de Janeiro, 1908.

Lista de altitudes no estado da Bahia. Ministerio de Viação e Obras Publicas. Rio de Janeiro, 1908.

Loyalty: An address to the students of Stanford University, Sept. 8, 1908. The Stanford Alumnus, 1908, pp. 9–13. Popular Seienee Monthly, Dec., 1908, Vol. LXXIII, pp. 549–553.

Manganese deposits of Morro da Mina, Brazil. Engineering and Mining Journal, Dee. 1908, Vol. LXXXVI, pp. 1196–1197.

Ralatorio sobre a geologia dos estados da Bahia, Sergipe e Alagôas. Report to the Brazilian Seeretary of Publie Works. Rio de Janeiro, 1908.

Syllabus of a course of leetures on elementary geology. Third edition. Stanford University, 1908. 434 pp.

The Delos Arnold eolleetion of natural history speeimens. Seienee, 1908, Vol. XXVIII, pp. 717–718.

Ibid. Popular Science Monthly, 1908, Vol. LXXIII, pp. 549–553.

The United States Geologieal Survey. Letter signed "Geologist." Engineering and Mining Journal, 1908, Vol. LXXXVI, p. 1066.

(With R. Crandall and H. E. Williams.) Mappa de parte dos Estados da Bahia, Pernambuco, Piauhy e dos Estados de Sergipe e Alagôas. Eseala 1:2,000,000. 1908. Reproduzido pela Inspectoria de Obras contra as Seccas. Ministerio de Viação e Ouras Publieas. 1908.

1909

Bibliography of the geology of the State of Arkansas. Geologieal Survey of Arkansas, 1909, Annual Report, pp. 97–164.

Bibliography of the geology, mineralogy and paleontology of Brazil. Bulletin of the Geological Soeiety of Ameriea, Feb., 1909, Vol. XX, pp. 1–132.

The economie geology of the diamond-bearing highlands of the interior of the State of Bahia, Brazil. Engineering and Mining Journal, May 15 and 22, 1909, Vol. LXXXVII, pp. 981–987; 1031–1033. Also published as a separate, New York, 1909.

Review of "Los temblores en Chile, by M. R. Maehado." Journal of Geology, Sept.–Oet., 1909, Vol. XVII, pp. 586–587.

(With R. Crandall.) O problema das seccas do Norte do Brazil. Boletim do Ministerio da Industria, Viação e Obras Publieas. Rio de Janeiro. No. I, pp. 83–110. 1909.

Some faets and corrections regarding the diamond region of Arkansas. Engineering and Mining Journal, Feb., 1909, Vol. LXXXVII, pp. 371–372.

Relatorio preliminar sobre os resultados das exploracoes no interior do Estado da Bahia, apresentado ao Exmo. Sr. Dr. Jose Mareellino de Souza, D. C., Governador do Estado da Bahia, 17 de Julho de 1909. Rio de Janeiro. Published in Boletim da Direetoria da Agrieulture, Viação e Obras Publieas do Estado da Bahia. pp. 105–108. 1909.

(With J. F. Newsom and Ralph Arnold.) Santa Cruz folio, California. United States Geologieal Survey, Folio 163, 1909.

1910

A brief grammar of the Portuguese language, with exereises and vocabularies. (Henry Holt & Company). 1910. 268 pp.

Review of "A eollege text-book of geology. By T. C. Chamberlin and R. D. Salisbury." Seienee, Jan., 1910, Vol. XXXI, pp. 146–147.

Education for economic efficiency. Proceedings of the Thirteenth Conference for Education in the South. 1910. pp. 196–209.

The geology and topography of the Serra de Jaeobina, State of Bahia, Brazil. Ameriean Journal of Seienee, Dec., 1910, Vol. XXX, pp. 385–392.

Geology of the coast of the State of Alagôas, Brazil. Annals of the Carnegie Museum, 1910, Vol. VII, Part I, pp. 5–22. Illustrated plates.

The geology of the Serra do Mulato, State of Bahia, Brazil. American Journal of Science, Oet., 1910, Vol. XXX, pp. 256–263.

Slates of Arkansas. By A. H. Purdue, with a bibliography of the geology of Arkansas by J. C. Branner. Geologieal Survey of Arkansas, 1910. 170 pp. Illustrated maps.

The tombador esearpment in the State of Bahia, Brazil. American Journal of Science, Nov. 1910, Vol. XXX, pp. 335–343.

Outline of the geology of the black diamond region of Bahia, Brazil. Proceedings of the Australian Assoeiation for the Advancement of Seienee, Section C, 1910, Vol. XII, pp. 324–328.

The luminosity of termites. Science, 1910, Vol. XXXI, pp. 24–25.

Earthquakes in Brazil. Journal of Geology, 1910, Vol. XVIII, pp. 327–335.

Geologic work of ants in tropical America. Bulletin of the Geological Society of America, 1910, Vol. XXI, pp. 449–490. Same condensed. Annual Report of the Smithsonian Institution, 1911, pp. 303–333. Illustrated, 1 plate.

1911

The aggraded limestone plains of the interior of Bahia and the climatic changes suggested by them. Bulletin of the Geological Society of America, May, 1911, Vol. XXII, pp. 187–206.

Comparison of the effects of the earthquakes of Mendoza, Valparaiso, Kingston, and San Francisco. Bulletin of the Seismological Society of America, 1911, Vol. I, pp. 23–27.

The minerals associated with diamonds and carbonados in the State of Bahia, Brazil. American Journal of Science, June, 1911, Vol. XXXI, pp. 480–490.

Methods of geologic investigation and publication. Economic Geology, Jan.–Feb., 1911, Vol. VI, pp. 73–75.

South America. Encyclopædia Britannica, Eleventh edition, 1911. Vol. XXV, pp. 485–489.

Suggested organization for seismological work on the Pacific Coast. Bulletin of the Seismological Society of America, March, 1911, Vol. I, pp. 5–8.

(With J. F. Newsom.) Syllabus of a course of lectures on economic geology. Third edition. Stanford University, 1911. 503 pp.

Reviews of seismological literature. Bulletin of the Seismological Society of America, 1911, Vol. I, pp. 23–27.

Impressions regarding the relations of surface geology to intensity in the Mendoza, Valparaiso, Kingston, and San Francisco earthquakes. Stanford University. Bulletin of the Seismological Society of America, 1911, Vol. I, pp. 38–43.

The geography of northeastern Bahia. Geographical Journal (Royal Geographical Society), 1911, Vol. XXXVIII, pp. 139–152; 256–269.

Miguel A. Lisboa and Eugene Hussak. Translated from the Portuguese by J. C. Branner. Journal do Commercio, Rio de Janeiro, Oct. 7, 1911.

1912

The boundaries and area of the Niles cone. The future water supply of San Francisco. Oct., 1912. pp. 257–259.

An early discovery of fuller's earth in Arkansas. Transactions of the American Institute of Mining Engineers, 1912, Vol. LXVII, pp. 747–750.

Earthquakes in Brazil. Bulletin of the Seismological Society of America, June, 1912, Vol. II, pp. 105–117.

Erroneous geological conclusions on the formation of Livermore Valley. The future water supply of San Francisco. By the Spring Valley Water Company, 1912. pp. 232–232a.

A hydrocarbon found in the diamond and carbonado district of Bahia, Brazil. American Journal of Science, Jan., 1912, Vol. CLXXXIII, pp. 25–26.

Portuguese as well as Spanish. Springfield Republican, Oct. 3, 1912.

Report on the geology of Livermore Valley. The future water supply of San Francisco. The Spring Valley Water Company, Oct., 1912. pp. 203–222.

Syllabus of a course of lectures on elementary geology. Fourth edition. Stanford University, 1912. 462 pp.

Eugene Hussak. Translation of an article by Miguel A. Lisboa. Journal of Geology, 1912, Vol. XX, pp. 148–157.

Reviews and notes on seismology published in Bulletin of the Seismological Society of America, 1912, Vol. II,

Geologic work of ants in tropical America. Report of the Smithsonian Institution, 1911. 1912. pp. 303–333.

Report on the geology of the proposed Hetch Hetchy aqueduct line for the water supply of San Francisco, made to John R. Freeman, July 11, 13, 20, 1912. J. R. Freeman's Report. pp. 110–111; 124–126.

1913

Address before the Instituto Historico e Geographico Brazileiro at Rio de Janeiro. Jornal do Commercio, Rio de Janeiro, June 10, 1913. Diario Official, June 20, 1913.

As areias do Rio Grande do Sul. Annuario do Estado do Rio Grande do Sul para O anno de 1913. Porto Alegre, pp. 294–296, 1913.

Chancellor Jordan and Presideut Branner. Sequoia, Sept., 1913. pp. 23–28.

Casper Branner of Virginia aud his descendants. Privately printed, Stanford University, July, 1913. 476 pp.

An early discovery of fuller's earth in Arkansas. Transactions of the American Institute of Mining Engineers, 1913, Vol. XLIII, pp. 520–523.

Earthquakes and structural engineeriug. Bulletin of the Seismological Society of America, 1913, Vol. III, pp. 1–5.

The estancia beds of Bahia, Sergipe, and Alagôas, Brazil. American Journal of Science, June, 1913.

The estancia beds of Bahia, Sergipe, and Alagôas, Brazil. Papers of the Stanford expedition to Brazil in 1911, Vol. I, pp. 35–48. Reprint from American Journal of Science, June, 1913, Vol. XXXV, pp. 619–632.

The fluting and pitting of granites in the tropics. Proceedings of the American Philosophical Society, 1913, Vol. LII, pp. 163–286. Papers of the Stanford University expedition to Brazil in 1911, Vol. I, pp. 1–30. Stanford University, 1914.

A inspectoria de obras contra as Seccas. Jornal do Commercio, Rio de Janeiro, July 15, 1913.
Os fosseis devonianos do Paraná. Jornal do Commercio, Rio de Janeiro, June 14, 1913.
Review of "The Flowing Road. By Casper Whitney." Science, June 24, 1913, Vol. XXIV, pp. 151–152.
Inaugural address. Trustees series, 1913. No. XXIV. Stanford University.
William Russel Dudley: An address delivered at the memorial services held in the University chapel. Dudley Memorial volume, Stanford University, 1913. pp. 7–10.
The word "selva" in geographic literature. Science, 1913, Vol. XXXVIII, pp. 155–156.
The influence of wind on the accumulation of oil-bearing rocks. Bulletin of the Geological Society of America, 1913, Vol. XXIV, pp. 94–95.

1914

Annual report of the president of the university for the 23d academic year ending July 31, 1914. Stanford University, 1914.
A brief grammar of the Portuguese language. Second edition. 1914. pp. 216—223.
Address to the graduating class of Stanford University, delivered May, 1914. Daily Palo Alto, May, 1914. Stanford Alumnus, May, 1914.
Earthquakes and business in the West: An address before the Commonwealth Club. The California Outlook, Jan. 10, 1914, Vol. XVI, pp. 17–18.
Geologia elementar preparada con referencia especial aos estudantes Brazileiros. 2a edição. Paris, 1914.
Some of the obstacles to North American trade in Brazil. Journal of Race Development, April, 1914, Vol. IV, pp. 461–470.
Papers of the Stanford expedition to Brazil in 1911. Introduction. Stanford University, 1914. pp. 3–6.
Review of "Across Unknown South America. By Henry Savage Landor." Science, 1914, Vol. XXXIX, pp. 577–579.
Review of "The Upper Reaches of the Amazon. By Joseph F. Woodroffe." London, 1914. Bulletin of the American Geographical Society, 1914, Vol. XLVII, pp. 59–60.
Review of "Geological Expedition to Brazil and Chile. By J. B. Woodworth." Bulletin of the American Geographical Society, July, 1914, Vol. LXVI, pp. 936–937.
Seismological notes. Bulletin of the Seismological Society of America, 1914, Vol. IV.

1915

Annual report of the president of Stanford University, 1914–1915.
Address to the graduating class, May, 1915. Stanford Alumnus, May, 1915.
Annual report of the president of the university for the 24th academic year ending July 31, 1915. Trustees series No. 29, 1915.
Brief grammar of the Portuguese language. Third edition, 1915.
American Association for the Advancement of Science. Pacific Coast Committee. Nature and Science on the Pacific Coast, San Francisco, 1915. pp. 62–64.
Patronizing the South American republics. Science, 1915, Vol. LXI, pp. 236–237.
Seismological notes. Bulletin of the Seismological Society of America, 1915, Vol. V.
Structural engineering and earthquakes. Engineering Record, 1915, Vol. LXXII, pp. 780–781. Separate, 1916.
Earthquakes. Nature and Science on the Pacific Coast, San Francisco, 1915.
The untrustworthiness of personal impressions of direction of vibrations in earthquakes. Bulletin of the Seismological Society of America, 1915, Vol. V, pp. 26–29.
Review of "Brazil and the Brazilians. By G. J. Bruce." Yale Review, April, 1915, Vol. IV, pp. 640–645.
Review of "Through the Brazilian Wilderness. By Theodore Roosevelt." Yale Review, April, 1915.
Review of "The Naturalist's Directory. By S. E. Cassino." Science, Jan. 22, 1915, Vol. LXI, p. 135.
Review of "The River Amazon from its sources to the sea. By Paul Fountain." Yale Review, April, 1915, Vol. IV, pp. 640–645.
Review of "The Lower Amazon. By A. Lange." Science, March, 1915, Vol. LXI, pp. 363–364.
The mistakes of professors. By a student. School and Society, 1915, Vol. I, pp. 132–135.
Address to the student body. Stanford University, Sept. 7, 1915.

1916

Can we keep the canal open? The Sunset Magazine, June, 1916. pp. 13–15.
The Panama slides. The Sunset Magazine, June, 1916. pp. 13–15; 70–71.
Committee on Panama Canal slides, of the National Academy of Sciences. Preliminary report on the possibility of controlling the land slides adjacent to the Panama Canal. Proceedings of the National Academy of Sciences, April 15, 1916, Vol. II, pp. 193–207.
The Geological Survey of Arkansas. Manufacturers Record, July, 1916. p. 47.
Orville A. Derby. Science, 1916, Vol. LXIII, p. 596.
Orville A. Derby. Journal of Geology, 1916, Vol. XXIV, pp. 209–214.
Memorial of Orville A. Derby. Bulletin of the Geological Society of America, March, 1916, Vol. XXVII, pp. 15–21.

The opportunities for and obstacles to North American business in Brazil. The South American, Dec.. 1916. pp. 17–19.

The potash-bearing rocks of Arkansas. Arkansas Gazette, April 30, 1916.

Recifes de pedra da costa nordeste do Brasil. Revista do Instituto Historico e Geographico Brasileiro, Rio de Janeiro, 1916, Vol. LXXVII, pp. 68–85.

Seismological notes. Bulletin of the Seismological Society of America, 1916–1917, Vol. VI.

1917

The genesis of asbestiform minerals. Discussion of the paper by Stephen Taber. Bulletin of the American Institute of Mining Engineers, Mar., 1917, Vol. CXXIII, pp. 397–400.

One of Mrs. Stanford's ideals. Founder's Day address. Stanford Alumnus, Mar., 1917, Vol. XVIII, pp. 217–222. And separate.

Seismological notes. Bulletin of the Seismological Society of America, 1917, Vol. VI.

Some of the scientific problems and duties at our doors. Presidential address before the Pacific Division of the American Association for the Advancement of Science. Science, May, 1917, Vol. LXV, pp. 417–424. Abstract with title Ignoring the Earthquake in Literary Digest, July 7, 1917, Vol. LV.

One of the scientific problems at our doors. Bulletin of the Seismological Society of America, 1917, Vol. VII. p. 45. (An abridged form of the paper just listed.)

The Tejon Pass earthquake of October 22, 1916. Bulletin of the Seismological Society of America, 1917, Vol. VII, p. 51.

1918

Address at the reunion of the descendants of Casper Branner of Virginia, held at Forestville, Virginia, Augus 30, 1918. New Market, Va., 1918. 24 pp.

Review of "South America. By Nellie B. Allen." Science, 1918, Vol. LXVIII, pp. 249–250.

A favor da lingua Portuguesa. O Estudante Brazileiro, 1918. Vol. I, pp. 3–4.

Seismological notes. Bulletin of the Seismological Society of America, 1918, Sept., 1918, to Sept., 1919.

1919

The importance of the study of the Portuguese language. Hispania, March, 1919, Vol. II, pp. 87–93 .

One of Mrs. Stanford's ideals. Extract. Daily Palo Alto Times, Memorial number, Stanford edition, 1919. pp. 19–20.

Outlines of the geology of Brazil to accompany the Geologic map of Brazil. Reprinted from Bulletin of the Geological Society of America, 1919, Vol. XXX, pp. 189–338.

Incidents in the history of the Geological Survey of Arkansas, and some conclusions to be drawn therefrom. Arkansas Gazette, Nov. 20, 1919.

1920

Bauxite: Historical foreword in "Outlines of Arkansas Geology." By John G. Ferguson. Little Rock, Arkansas, 1920. pp. 45–46.

Herbert Hoover as an educational illustration. Address delivered at the banquet offered Herbert Hoover by the Alumni of Stanford University at San Francisco, Dec. 29, 1919. Stanford Illustrated Review, Jan., 1920.

Incidents in the history of the Geological Survey of Arkansas, and some conclusions to be drawn therefrom. In "Outlines of Arkansas Geology." By John G. Ferguson. Little Rock, Arkansas, 1920. pp. 14–20.

Resumo da geologia do Brasil para acomphar o mappa geologico do Brasil. O Estudante Brasileiro, Vol. I, pp. 3–5, April, 1920.

Resumo da geologia do Brazil. Edição Brazileiro, 152 pp. 1920.

Trouble with Loro Tatus. Nature Study Review, 1920, Vol. XVI, pp.189–194.

What some animals know about topography. Nature Study Review, 1920, Vol. XVI, pp. 143–144.

Oil and gas geology (of Arkansas). In "Outlines of Arkansas Geology." By John G. Ferguson, 1920. pp. 104–105.

Recent earthquakes in Brazil. Bulletin of the Seismological Society of America, 1920, Vol. X, p. 90.

In addition to the seismological publications of Doctor Branner already mentioned, he was the author of a number of notes and reviews in the bulletins of the Seismological Society of America from 1910 to 1920. Some of these were not signed.

1921

Brazil and its geology with reference to future possibilities of developing oil. California Oil World, May 26, 1921, Vol. XIII, pp. 32–33.

Memorial of J. C. da Costa Sena. Bulletin of the Geological Society of America, March, 1921, Vol. XXXII, pp. 16–18.

O que en faria si fosse estudante Brazileiro. El Estudiante Latino-Americano, May, 1921, Vol. III, pp. 4–7.

Oil prospects in Arkansas. Arkansas Gazette, May 25, 1921. p. 16.

How and why stories. Henry Holt & Company, 1921. 104 pp.

W. G. Farlow.

NATIONAL ACADEMY OF SCIENCES

Volume XXI
FOURTH MEMOIR

BIOGRAPHICAL MEMOIR WILLIAM GILSON FARLOW

1844-1919

BY

WILLIAM ALBERT SETCHELL

Presented to the Academy at the Annual Meeting, 1924

WILLIAM GILSON FARLOW

1844–1919

By William Albert Setchell

William Gilson Farlow was born in Boston, Mass., on December 17, 1844. He was the son of John Smith Farlow and Nancy Wight (Blanchard) Farlow. He received his early education in the public schools, both of the grammar and high school grades. He entered Harvard University in 1862 and received the degree of B. A. in 1866. He entered the Harvard Medical School in November, 1867, and received the degree of M. D. in May, 1870. He was appointed assistant to Asa Gray, Fisher professor of natural history in Harvard University, in July of 1870, continuing in this position for two years. In June, 1872, he sailed for Europe, where he traveled and studied for somewhat over two years, returning to America in the summer of 1874. In the same year he received the appointment of assistant professor of botany at Harvard, with the particular field of cryptogamic botany, giving instruction both at Cambridge and in the newly established Bussey Institution at Jamaica Plain. In 1879 he was appointed professor of cryptogamic botany in Harvard University, with teaching entirely at Cambridge. He continued to teach until 1896, at which time he withdrew from all work along this line except as to advising and assisting certain graduate students. He married Miss Lilian Horsford, daughter of Eben Horsford, in 1900. He died June 3, 1919, having served in the faculty of Harvard University as assistant professor and as professor for 45 years and having advanced to the position of senior member.

As a boy and undergraduate student, William Gilson Farlow seems to have had strong inclinations toward music and botany. In these respects he resembled his father, John Smith Farlow, born in Boston in 1817 and educated there, who, besides being a successful man of business, member of the Legislature of the State of Massachusetts, president of the Massachusetts Reform Club, for many years president of the Newton Public Library, etc., was also for a time president of the Handel and Haydn Society of Boston; and, although with no critical knowledge of botany, was very fond of plants, was a member of the Massachusetts Horticultural Society as well as that of Newton and was awarded many prizes at their exhibitions. During his boyhood and youth, William Gilson Farlow followed his father's likings for science and the humanities and gave many evidences of the same alert and active mind as well as capabilities for comprehensive grasp of fundamentals which characterized his later life. He was twice awarded the Franklin medals for scholarship in the Boston schools. In college he was a member, and secretary for one year, of the Pierian Sodality, acting as pianist and several times soloist at its public concerts. His unusual musical ability attracted the attention of his instructors in music and J. K. Paine, professor of music at Harvard, urged him to take up music as his chosen profession. He was an inimitable story-teller, even in his younger days, and likewise took part in amateur theatrical performances. He was also secretary and treasurer of the O. K. Society in his junior year. He was a member of the Harvard Natural History Society and curator of its herbarium, and his scientific attainments were held in high esteem by his classmates and fellow collegians as well as by his instructors. He was elected secretary of his class at senior class election. He was accustomed to explain his habit of casting quick glances from side to side and slightly upward by saying most humorously that when he was a freshman he was much smaller than he was later on in life and that the sophomores used to throw water out of the second-story windows on the freshmen as they passed the dormitories. Consequently, in watching out for the sophomores at their windows he acquired a lifelong habit. At graduation, filing answers to questions asked by the class, he stated that he had "no definite plans for life."

During the year following his graduation he continued his botanical interests and, taking the advice of Gray and following in the footsteps of most of his botanical predecessors in this country, resolved to proceed to the doctorate in medicine as a preliminary and possibly also as an alternative to entering the field of botany. He spent a portion of this year in studying anatomy under Dr. Jeffries Wyman, in Cambridge, himself an enthusiastic naturalist, and entered the Harvard Medical School in Boston in November, 1867. He entered upon and carried through his medical studies with the zeal and thoroughness characteristic of him, and at the end of his third year he won a coveted appointment as surgical interne at the Massachusetts General Hospital in Boston under the distinguished surgeon Dr. H. J. Bigelow. He obtained his medical degree in May, 1870, and his duty done, his anchor cast to windward, he relinquished the favorable opportunity of advancing in medicine as he had earlier that of entering upon a career in music.

His medical education finished and with no intention of practicing, he returned again to Cambridge, studying with Asa Gray and even helping Gray with his classes. He was formally appointed to an assistantship with Gray in July, 1870, in which position he continued for two years. He succeeded Horace Mann, who had died during Gray's absence in Europe, and at Gray's request attempted to give more instruction in cryptogamic botany than had hitherto been given. With such an inspiring and enthusiastic principal as Gray, he undoubtedly absorbed and otherwise gained an extensive knowledge of the flowering plants and vascular cryptogams, but his chief attention seems to have been directed toward the marine algæ, of which Gray had obtained a considerable collection for an American botanist through his friendship and connection with William Henry Harvey, who had written the Nereis Boreali-Americana, the first account of our American marine algæ, and who with J. Whitman Bailey had worked over and reported on the algæ of the United States exploring expedition, from Charles Wright, from J. G. Agardh, of Lund, and others. He used to tell, with some amusement and for the instruction of those who later were studying in this same field, of his lack of the sense of the importance of certain numbers noted on some of the specimens and how he nearly lost for future generations the valuable specimens distributed by Harvey from Ceylon, from the Friendly Islands, and from Australia. At the same time, another of Gray's pupils, Daniel C. Eaton, of Yale University, was occupying himself with marine algæ and both cooperated with the United States Fish Commission under Spencer F. Baird in work on the southern coast of New England. Farlow spent the summer of 1871 at Woods Hole, on the southern shore of Massachusetts, with the wonderful corps of naturalists which Baird had assembled there. Eaton afterwards joined with Farlow and Dr. C. L. Anderson, of Santa Cruz, Calif., in issuing a series of dried specimens in fascicle form of the marine algæ of North America, but soon relinquished the algæ into Farlow's exclusive charge. During this period of his life, Farlow came into correspondence with J. G. Agardh, of Lund, in Sweden, sending him many rare specimens and receiving determinations, criticisms, and specimens in return. He began during this period of his assistantship to prepare and publish his earlier papers on the marine algæ. During his assistantship he introduced the study of the lower cryptogams into the Harvard curriculum, a novelty in American educational practice.

It will be of interest as well as instructive to glance for a moment at the botanical situation in the United States at the period of Farlow's assistantship to Gray (1870–1872). Gray himself, at 60, was meditating retirement from teaching and administrative duties and was negotiating with Charles Wright as to the work in the herbarium. He was also preparing for the addition of a lecture room and laboratories (completed late in 1871). Sereno Watson was with Gray at that time, whither he had proceeded (1870) to complete his account of the plants of King's expedition. George Lincoln Goodale, who joined Gray as assistant, was destined (1873) to take over the subject of "Vegetable physiology." The corporation of Harvard University had started (1870) the organization of the Bussey Institution, a school of agriculture and horticulture, for which plans had been made by the founder as early as 1835 in a will proven in 1842 and funds turned over to Harvard University by the trustees of the founder in 1861. John Torrey was still alive and professor of botany in Columbia University, although in the last

years of his life (†1873). Daniel C. Eaton, pupil of Gray and grandson of Amos Eaton, was professor of botany in Yale University, with his specialty in ferns, but publishing, at this period, his few papers on marine algæ. Sullivant was still alive but was not publishing. Lesquereux and James had taken over the mosses and C. F. Austin the hepatics. Edward Tuckerman was professor of botany at Amherst College and was writing his classical papers on American Lichenology. Charles H. Peck, at Albany, was acting as State botanist and beginning to publish on fleshy fungi. T. F. Allen was beginning to publish on American Characeæ. In Europe, Hofmeister, although still active, had passed on his mantle and task of preparing a comprehensive Handbuch der Botanik to Sachs and to De Bary, and the oncoming generation was deeply immersed in what has been designated the "vegetations-punkt" type of investigation. J. G. Agardh, at Lund, was veteran in phycology, as Elias Fries was in fungi, while Müller, at Geneva, was working on the Flora Brasiliensis, with lichens as his hobby. Schwendener (1860–1868) was publishing the series of papers on the algal types of lichen-gonidia and was bringing about the fundamental and spirited discussion as to the possible dual nature of the lichen-thallus, which was to be prominent for so many succeeding years. Bornet and Thuret were the foremost exponents of algal morphology and reproduction. Of great importance to Farlow were all of these, but possibly foremost in influence for his chosen profession was the fact that Sachs had produced the second edition of his epoch-making Lehrbuch der Botanik, which had not yet been translated into English and which had not, at the time, made the profound impression outside of Germany which it later created. It seems worth while to mention the situation outlined above, since it, as limited, had a direct influence on Farlow and his work. Even the oldest of our prominent American workers of the present day and even those of Europe were, at this time, not advanced beyond the grades below the university. There was no strictly botanical periodical in America except the Bulletin of the Torrey Botanical Club (1870–), botanical articles being few in production and published in the American Journal of Science or the American Naturalist, but mostly in the proceedings of the few learned societies of that era, such as the American Academy of Arts and Sciences of Boston, the Boston Society of Natural History, the American Philosophical Society of Philadelphia, the Philadelphia Academy of Sciences, the St. Louis Academy of Sciences, and the California Academy of Sciences. The National Academy of Sciences was not founded until 1863.

From the point of view of the condition of botanical science and teaching in America, where the "college" point of view still held the most considerable place in higher education and where "research" was not, as yet, spelled with a capital "R," it is little to be wondered at that Farlow, having followed the botanical tradition of preparing himself in medicine, having associated himself with teaching in Harvard University, following his own natural bent, and in accordance with Gray's sympathetic advice and desire for extending the bounds of botanical instruction and specialization at Harvard, should have been attracted toward the lower cryptogams and have turned his attention to northern and central Europe for the assistance he needed for his further training and orientation in this field as well as in general. As Farlow himself says later on in his life (1896): "It certainly now seems ridiculous that one who had only just finished his medical studies and knew nothing about cryptogams beyond what he had read in leisure moments or had picked up in the field should attempt to teach the subject. But the young are courageous, not to say audacious, if they are not learned, and, it must also be admitted, the demands of students for information on the subject were easily satisfied at that time." Consequently we find him leaving Cambridge and Gray at the end of the second year of his assistantship and setting sail for Europe in June, 1872, where he spent the next two years in study and travel. He burdened himself with specimens, particularly with algæ, many of them from the Oregon and California coasts, collected by E. E. Hall, and C. L. Anderson. Landing in England, he proceeded at once, via Copenhagen, to Lund, in Sweden, to consult and absorb wisdom from J. G. Agardh, the founder of phycological taxonomy. He has left us a glimpse of his visit and experiences at Lund in the charming and characteristic letters to Gray, of which only too little was published in the American Naturalist in 1874. Among the west American marine algæ submitted to Agardh at that time were the specimens upon which he founded the

genus Farlowia. From J. G. Agardh, during this visit and by his correspondence, Farlow was assisted in fixing determinations of his later lists (1875 and 1876) of the algæ of the United States, as well as many which he never published but passed on to his own disciples. From Lund he went to Stockholm and on to Upsala, where he met Elias Fries and his son Th. Fries, authorities in taxonomic lichenology and fungology. Farlow tells in his biographical notice of Edward Tuckerman how the elder Fries recalled the visit of the American lichenologist whose sharp eyes detected, as they strolled on the famous avenue near the University of Upsala, a species of lichen which the elder and most famous lichenologist had never seen there. It is to be recalled also, in connection with the visit to Upsala, that the younger Fries was bringing out his comprehensive work on Scandinavian lichens (1871–1874).

From Sweden, Farlow went to Norway for algæ and then on to Petrograd (St. Petersburg) to examine the collections at the Imperial Academy of Sciences collected by the Lütke expedition (1823–1827), and those collected later by Wosnessenski for the Imperial Academy, on the northwestern coasts of America. These had been studied and reported upon by F. J. Ruprecht and were apparently in the same condition and arrangement as when left by him. From Petrograd, he went to Moscow, thence on to Berlin and Cologne, and finally to Strassburg, to the laboratories of Anton de Bary, in the newly established German university in the territories recently wrested from France.

Anton de Bary was at that period easily the first and foremost plant morphologist in the world, and his students were drawn from all countries. In his laboratories Farlow met many of the future leading botanists of central and northern Europe. Two of these, both Poles, J. Rostafinski and E. Janczewski, became his especial intimates, and after their work at Strassburg was over they journeyed on into France together, or at least met again at the Villa Thuret. De Bary and Sachs were both associated with Hofmeister in his plan for issuing a comprehensive Handbuch der Botanik. De Bary had already published his remarkable work on the morphology and physiology of the fungi, lichens, and myxomycetes (1866), in which it is noticeable that the bacteria were not included as they were in the revised edition (1884), and was at work on his comparative anatomy of the vegetative organs of the flowering plants and ferns (published 1877). Farlow found at Strassburg a master and his disciples deep in the work of testing and advancing botanical knowledge in extensive fields. Of the three associated more closely, Rostafinski gave the world a monograph of the Mycetozoa (1873 and 1875), Janczewski elucidated the development of the ascogonium in Ascobolus (1871), and Farlow investigated and described the first known case of apogamy in ferns (1874). In De Bary's laboratory Farlow learned and practiced the microtechnique of that day and learned much as to methods of instruction, literature, and the work of his contemporaries. Since De Bary paid much attention to the parasitism and saprophytism of fungi and the reactions of host plants to their parasitic forms, we may readily infer that Farlow received much inspiration for the work he instituted on his return to America on phytopathology. It was at this time, as he related later, that he became acquainted with Sachs's textbook (second German edition, 1870), for which his admiration never ceased.

Farlow fully occupied his stay of two years abroad. Besides his work in De Bary's laboratory, he visited Switzerland, becoming acquainted with its Alpine flora, both as to flowering plants and cryptogams, especially the lichens. He settled down for a while at Geneva, where Johann Müller-Argoviensis assisted him in his study of the rich lichen flora of that locality. From his notes as to this part of his stay, we learn that he did not neglect the fungi in his collections and studies. During the stay abroad, Farlow found opportunity of spending some time at the Villa Thuret at Antibes, with G. Thuret and E. Bornet, in phycological studies. Rostafinski and Janczewski were also there. The two French phycologists were foremost in the study of the morphology and development of the algæ. Thuret's masterly series of papers on the zoospores and antheridia of plants, with their superb illustrations (1850–1853), his researches on the fertilization of the Fucaceæ (1855–1857) and, in connection with Bornet, the solution of the cystocarpic development in the red algæ (1867) had marked a new epoch in such study, and his taxonomic work, although he published little in this line, was

based on a thorough knowledge of both the morphology and development of the living plant as well as on the work of his predecessors. Bornet, his coadjutor, was fully his equal and was destined to become Farlow's most beloved and revered friend and correspondent for the rest of his life. Farlow used to say that Bornet was the only botanist he knew who made no real mistakes. Bornet was at the time making the famous study of the algal nature of lichen-gonidia, soon after published (1873). The Bornet and Thuret publications, Notes Algologiques (1876 and 1880) and Etudes Phycologiques (1878) are the most outstanding in the realm of phycology. As a result of their work at Antibes, Janczewski published most important papers on the propagula of the Sphacelariaceæ (1872) and on the methods of growth of the thallus of the brown algæ (1875), as well as papers on the structure of Porphyra (1872) and the development of the cystocarp in certain red algæ (1877). Rostafinski also published several papers on algal structure (1875–1877) and was inspired to begin a revision of the Laminariaceæ, which never came to other than preliminary publication, but which nevertheless had its influence. Farlow, while publishing nothing as an immediate result, was influenced most profoundly in his later work on the algæ, particularly in the studies leading up to his Marine Algæ of New England.

It is impossible to follow all the wanderings of Farlow during his two years abroad, but it is sufficient perhaps to say that he visited Paris and various places in Germany, Italy, England, and Ireland, to examine type specimens, to visit and consult with various botanists, and to familiarize himself with their floras, both phænogamic and cryptogamic. He returned to Cambridge late in the summer of 1874, well equipped in every way to take up the work in his chosen field. He brought with him many authentic specimens, much in the way of literature and notes, and had annexed a host of sympathetic correspondents to assist in developing exact knowledge of our lower cryptogamic orders.

From 1874 to 1879, Farlow was attached particularly to the Bussey Institution, although he gave a certain portion of his time to cryptogamic instruction at the Botanical Garden in Cambridge. I have previously mentioned the Bussey Institution, the idea of which was in the mind of its founder, Benjamin Bussey, of Roxbury, as early as 1835, but which, because of the conditions of the bequest, did not come into active existence until 1870. The workers here were at that time F. H. Storer, dean, and in charge of agricultural chemistry; D. D. Slade, in charge of applied zoology; and C. S. Sargent, at the Arnold Arboretum (established 1872), in charge of arboriculture. It is to be borne in mind that the continent of North America possessed few agricultural colleges or agricultural courses in universities, the majority of the older of these institutions being founded in the late sixties or early seventies. Of stations for agricultural experimentation there were few in Europe, the first, that of Moeckern, near Leipzig, having been organized in 1851, the Rothamstead station, under Lawes and Gilbert, having started somewhat later, and at the time of the inauguration of work at the Bussey Institution between 30 and 40 in Europe all told. The first strictly agricultural experiment station in North America (Connecticut) came into existence in 1875. Storer, with S. W. Johnson, of Yale, and E. W. Hilgard, of Mississippi, Michigan, and finally of California, were developing agricultural chemistry in this country, Storer being particularly interested in the chemistry of fertilizers. The Bussey Institution was intended for several classes of students, both for those not intending to proceed to a degree and those who were candidates for one. We may not wonder, then, that Farlow's work in connection with the Bussey Institution was primarily directed toward the fungi of economic interest and that he laid there, firmly and efficiently, the foundations of what has come to be known as phytopathology. The papers published by Farlow in the Bulletin of the Bussey Institution and elsewhere, between the years 1876 and 1880, show by their titles and content the trend of his interest toward the taxonomic, physiological, and pathological aspects of the fungi, although he still paid very considerable attention to the algæ. His papers on potato rot, diseases of oranges and olives, the downy and powdery mildews, particularly of the grape, the black knot, onion smut, the reddening of salted codfish, the deteriorating effect of certain lower algæ and related organisms in water supply, all are models of their

kind and indications of his activity in connection with the work laid out for the Bussey Institution and the Massachusetts Society for the Promotion of Agriculture, the latter furnishing the plates for Farlow's articles.

During his connection with the Bussey Institution, Farlow also gave instruction in cryptogamic botany at Cambridge two days a week, in a primitive laboratory in Lawrence Hall, and also summer-school instruction at Cambridge and in the marine algæ at Woods Hole, Mass., in what he calls an "improvised laboratory." He had certain advanced students, the first of whom, Byron D. Halsted, later professor of botany at Rutgers College and botanist of the New Jersey Experiment Station, took for his thesis subject: "A classification and description of the American species of Characeæ" (Boston Soc. Nat. Hist., Proc., vol. 20, pp. 169–190, March, 1879).

In 1879, as he tells us (1896, p. 2), the diminished income from the Bussey funds caused a suspension of his instruction at the institution and he was transferred to Cambridge, with the appointment to a professorship of cryptogamic botany, the first recognition of the equal standing of the lower plants with the higher, "cryptogamic" being adopted as a portion of his title, "in order," to quote his own words (1896, p. 9), "to point out the existence of this branch of botany as a proper field for study in this country." Farlow was now 35 years old and had established firmly cryptogamic botany as a worthy branch of university instruction and attention. He was free to devote himself to the building up of his own branch of botany as Asa Gray had in his time, and from even less beginnings, built up his wonderful structure and equipment of phænogamic botany. A room was assigned for laboratory and herbarium in the building of the Lawrence Scientific School, whence it was removed to the attic of Boylston Hall, later to the lower floor of the east wing of the Museum of Comparative Zoology, then to the third floor of the Agassiz addition to that building, and finally to the upper floor of the central or botanical section of the museum building, where it met other divisions of botanical instruction. The botanical establishments at Harvard University have always been scattered and are scattered even at the present day, but during the last years of Farlow's life, economic botany, histology, and physiology were housed in the same building with cryptogamic botany, while the Gray Herbarium and the Arnold Arboretum were more or less distant from them. The period in Farlow's life extending from 1879 to 1896 represents the time of his active teaching of larger as well as of smaller classes and of graduate students. Among his earlier advanced students and assistants of this time was William Trelease, and somewhat later Roland Thaxter, the former soon becoming immersed in work on the morphology and taxonomy of the higher plants, particularly after becoming the first director of the Missouri Botanical Garden, and the latter continuing on with the fungi and becoming Farlow's successor, to carry on the work of placing the great Farlow Herbarium and Library on a permanent basis for growth and influence.

About the year 1885 there came into Farlow's laboratories George Howard Parker, Benjamin Lincoln Robinson, Robert Paine Bigelow, William McMichael Woodworth, and James Ellis Humphreys, who brought with them a true biological spirit and introduced some innovations in botanical methods. Some of this group brought with them from the zoological laboratories the method of embedding in paraffin, and used this technique in their cryptogamic research, probably the first application of this method in any botanical laboratory. About 1887, A. B. Seymour was appointed assistant to Farlow and began his long association with the cryptogamic herbarium and preparation of indices of species and host plants of North American fungi. In the fall of 1887, began my own four years of connection with the cryptogamic laboratories, first as Morgan fellow and later as assistant in biology, and with me, in the laboratory, besides Seymour as assistant, were Kingo Miyabe and W. C. Sturgis. From this time on the cryptogamic laboratories became the shrine toward which the pilgrimages of the cryptogamic students of the United States and Canada were directed. There may be mentioned H. M. Richards, G. J. Peirce, C. L. Mix, T. W. Galloway, L. M. Underwood, E. A. Burt, R. A. Harper, B. M. Duggar, Hermann Schrenck, George T. Moore, and others, most of whom finished up one or more short papers with Farlow or began research work to be reported on later.

In 1883, Farlow began to issue the important series of papers entitled "Contributions from the Cryptogamic Laboratory of Harvard University."

In 1891, intending to relieve himself of routine teaching and to take a trip to Europe, he gave over the teaching of cryptogamic botany to Roland Thaxter, who was called from his position as botanist of the Connecticut Agricultural Experiment Station at New Haven, Conn., for the time being, resuming only graduate instruction in 1892. This, also, he finally relinquished in 1896, in his fifty-second year, although he remained helpful in matters of advice and reference to the end of his life. After 1896, however, the younger generation did not come into intimate contact with him or share to any considerable degree the benefits arising from his direct suggestion and criticism.

After 1896, Farlow devoted himself largely to furthering the projects which had been in his mind, in building up the material basis for his subject, devoting his time to clearing up undetermined and current specimens, preparing material for a future distribution, pushing forward the work on the bibliographical index of North American fungi, and to answering the multitudinous letters asking for advice or assistance on critical points in cryptogamic taxonomy and literature. He was compelled also at this period to assume certain large responsibilities in the business affairs of his family, which made serious inroads on his time and energy. He carried through all these matters with his usual energy and thoroughness, shaping his affairs so as to leave all in orderly fashion when his end might come. Fortunately, he continued able to go on with his work of all kinds until a few weeks before he passed away quietly, conscious and calm until his last moments. There passed away at the close of this last and by no means least active period of his life the dean of American botanists, one who had created more than one subdivision of botany, pure and applied, in North America, who had led, generally directly, but at least indirectly, to the highest goal of attainment practically all of the surviving botanists of his country. He left behind him a sorrowing wife, a host of ardent pupils and followers, and, as a further heritage, collections of books, specimens, notes, drawings, and indices unequaled for work along the lines of cryptogamic botany. His memory remains green and will continue to live with us, his pupils and associates, and his example will continue for the inspiration of generations to come.

The character of William Gilson Farlow was too many-sided for any one person to appraise, record, and attempt to make plain, especially to those who have not had the privilege of prolonged personal contact. To those of us who knew him well little need be said as to his personality and accomplishments. To those who knew him only from his writings or from the treasures of specimens and books which he brought together there is some fair indication of his energy, wisdom, and farsightedness. For the coming generations there is desirable some expression, feeble and inadequate though it must necessarily be, as to his lovableness, his kindliness of spirit, his regard for truth, and straightforwardness. I am thoroughly conscious of how far short any attempts of mine may be in attempting to summarize the qualities and accomplishments of such an outstanding personality as that of William Gilson Farlow; but, having passed in review the main periods of his life, it seems best to undertake some general exposition of certain of numerous manifestations of his personality and his pursuits.

In stature, Farlow was decidedly below the average, a matter concerning which he was somewhat sensitive, especially when associated with one who was tall. He seldom, however, made reference to it except through some witticism. In referring, as he did on rare occasions, to his college days, he used to remark that at that time he was even smaller than at maturity. In the one room on the third floor of the Alexander Agassiz section of the Museum of Comparative Zoology, which served for cryptogamic laboratory and herbarium in my own first years at Cambridge—a lofty room piled high with materials—he was accustomed to ask me, the tallest of the workers, to get something from the top of one of the cases, with the usual after remark, "Now, please touch the ceiling." His own worktables and desks were made so low and his chairs so high that no one else could work at them comfortably.

His figure—erect when younger and slightly bowed in his latest years—passing from his house on Quincy Street up through Divinity Avenue to the museum with short, rapid steps,

always with books or manuscript under his arm, was distinctive and could easily be recognized as far as it could be seen. His downward, sidewise glance, seemingly furtive but really diffident when one came to realize its significance, was keen, and there was little that escaped it. The beginning of his conversation was often abrupt, but passed on into a monologue when discussing a problem or recent occurrences in the botanical world, which ended usually with some interrogation, often disconcerting as to whether an answer was demanded or not. Often some query on the part of others was greeted with a laugh or chuckle, which frequently placed his listeners more or less hors de combat and demanded further explanation or discussion. His ejaculations of surprise or incredulity were characteristic. Very commonly he would say: "Mercy! Bless my soul! I wonder where we are coming to when so-and-so puts forth such a view." Occasionally when he had some puzzling plant before him he would come over to our laboratory table and, laying down the specimen, say: "I will give any one of you 5 cents if you will tell me what this is." Many such a problem was placed before us, and we wrestled with it mightily, but seldom were we able to win the munificent reward, although at times we were given what we were much the more anxious to obtain, viz, his recognition of merit in our suggestions. This recognition was difficult of complete affirmation, since his critical mind interposed every possible objection, and the attainment of even partial approval was the result of a strenuous elimination of all that could not be sturdily and properly maintained. This method begot caution about accepting evidence unless of the most definite and pertinent variety. The alternative views he presented during such discussions, the keenness with which he detected flaws in the arguments presented, or the merciless fashion in which he carried some point raised to its logical and usually absurd or irrelevant conclusion, all these characterized the workings of his mind and made a profound and, if viewed properly, a most profitable impression on his associates. Many there were who misunderstood his extremely critical attitude, his witticisms, and his lack of acceptance of any pronounced opinion, even of his own, but those who came in daily contact with him soon learned to estimate them at their true worth and to welcome them as leading to the truth as nearly as it might be possible to approximate to it. To his students Farlow, while critical of their endeavor, was always sympathetic, even to those who least appreciated his efforts. Many a student received material aid, either directly or indirectly, and found him most embarrassed, seeming even cynical, when he attempted to express his appreciation. I remember the case of one assistant who married during vacation time and chose for his wife a young woman as poor as himself. Farlow was much excited and said to me: "Mercy! Bless my soul! What do you think has happened? I have just been informed that X has been married. He only receives $500 for the next year and no prospect of any more for I don't know how many years. What are we coming to?" This was accompanied by a look which showed his concern and despair. X, however, seemed to manage and soon passed on from Harvard to a position yielding at least more than $500, but Farlow gave no sign of having intervened. This case is typical.

In spite of differences of opinion as to the value of certain methods of work and the kind of results obtained, Farlow was always willing to look up points in literature and material for others and spent much of his time and energy in doing so, although often ill repaid in the final outcome. He was earnest rather than outwardly enthusiastic, but the attention he gave to details and larger points for those who desired to do good work was more significant than any amount of outward approval or compliment. He was a kind friend and counselor, although he seldom gave direct advice and his assistance, other than in direct line of his subject, was indirect and unobtrusive. As a host he was perfect, and at the gatherings at his rooms, or later in his own home, he knew how to draw out even the most diffident to join in the conversation and to feel at ease. He made the treasures of his library and his collections available, but always with circumspect reserve, to his students and visiting specialists, ever with due respect to their care and preservation. On the treasures of his mind, which were enormous, one could always draw and no one ever came away from a visit to him without added profit and comfort. His retentive memory and the breadth of his reading and acquisitive instinct made his knowledge encyclopedic in extent and his mastery of detail, without loss of coordination, was simply marvelous. At no

time did he show greater control of his mental balance and wisdom in meeting a particular situation than in the last weeks of his life, when, knowing as a physician that his end was certainly approaching, with calmness and deliberation he arranged his various and very considerable affairs and consulted with those who were to carry on his work and those for whom he desired to provide. During the last several years of his life, in fact, he had devoted himself to preparing for this end, which came peacefully to him, still in possession of his mental faculties. A word as to Farlow's health may not be amiss in this account. It may be said that, although never robust in the commonly accepted sense of the term, and although subject through much of his life to distressing and nerve-racking headaches, he lost little time from his work through illness and spent more than the ordinary working hours of the day in his pursuits. In later years he was less subject to these interruptions of his work and was amazingly cheerful as well as industrious.

As a field naturalist, Farlow was keen and untiring, although few of his later students had the opportunity of observing him in this capacity. As to his earlier trips and methods I know little except from casual remarks. He was wont, at times, to compare the condition of the neighborhood of the time with what it was earlier, when, judging from his reminiscences over some specimens, he lamented the intrusion of asphalt pavements and garbage heaps in select localities and called to mind that Rev. Prof. A. P. Peabody, then an elderly man and college pastor, could remember back to the time when *Arethusa bulbosa* grew in one corner of the college yard. Even in my own day (1887–1891) at Cambridge there remained some traces of good collecting places, such as "Norton's Woods," a small patch of woodland to the north of the museum, "Glacialis," near Fresh Pond, etc., but the tracks of progress were already blotting them out, although it was still possible to obtain a considerable number of both algæ and fungi from them. Our few excursions with Farlow, especially those to the seashore, opened our eyes to the possibilities of keen-eyed and experienced collecting. Every form of plant life had its point, or points, of interest, and we returned home from such a trip laden down with specimens and our minds stored with information concerning them. His first collecting was undoubtedly in the vicinity of Boston, Cambridge, and Newton. He early visited the seashore of the north coast of New England and the White Mountains of New Hampshire. These remained his principal collecting places, but in his early years of teaching he collected on the south shore of New England and proceeded on the north shore as far as Eastport. During his two years abroad he collected, probably extensively, in some favored localities. He mentions Switzerland particularly for the lichens and flowering plants. He was zealous also in his search for fungi, since he realized, as he intimated again and again in his writings, that little was to be obtained from American sources as to type or even reasonably authentic specimens of any kind, and an acquaintance with the traditions of mycology was one of the first points to be gained for future progress. His collections of marine algæ at Woods Hole and Gloucester, Mass., and at Eastport, Me., supplemented by his considerable collections at other places along the northeastern coast of the United States, were the foundation of his Marine Algæ of New England, and supplemented by his experiences along the Florida coast in 1875 and the California coast in 1885, both trips in company with Asa Gray, formed the personal experience basis of his broader work on the marine algæ of the United States. Farlow made trips to the Bermuda Islands in 1881 and 1900, collecting all sorts of cryptogams, but especially algæ, fungi, and lichens. He detected during these visits several species not noticed by any of the other botanists visiting the islands.

While Farlow's trips to Florida and to Mexico, California, and the Bermudas were general as to interest, yet marine algæ were the principal feature. His mycological collecting was largely done nearer home and almost exclusively in New England. Owing to his attraction and more or less of propulsion toward phytopathology, the parasitic fungi are more prominent in his published writings, yet it must be emphasized that he was a great collector and student of the fleshy fungi and that he left unpublished a considerable series of magnificent colored plates (already printed) of our American species. His studies on the Gymnosporangia or Cedar-Apples of the United States (misprinted "The Gymnosporangia or Cider-Apples of the United States" in first proof) is classic and was the forerunner of such monographic work on our fungi.

His other published work on the Rusts or Uredineæ shows his interests and insight into this difficult group of plant parasites. He went so far as to have prepared and even lithographed figures of the spores (telia) of the species of some of the more critical genera, but the text was never prepared. In regard to the perplexing synonymy, he used to remark that it was very likely that Adam may have named all the flowering plants, but that Eve must have named the Uredineæ. Eastern Massachusetts and New Hampshire, particularly the White Mountain region, were his field for fungi, as well as other cryptogams, nor did he pass unnoticed the flowering plants. His friendship with such inveterate collectors and students as the Faxon brothers, led him even into other New England territory. In later life his summers were usually spent in New Hampshire, either at Shelburne, where he found so many rarities, or, after his marriage, in his summer residence at Chocorua, overlooking the lake, where the field for fungi of all kinds was of the richest. He himself has told the very interesting story of how, while resting on a couch on the veranda of his place at Chocorua, he heard a pattering noise and, looking, saw a squirrel with some object in his mouth. A movement alarmed the squirrel, who dropped what it was carrying and fled. On examining the object, Farlow found it to be one of the hypogæous fungi which are so seldom collected and which, without this contribution from the friendly animal, he might never have seen. It brought also to his mind the larger suggestion of the dispersal agency we are now realizing so well in California, concerned in connection with hypogæous fungi in general. In his honor, one of the shoulders of Mount Chocorua, running from the peak along the ledges to the "Brook Trail," where he did much of the collecting of his last years, has been named Farlow Ridge. The last years of his life, Farlow spent much of his time putting the various specimens he had collected into condition, and since his death some of them have been sent out under the title of "Reliquiæ Farlowianæ." While realizing that the "closet-botanist" was a very important and helpful member of the profession, his various expressions as to fear of his being classed strictly in that ilk gave evidence of the importance he attached to field studies.

As a collector in the field, Farlow was very keen and successful, and his herbarium is full of results of his activity in this line. The influence of the great collections accumulated by Asa Gray, the foundation of the Gray Herbarium of to-day, rich in variety and in type material of the flowering plants and the vascular cryptogams, and poor, but not entirely lacking, in representatives, and very valuable ones, of the lower cryptogams, as well as the influence of Asa Gray himself, by example and by practice, led Farlow very early to the task of bringing together a similar authoritative and working collection of cryptogamous plants, particularly of lichens, algæ, and fungi. Farlow's earlier experiences in attempting to put into order and availability the cryptogamic portions of Gray's herbarium and to arrange and classify his own collections were augmented by his many and extremely valuable purchases and exchanges. The first considerable collection to be purchased was the fungus herbarium of Rev. M. A. Curtis, of Asheville, N. C. This was acquired for Farlow by Asa Gray while the former was studying in Europe. The Curtis collection is rich in specimens from Schweinitz, in those collected by the various exploring expeditions, and in duplicate specimens retained by Curtis from sendings abroad for identification and publication by such European authorities as Elias Fries, Berkeley, De Notaris, Desmazieres, Duby, and others. This collection was purchased in 1872. Through "friends" of Harvard University, there was purchased in 1898 the collections of Prof. Edward Tuckerman, of Amherst College, the founder of American lichenology. These collections were rich in types and other authentic specimens of Tuckerman and all the lichenologists of his day. The Tuckerman collections contain most of the older and rarer lichen Exsiccati as well as the unrivaled series of North American specimens collected by the founder and his correspondents. It has also a representation of the lichens of the various exploring expeditions undertaken by the United States. To these collections of fungi and lichens, Farlow added enormously through his own collecting and by those received through his pupils and correspondents. The marine algæ are due to his own efforts and those of his correspondents, the only collection of any size acquired by purchase being the small De Alton Saunders collection. In the collection of marine algæ, however, are specimens from every then living phycologist of note as well as from those who preceded them. I am not in possession of any exact

numerical estimate of these various collections which Farlow brought together, but figures give only a very inadequate idea of the value of the assembled material. In 1896, however, Farlow made the statement that the cryptogamic collections (in largest degree due to his own efforts) must number several hundred thousand.

Farlow early appreciated the value of published sets of specimens ("Exsiccati" or "Exsiccatæ") and diligently sought out such as might be purchased. His success in this direction was most extraordinary, so that in his Sketch of Cryptogamic Botany in Harvard University, he states that between 1872 and 1896 there were brought together (and kept together as sets), not including those complete or partial sets whose numbers were scattered through the general herbarium, 75 different series, including 64,000 specimens representing about 23,000 distinct species. From 1896 to 1919 he continued to add to this series, both of older and of current issues. It is to be remembered that each of these specimens is a datum of reference, and it is doubtful whether any such considerable collection of fundamental specimens exists anywhere else. In connection with this unique collection of published specimens of the lower cryptogams, it seems very desirable to note Farlow's attitude toward their preservation and arrangement. He kept each series of specimens with their printed labels, title pages of the fascicles, etc., together and intact, while the more usual method is to separate them from one another and distribute them through the general collection. By the latter method, the relation of the series, date of issue, etc., is lost. The specimens cease to be integral parts of a "published" series and are often difficult of location in the general collection because of shifting views as to synonymy, etc. Under Farlow's method the specimen, usually quoted by number, is readily located and all data as to details of publication may be readily ascertained. To facilitate access, Farlow indexed all these specimens and even made the proper cross references, so that the existing status of a specimen might readily be ascertained, or all published specimens relating to a certain species might readily be found and comparison made. Farlow left these collections, both general and published, to Harvard University under certain conditions. It is to the credit of those concerned in carrying out the trust that the conditions have been fulfilled and that the Farlow Herbarium is now lodged in a fireproof building, arranged and cared for as a basal unit, for the benefit of cryptogamic botanists of the present generation as well as of those to come.

Under the present disposition of the Farlow Herbarium, the Farlow Library is housed in the same building and in convenient juxtaposition to the specimens. During his lifetime, Farlow used to lament the impossibility, in his estimation, of bringing the two together, the herbarium having been located in the museum building, while the library, in the later years of his life, occupied a fireproofed addition to his own residence. The necessary books had to be carried back and forth between the two locations or else consulted separately. As in the case of the series of published specimens, Farlow sought out and purchased rare publications relating to his specialties, bought current periodicals and books, acquired separates, and all that were of interest or importance. His eye was keen over book catalogues, and his library was as complete as an expert with means at his disposal could make it. Farlow was extremely careful of his books and rarely could be induced to loan one, and consultation was chiefly in his study and under his own eye. In this way he kept his collection intact and uninjured. He was an omnivorous reader through the whole field of botany, keeping track, largely through the original articles, of progress in special fields—others as well as his own. His memory was exceedingly retentive, and he provided a fund of information to his students, his botanical visitors, and his correspondents.

Associated with the work of accumulating two such fundamental adjuncts to accurate work on the lower cryptogams as a satisfactory herbarium and an adequate library, came the matter of making both and the results of coordinated labor in the two available. That was accomplished by a series of indices. References and cross references were made, both in connection with the literature and the published specimens. The species were carefully attended to and their host plants (or animals) in case of the parasitic species. The synonymy, not only from the published data but from critical research, was carefully worked out. The result was not only indices to facilitate the work of Farlow himself, his students, and his correspondents, but for publication.

Several of these did come to the point of publication, such as a list, followed by a supplementary list, of works on North American fungi (1887 and 1888), a host index of the fungi of the United States (1888 and 1891), and finally the first part of the magnum opus, the Bibliographical Index of North American Fungi (1905), which included the fungi only as far as Badhamia, the rest remaining still in card form (approximating 350,000 references) awaiting funds to make it available to the many to whom it would be of the greatest benefit. These indices have been of inestimable influence in the work on North American fungi, both as to those published and those unpublished. Information and criticism founded on the data contained in them has been freely given, especially in correspondence, and has tended to keep down errors, unnecessary publication, and constructively to keep accuracy up to a high level. One of the greatest boons to our current work on fungi would be conferred by the publication of this last great index and adequate provision for its continuance from Farlow's farseeing and most admirable inception.

While the number of titles of the writings of William Gilson Farlow is ample, while the variety of topics he touched is very large, and while the new facts and considerations brought forward by him are very considerable, yet his critical knowledge of the various groups upon which he worked was so enormous and so detailed that we turn from what he has left us to that which we feel that he had to give with a sense of most serious loss. His very early publication on the apogamy in certain ferns was clearly a student publication, a happening in a laboratory where its importance was realized by an able instructor of wide experience. Farlow's main interest, however, did not lie in that direction and he did not follow up that lead, although he retained a deep interest in apogamy and related phenomena, as I well remember from experiences somewhat over a quarter of a century later while a student in his laboratory and in connection with pteridophyte apospory. His earliest papers concerned themselves with the marine algæ, taxonomic and critical, and these led up to what many of us, and it seems to me justly, consider his most characteristic and outstanding publication: viz, his account, really manual, of the marine algæ of New England and adjacent coasts. In arrangement, in content, and especially in critical and explanatory remark, this small volume is a model, refreshing, instructive, and intriguing to personal effort on the part of reader or student. Farlow's matchless humor and keen characterization show themselves again and again. For example, speaking of the common Leathesia, he notes that it is "sometimes called potatoes by the unromantic dwellers on the shore," or again, in speaking of a nomen nudum, *Callithamnion Tocwottoniensis* of Olney's list, which he says: "fortunately for printers and the throats of American algologists has never been described." It was one of Farlow's sincere desires that a new manual of New England algæ be prepared and issued, and the task fittingly devolved on Frank Shipley Collins, who had accomplished so much in that direction, but he, too, passed away without having completed the task.

Through his connection with the Bussey Institution and the turning of his attention from his favorites, the marine algæ, to what later came to be called plant pathology, or phytopathology, Farlow gave us the results of his work on certain species and groups of parasitic fungi. The potato rot and the grapevine mildew in particular led him to the Peronosporaceæ and their relatives, and his papers on these organisms were for long years authoritative. Onion smut, the black knot of cherry, and many miscellaneous plant diseases caused him to write other illuminating papers, but his chief attraction along these lines seemed to be the group of rusts, or Uredineæ, as they were called for so long a period. His pioneer paper on the Gymnosporangia led to a series of investigations, first, in the way of cultural studies by Thaxter and, later, by others, to determine their exact heteroecism. His critical notes on that troublesome question, synonymy, particularly vexatious in the group of the Uredineæ, and his notes on some species in the third and in the eleventh centuries of Ellis's North American Fungi (1883) are among his important contributions. He likewise elucidated and arranged the Synchitrium species of the United States. All these—Synchitria, Peronosporaceæ, Ustilagineæ, and Uredineæ—parasitic groups of fungi and of both biologic and economic interest, he touched but to adorn, and we feel bereft that out of his encyclopedic knowledge of these groups he did not find the opportunity to yield still more than he did in permanent form. We

feel that we might have expected, and with all propriety, one or even several monumental works such as the Bibliographical Index to North American Fungi, already alluded to, some monographs and revisions, but it was not to be. His index work, published and unpublished, numerous lists and occasional notes such as most of his later publications consisted of, are most grateful, but aggravating, as promise unfulfilled. His knowledge of the fleshy fungi was second only to that of his on the parasitic fungi, but we possess little of it. Even his collection of wonderful printed plates was not brought to publication. He is perhaps to be envied in that he leaves us in the position of Oliver Twist, asking hungrily for more with never an approach toward satiety.

Farlow's attitude toward general questions of a botanical or biological nature was largely expressed in conversation or in his public addresses, some of which have, fortunately for us, been printed. In conversation and formal address, he showed keenness of vision as well as great modesty, which he was inclined to cloak under pessimistic or sarcastic utterance. His classic statements, humorous or sarcastic, were generally the opening statements or used at times in the body of the address to suggest a "reductio ad absurdum." There has already been quoted in another account of Farlow an extract from his address before the American Association in 1905. In introducing his subject, which was entitled "The popular conception of the scientific man at the present day," Doctor Farlow says:

> What is or is not progress, depends, of course, upon the point of view. Some are so far ahead of the majority that they cannot see how much progress is made by those behind them. Others are so far in the rear that they cannot distinguish what is going on ahead of them. We must also admit that there are different directions in which progress can be made. You have all seen the agile crab, and been surprised to find how rapidly he gets over the ground, although he never seems to go ahead, but to scramble off sidewise. The crab perhaps wonders why men are so stupid as to try to move straight forward. It is a popular belief, but, not being a zoölogist, I cannot vouch for its correctness, that the squid progresses backward, discharging a large amount of ink. One might perhaps ask: Is the progress of science sometimes like that of the crab, rapid, but not straightforward; or, like the squid, may not the emission of a large amount of printer's ink really conceal a backward movement?

On another occasion, but at dinner and consequently informal, Farlow alluded to conventions and meetings and their purpose by relating the difficulties Mrs. Farlow experienced in obtaining eggs of the proper quality. On consulting with various dealers she was instructed and had trials of various grades, from "fresh," through "strictly fresh" to "newly laid" eggs. On inquiring as to how one could tell when eggs were newly laid she was informed, "by the cackle." Farlow then said, "How are we going to tell the newly laid discoveries at our meetings? The answer is, 'by the cackle.'" His attitude toward most of the newly announced discoveries was, as was natural to him in all things, skeptical. His address expresses this over and over again. On one occasion (Amer. Soc. Naturalists, 1886), he said:

> Probably a good many of my hearers have heard the remark, "I suppose you must make considerable out of your scientific papers." Unfortunately, with the exception of text-books of a lower grade, one is only too glad not to be money out of pocket. I fear that you all can bear witness that, with rare exceptions, your published papers have never paid for themselves. It is only after the results of research have reached a homœopathic dilution in some text-book or popular article that they begin to pay. Of such dilutions we already have an abundance, and the more important point is to get something new which will bear dilution. Unfortunately the public do not clearly see the difference between the original work and the dilution. The former does not pay, and needs encouragement; the latter is a commercial article having a recognized money value.

A characterization such as this is certainly definite and not by any means "out of order." It is matched by the graceful closing of the same address:

> But you will probably think that this paper is not like a ball of twine, which, however much it may be twisted and snarled, really has an end. There is much more I should like to say on the subject; as it is, I have tried to avoid particular specifications as to subjects of research, which would be interesting only to botanists, but to state broadly some of the difficulties in the way of botanical research, and to indicate the path which promises to be most favorable in the future. If my life proves to be as long as your patience, there will be plenty of opportunities hereafter to consider some points which I have been unable to touch upon today.

Aside from his witticisms, as such, and often in connection with them, Farlow presented his general ideas in the same clean-cut and pointed fashion in which his detailed work was done.

His clear outline of "The task of American botanists" in 1886, and his analysis of "Biological teaching in colleges," in the same year, and his humorous but searching characterization of "The popular conception of the scientific man at the present day" convey no less direct and profitable food for thought than his masterly and detailed treatment of "The conception of species as affected by recent investigations on fungi." It is from these published addresses that one may obtain some vivid and truthful ideas concerning the nature and work of the man who wrote them. If one may add, as many still living are able to, impressions from personal contact, informal conversations and talks at small dinners or in company, one may discount certain impressions of cynicism, pessimism, and sarcasm, and realize the kindliness yet clear vision of him, whom all those of us who did know him will love and revere.

As a conversationalist, Farlow was recognized as more than usually endowed with ready wit and repartee. The witticisms which characterize his public addresses were even more abundant and more pointed at times when the occasion called for them. To the bumptious or overgrateful person alike, his shafts struck directly and the conceited received short shrift at his hands. Yet he was ever gentle with the sensitive and, although really embarrassed, had extreme sympathy and desire to assist in the case of misfortune on the part of the truly deserving. He gave of his deep wells of information at times of friendly intercourse. Well do I remember being informally inducted into the history of the development of our knowledge of cryptogamic botany. This happened on the occasion of my more or less formal evening calls upon him in his rooms, then in Holyoke House. After a short call which I presumed would be agreeable to him and I rose to go, he would detain me, with my hand on the door knob, for an hour or more while he discoursed, almost in a monologue, on the personality, ancestry, botanical pedigree, and accomplishments of some distinguished botanist or botanists who had come up in our work. There was much of the unwritten history in these informal talks and food for thought as well as stimulus to further reading after I had finally been allowed to say my last adieu and depart, full of increased knowledge. At his dining club and elsewhere it was more or less a practice to bait Farlow, as it were, to bring out his ready and often biting repartee. It was a contest of some of the best wits of Harvard University, and Farlow is said usually to have borne away chief honors.

Farlow's letters were by no means the least of his influences exerted on behalf of what was best in cryptogamic work in the United States and even abroad. His correspondents seem to have been limited to those interested in any phase of cryptogamic botany. He was in constant interchange of views, literature, and specimens with practically all of the foreign cryptogamic botanists, while those at home had mostly been students with him or later in the cryptogamic laboratories at Harvard University. All difficulties, and particularly puzzles, were submitted to him, and while, at times, somewhat slow to answer, he generally replied briefly but to the point, giving much of his valuable time to this work, solely for the sake of assisting his friends or, possibly at times, to confound those of whose methods and work he could not approve. He must have written many thousand letters, with few exceptions in his own scrawly hand, and of which he, himself, was the severest critic. He did not accustom himself to a secretary or to a typewriter. In his experience were many extraordinary requests and he himself speaks feelingly (1887) of "the impecunious ignoramus who informs you that he is going to write a book, to include all the fungi of this continent, and coolly asks you to give or lend him all your books and specimens and tell him how to begin." While something definite is likely to have happened to this particular type of person, yet I have no doubt that if there were a grain of reasonableness to be discerned in such a character, Farlow would have recognized it and not have withheld such aid as he might be able to render. We have all fed upon the crumbs which dropped so plentifully from his well-filled larder and yet find ourselves unable to express our indebtedness and gratitude except in a few colorless words.

As a critic, Farlow was thorough and at times severe, but not intruding his criticism otherwise than called for by his duty to one of his students, nor unasked for. In his many reviews of particular papers or outlines of progress he was manifestly fair. He did not assume the rôle of mentor as Gray did occasionally in his later years. There is one review of Gray's, a rebuke of

some recent work in cryptogamic botany, in which the voice is the voice of Gray but the hand seems most likely to have been that of Farlow. He always warned us who were composing our youthful papers under his direction against too strong statements about any writer or his works. "Do not say," he often remarked, "that he is wrong or make use of any such direct expression, no matter what you think; simply quote him or his work with great respect and then show that he is thoroughly mistaken." This is a practice he always carried out in his own writings.

Farlow's influence as a builder up of unrivaled facilities for work in cryptogamic botany was supplemented by his influence on the teaching of this subject as well as on the teaching of botany in general. Few of us think of him as the founder of a pedagogical system and perhaps it is not possible to advance that claim; nevertheless, his methods and his viewpoints were so distinct, so analytic, and based so firmly on the psychologic aspects of both teacher and taught that he at least emphasized in botanical pedagogy a distinct and practically novel method. Farlow has given some of his ideas in his Sketch of Cryptogamic Botany at Harvard University from 1874 to 1896, and those who have access to a copy of this very interesting and instructive publication will do well to consider most carefully what he says. I suspect, however, that this privately printed document is not readily at hand to many, and I excuse myself for having quoted or abstracted many details from it. His other pedagogical disquisition, Biological Teaching in Colleges (1886), is readily accessible and much of his own attitude toward methodology is contained in it, with touches of his own personality which render it most illuminating.

It was my own good fortune to be associated with him as assistant (1888–1891) in the first part of what was called natural history 5 (botanical instruction being given the first half and zoological the second) and that, too, at a time when his ideas were fully developed. The first four plants used by him in this course were the distinctive features, since the training toward developing power in observing, recording, and inferring the structure and activity was the point laid stress upon. We began with a yeast cake, rock candy, and water. The rock candy was dissolved in water in a tall but slender glass cylinder and the yeast cake was then pulverized and added. The jar (or several of them) was placed in a warm place, usually on the window sill, where the student could look through it, and this was done several hours, or days even before it was to be used by the class, so that each cylinder might be evidencing proper activity. The details of preparation were announced to the class and they were asked to record in notes and drawings what they saw. The results, of course, are obvious, and the answers varied. The students were led by questions to distinguish their observations from their inferences. The plain facts of the rock candy being sugar and of the fermentation which most of them *saw* being an inference, as well as how they might, or might not, be able to demonstrate the truth of one or another inference, was brought out through questioning, objecting, and suggesting. Then the students were directed to make examinations with the compound microscope, using low power and then high powers, and to test with iodine and follow that with sulphuric acid of proper strength. Having listened to lectures on the cell and having heard that the "yeast plant" was concerned, all the students found cells, although usually their first finds were either air bubbles or starch grains. Many desired to know what they were to look for and seemed disappointed or even helpless when advised to determine, draw, and describe as many kinds of things as they might be able to distinguish in their preparations. After drawings were made, the students wanted names, but Farlow always suggested that they study each kind of object under each power of the microscope and under the influence of each reagent before coming to a conclusion. When the yeast cells were finally distinguished from the air bubbles, starch grains, and bacteria associated with them, they frequently proceeded to endow them with nuclei and even at times with chlorophyll. By the time the yeast exercise was completed most of the students had come to realize the manner of procedure and to distinguish "what they could see" from "what was purely a matter of inference."

After yeast came Spirogyra, the same care being exercised to emphasize method; and besides iodine and sulphuric acid, glycerin was applied to untreated filaments and also strong

alcohol. The students thus became acquainted with a vegetable cell, its wall, chromatophores, pyrenoids, and starch inclusions, the nucleus suspended in the center of the vacuole, and the primordial utricle, being induced to reason out each part and its structure by the "Yankee" method of answering one question by asking another. As a final test, each student was required to draw a diagram of a median longitudinal section of the Spirogyra cell. Thus the student was induced to infer the details of an object with three principal dimensions and portray it graphically. Spirogyra was followed by Nitella to show cyclosis, and a diagram of a median longitudinal section was also required to represent relation of layers from cell wall to center of a joint or tip cell. The final test of power to interpret solids came with the study of pine wood. First a transverse section was cut and mounted in balsam, so as to be properly cleared. This section was contrived so as to cover several annual rings. A careful drawing of this section was required, and the student was asked concerning his idea of the shape of the cells in pine wood, the answer usually being "square." He was also led to realize that there were several varieties of cells in the section and, by comparison with the microscopic view of the piece of wood whence the section had been cut, as to the direction of the center of the original tree, and consequently to distinguish spring wood, autumnal wood, and medullary rays. Most students were brought to the point of acknowledging that the only way to be certain about the shape of the cells would be to cut a longitudinal section. Over this would ensue a discussion as to what direction the longitudinal section must be cut, whether in any longitudinal direction relative to rings or rays or parallel to one or the other. The discussion in this connection, aided by suggestions as to consequences, led to the cutting of radial and tangential sections. About this time the student was frankly and thoroughly puzzled and at his wit's end as to how to match up three such different looking sections as those cut transversely, radially, and tangentially through coniferous wood. By directing attention through questions as to direction of center, occurrence, etc., the identification of the various kinds of cells and discoid markings was accomplished in all three sections. The final exercise, that of drawing in isometric projection the corner of a block of pine wood and matching the cell outlines, finally and emphatically completed the training in solid geometry and at least induced caution as to answering questions without careful consideration. After these several preliminary exercises the course proceeded to various selected plant types, from the simpler to the more complex, and the benefit of the preliminary training became apparent. The attack directed toward each problem was more straightforward, the reasoning more cautious and based on more actual observations, and the inferences drawn more logical.

Natural history 5, especially as to the first half, became nationally famous and one heard of it in various places and with differing comment. It was said that the instructor gave his students a razor, a microscope, and a broom handle and insisted upon a complete report. Many were the wild surmises and improbable hypotheses presented by the students, some received by Farlow with his inimitable chuckle, but all treated with respect and seriously argued. The instructor had need of ready wit and resource. The son of a distinguished member of Harvard University, after having ruined his best razor, told me in all solemnity that he considered that form of implement a very poor tool for cutting pine wood. The attitude of Farlow toward his students, especially beginners, but applying to all, was much more psychologically pedagogic than was usual in his time. It was something of the point of view of Louis Agassiz, but was more directive than his, so far as I may learn. He often said that if he were to live his life over again he would be a psychologist like "Willie" James because then he would not be compelled to bother to collect specimens everywhere and could dismiss them when through with studying them. His classification of students given in his naturalist address of 1886 is typical: Two classes, one of which was composed of individuals who wanted to be told what to see, and the other of those who knew so much (?) that they began to lecture on what they thought the specimen ought to show and who were led into extraordinary errors through their superficial training. The latter is the kind of student who, to use Farlow's own words, "called a hole in a cell wall a bioplast," and was highly pleased with his achievement until he was asked what a bioplast was. "The suggestion that a hole might without any great violence

to the English language be called a hole, was timely, if not pleasing." In quite another vein and yet to the same point, he said (The Task of American Botanists): "It is well to have our standard high but it should not be unattainable." "We may well set before our young men such models as De Bary, Sachs, Strasburger, and others; but it is just possible that a young man who is determined to be a De Bary, a Sachs, a Strasburger, or nothing, may have to adopt the latter alternative." "The trouble is, too many young men assume that the work they are destined to do is of the highest grade and they expect to be provided with all the refined apparatus and complete equipment which the leaders abroad possess." "They will not begin the simplest thing without an array of reagents which would be the envy of a good many chemists and the number of staining fluids which they must have around them would make the rainbow blush at its own poverty." "One young man thinks that he can not do any work because he has not a Jung microtome, another has been unable to do anything during a vacation at the seashore because he had no osmic acid. The botanist who declares that he can not do physiological work because he has not a large amount of apparatus would do well to recall the case of a Mr. Charles Darwin who published something on the power of movement in plants." His whole philosophy as to development of power rather than sponge capacity may be considered as being summed up in the sentence: "You can not make a boy a good mountain climber by carrying him up the Mount Washington Railway, no matter at how rapid a rate; and, in ordinary life, there are many mountains to be climbed, up which there is no railway."

As a lecturer, Farlow had a manner of his own. Incisive, yet coherent, with emphasis and yet not neglecting minor matters, glancing sidewards to discover the effect being made, biting the ends of his mustache when he paused to allow the effect of a rhetorical question to sink in. He usually began: "The subject of my lecture to-day is—by the way, are there any questions about the last lecture"; and when there were none, continuing, "I am pleased to see that you understood it so well." He was accustomed to emphasize his points by touching the desk in front of him with the outstretched forefinger of his right hand. He was more than successful in extracting the meat from a topic and laying it plainly before his hearers. He had a horror of extraneous details, although he said they often help. His classical illustration was of ergot. "Ergot," he told his class, "is a very interesting fungus. By the way, it grows about here in the flowers of the wild rye on the banks of the Charles River," going on to describe its characters, etc. On examination, asking about ergot, he received the reply: "Ergot is a plant growing on the banks of Charles River."

With advanced students and those studying for higher degrees his methods were, of course, different, but he always used the question method, answer and rebuttal following. He could ask the most searching questions, taking the wind completely out of the sails of the over-confident and reducing superficial conclusions from a turgid condition to that of complete collapse. He never assumed an authoritative tone himself, but always expressed a conclusion tentatively and often interrogatively, unless it were negative, in which case he was often most decisive. I remember well his statement as to the claims of a botanist who had distributed a number of sterile specimens of a critical genus of the green algæ, claiming, when remonstrated with, the ability to determine such specimens, whether other botanists could or not. "One may not be able to say definitely whether such sterile specimens are undoubtedly of a certain species," said he, "but one can say what they are not, and the specimens distributed certainly do not belong to the species whose names are on these labels." In the first work of research I attempted with Farlow it was necessary to compare the structure of an alga (Tuomeya) with which I was at work with that of the type specimen. As Farlow possessed only a wee fragment of the type, I could take only one slice from it, and I was compelled to make a section of my material which corresponded exactly with that slice before he would allow satisfactory identity. I finally succeeded, but it cost me nearly a week's time to obtain that identical section. Farlow could find more flaws and raise more objections than any other instructor with whom I ever came into contact, but when he finally did approve there was the satisfaction that little further destructive criticism could be directed against it. On this account, the writ-

ing of a paper under Farlow's supervision was an experience long to be remembered, but also an experience worth while. Every sentence was discussed, both as to the truth of the statement and the way in which this truth might be conveyed.

Farlow's influence on the teaching and research of botany is by no means confined to the cryptogamic side, although most of his activity belongs there. His example, in its manifold excellence, penetrated to many fields not peculiarly his own. By the time of his death he had become the Nestor of American botanists, and his appearance at the annual meetings was always hoped for and thoroughly appreciated when he could attend. His words of wisdom, his witty remarks, his rare addresses, and his after-dinner speeches were events. In Cambridge he received and entertained visiting botanists so that his home became a veritable Mecca to those seeking counsel and consolation. He was welcomed into all American societies to which he was eligible. He was elected a member of the National Academy of Sciences in 1879. He was elected president of the American Association for the Advancement of Science and of the Botanical Society of America. He was a corresponding member of various societies and associations of England, France, Germany, and Italy; in fact the list of his honors in this direction is long and varied, even for a distinguished member of Harvard University.

Besides the degrees of B. A., M. A., and M. D., in course, Harvard University conferred the degree of LL. D. in 1896. The University of Glasgow in 1901 and that of Wisconsin in 1904 conferred upon Farlow the same degree and the University of Upsala that of Ph. D. in 1907, on the two hundredth anniversary of the birthday of Linnæus. Many species were named in his honor and at least two genera. He died full of honors, revered and respected by his colleagues and sincerely mourned by his former students and his friends. I may be allowed, in closing, to quote the final paragraph entered on the minutes of the faculty of arts and sciences of Harvard University, on December 2, 1919, as a fitting epitaph:

> A pioneer, a cultivated and learned man of wide influence, a stimulating teacher and keen investigator, a loyal friend, Dr. Farlow was original, versatile, conscientious, modest, sympathetic, and generous; with him has passed from the Harvard group of scholars a unique personality.

I desire to make grateful acknowledgment to Mrs. William G. Farlow, Prof. Roland Thaxter, and Mr. A. P. D. Piquet for assistance and suggestion. I have obtained material and inspiration from the following biographical notices and resolutions:

THAXTER, ROLAND, WINTHROP J. V. OSTERHOUT, and THEODORE W. RICHARDS.
Faculty of Arts and Sciences. Minute on the life and services of Prof. William Gilson Farlow. Harvard Univ. Gazette, 15 : 60, Dec. 13, 1919.

THAXTER, ROLAND. William Gilson Farlow. Harvard Graduates Magazine: 269, Dec. 1919 (with portrait).

——— ——— William Gilson Farlow. Bot. Gazette, 69: 83–87. Jan. 1920 (with portrait).

CLINTON, G. P. William Gilson Farlow. Phytopathology, 10 : 1, Jan. 1920 (with portrait).

BLAKESLEE, A. F., ROLAND THAXTER, and WILLIAM TRELEASE. William Gilson Farlow. Amer. Journ. Botany, 7: 173, May 1920 (with portrait and bibliography).

RIDDLE, L. W. William Gilson Farlow. Rhodora, 22 : 1, Jan. 1920 (with portrait).

PUBLICATIONS

The following list of Doctor Farlow's publications was prepared from memoranda furnished by Mr. A. P. D. Piquet and is as nearly complete as it has been possible to make it except that none of his numerous reviews of books and articles have been included. This list was published by Blakeslee, Thaxter, and Trelease in connection with their notice of Doctor Farlow's life in the American Journal of Botany for May, 1920.

1871. Cuban seaweeds. Amer. Nat. 5 : 201.

1872. Marine algæ. Proc. Boston Soc. Nat. Hist. 14 : 64.

1873. List of the seaweeds or marine algæ of the south coast of New England. Rept. U. S. Fish Comm. 1871–2 : 281.

1874. Notes from the journal of a botanist in Europe. I. Amer. Nat. 8 : 1. II. Amer. Nat. 8: 112. III. Amer. Nat. 8: 295.

An asexual growth from the prothallus of *Pteris cretica*. Bot. Zeit. 32 : 181.

The same. Quart. Journ. Micr. Sci. II, 14 : 266.

An asexual growth from the prothallus of *Pteris serrulata*. Proc. Amer. Acad. Arts and Sci. 9 : 68.

1875. List of the marine algæ of the United States, with notes of new and imperfectly known species. Proc. Amer. Acad. 10 : 351.
The potato rot. Bull. Bussey Inst. 1 : 319–338.
1876. Gustave Thuret. Journ. Bot. 14 : 4.
Fungi heaped up in pines by squirrels. Amer. Nat. 10 : 112.
On a disease of olive and orange trees occurring in California in the spring of 1875. Bull. Bussey Inst. I : 404. Abstr. in Journ. Bot. 14 : 287.
The same. Monthly Micr. Journ. 16 : 111.
The same. Amer. Journ. Sci. III, 12 : 37.
On the American grapevine mildew. Bull. Bussey Inst. I : 415.
Synopsis of the Peronosporeæ of the United States. Bull. Bussey Inst. I : 426.
List of fungi found in the vicinity of Boston. Bull. Bussey Inst. I : 430.
The black knot. Bull. Bussey Inst. I : 440.
University instruction in botany. Amer. Nat. 10 : 287.
Spores of *Blodgettia confervoides*. Amer. Nat. 10 : 428.
Algæ : in Contributions to the natural history of Kerguelen Island. Part II. Bull. U. S. Nat. Mus. 3 : 30
List of the marine algæ of the United States. Rept. U. S. Fish Comm. 1875 : 1.
Algæ: in Report on a peculiar condition of the water supplied to the city of Boston 1875–76, by Professor Nichols, Dr. Farlow, and Mr. Burgess. Rept. Cochituate Water Board, Boston 1876 : 10.
1877. Remarks on some algæ found in the water supplies of the city of Boston. Bull. Bussey Inst. 2, pt. 1 : 75.
Botany; pp. CLXXV–CLXXX, in Annual Record of Science and Industry for 1876.
Notes on some common diseases caused by fungi. Bull. Bussey Inst. 2 : 106.
On some algæ new to the United States. Proc. Amer. Acad. 12 : 235.
Onion smut. Rept. Mass. Board Agr. 1876, pt. 2 : 164.
Report on matters connected with the Boston water supply. Rept. Water Board, Boston 1877 : 4.
1878. On the synonymy of some species of Uredineæ. Proc. Amer. Acad. 13 : 251.
Diseases of fruit-bearing trees. Rept. Mass. Board Agr. 1877 : 218.
1879. Botany: in Annual Record of Science and Industry (editor S. F. Baird) 1878. Also in other volumes.
Diseases of forest trees. Trans. Mass. Hort. Soc. 1879 : 44.
List of algæ collected at points in Cumberland Sound during the autumn of 1877. Bull. U. S. Nat. Mus. 15 : 169.
The sea weeds of Salt Lake. Preliminary report. Amer. Nat. 13 : 701.
1880. On the nature of the peculiar reddening of salted codfish during the summer season. Rept. U. S. Fish Comm. 1878 : 969.
The Gymnosporangia or cedar apples of the United States. Anniversary Memoirs Boston Soc. Nat. Hist. pp. 1–38.
On some impurities of drinking water caused by vegetable growths. Rept. Mass. Board of Health, Lunacy, and Charity, 1 : suppl. 131.
1881. Unusual habitat of a Coprinus. Bull. Torrey Club. 8 : 67.
Note on Laminariæ. Bull. Torrey Club. 8 : 67.
Notes on Gymnosporangia. Bull. Torrey Club. 8 : 85.
In Burnett, S. M.: *Otomyces purpureus* (Wreden) in the human ear. Archives of Otology, 10 : 324.
In Remsen, I.: Report on a peculiar condition of the water of Boston in November, 1881. City of Boston, Document 143 (1881) : 15.
An account of recent progress in botany (for the years 1879 and 1880). Smithsonian Rept. 1880 : 313.
1882. The marine algæ of New England and adjacent coast. Rept. U. S. Fish Comm. 1879 : 1–210. Separates publ. in 1881.
American grape mildew in Europe. Bot. Gaz. 7:30.
Grape mildew. Bot. Gaz. 7:42.
Notes on New England algæ. Bull. Torrey Club. 9:65.
Swarm spores of Closterium. Amer. Monthly Micr. Journ. 3:118.
1883. Notes on fresh-water algæ. Bot. Gaz. 8:224.
Notes on some species in the third and eleventh centuries of Ellis's North American Fungi. Proc. Amer. Acad. 18:65.
Note on *Phallus togatus*, Kalchb. Bot. Gaz. 8:258.
Cryptogams; in Watson, S.: List of plants from southwestern Texas and northern Mexico, collected chiefly by Dr. E. Palmer in 1879–80. Proc. Amer. Acad. 18:190.
Notes on some Ustilagineæ of the United States. Bot. Gaz. 8:271.
Additional note on Ustilagineæ. Bot. Gaz. 8:318.
Botrytis Rileyi Farlow; in Riley, C. V.: Report of the entomologist. Rept. U. S. Dept. Agr. 1883:121.
Enumeration of the Peronosporeæ of the United States. Bot. Gaz. 8:305, 327.
An account of progress in botany in the year 1881. Smithsonian Rept. 1881:391.

1884. Notes on the cryptogamic flora of the White Mountains. Appalachia 3:232.
Additions to the Peronosporeæ of the United States. Bot. Gaz. 9:37.
Maladies des morues sèches. Rev. Mycol. 6:197.
The spread of epidemic diseases in plants. Proc. Amer. Assoc. Adv. Sci. 32:307.
An account of the progress of botany in the year 1882. Smithsonian Rept. 1882:551.
Notes on a fungus parasitic on a species of Potamogeton. Trans. Ottawa Field Naturalists' Club 2:127.
1885. Notes on Fungi. Bot. Gaz. 10:219.
The Synchytria of the United States. Bot. Gaz. 10:235.
A new locality for Nelumbium. Bull. Torrey Club. 12:40.
Notes on some injurious fungi of California. Bot. Gaz. 10:346. Also in Proc. Amer. Assoc. Adv. Sci. 34:300 and Proc. Soc. Prom. Agr. Sci. 1885:29.
An account of the progress of botany in the year 1883. Smithsonian Rept. 1883:681.
Lower Cryptogamia; in Ray, Lieut. P. H.: Report of the international polar expedition to Point Barrow, Alaska 1885:192.
Notes on some species of Gymnosporangium and Chrysomyxa of the United States. Proc. Amer. Acad. 20:311.
1886. Botany at Harvard. Bot. Gaz. 11:43.
Biological teaching in colleges. Pop. Sci. Monthly 28:577.
The Brothers Tulasne. Bot. Gaz. 11:93.
Nostoc group (Phycochromaceæ). Bot. Gaz. 11:149.
White Mountain Floras; in "The Appalachians." White Mountain Echo 9:12.
Yeast (Saccharomycetes). Bot. Gaz. 11:150.
Development of Roesteliæ from Gymnosporangia. Bot. Gaz. 11:189.
The development of the Gymnosporangia of the United States. Bot. Gaz. 11:234.
Puccinia malvacearum Mont. in Massachusetts. Bot. Gaz. 11:309.
On a supposed disease of roses caused by a fungus. Proc. Soc. Prom. Agr. Sci. 1886:233.
Notes on Arctic algæ, based principally on collections made at Ungava Bay by Mr. L. M. Turner. Proc. Amer. Acad. 21:469.
1887. The task of American botanists. Pop. Sci. Monthly 31:305.
Vegetable parasites and evolution. Bot. Gaz. 12:173. Also Proc. Amer. Assoc. Adv. Sci. 36:233 (1888).
H. W. Ravenel. Bot. Gaz. 12:194.
(With Trelease, W.) List of works on North American fungi, with the exception of Schizomycetes, published before 1887. Harvard Univ. Libr. Bull., nos. 37–38 (Bibliographical Contributions No. 25), with Supplement: Fungi Exsiccati Am.–septentrionalis.
Vegetable parasites of codfish. Bull. U. S. Fish Comm. 6:1.
Aecidium on *Juniperus Virginiana*. Bot. Gaz. 12:205.
1888. Asa Gray. Bot. Gaz. 13:49.
Apospory in *Pteris aquilina*. Annals of Botany 2:383.
A curious vegetable growth on animals. Garden and Forest 1:99.
Tubercles of leguminous roots. Garden and Forest 1:135.
Fungus diseases of insects. Garden and Forest 1:159.
The cultivation of truffles. Garden and Forest 1:194.
Apical growth in Fucus. Proc. Amer. Assoc. Adv. Sci. 36:271.
Algæ and fungi; in Enumeration of the plants collected by Dr. H. H. Rusby in South America 1885–1886. Bull. Torrey Club 15:183.
Anton de Bary. Garden and Forest 1:15.
Siphoptychium casparyi, Rfski. Journ. Mycol. 4:82.
Memorial of Asa Gray. Proc. Amer. Acad. 23:321.
Remarks on the collections of lichens belonging to the Boston Society of Natural History. Proc. Boston Soc. Nat. Hist. 23:274.
Notes on fungus diseases in Massachusetts in 1888. Proc. Soc. Prom. Agr. Sci. 1888:25.
A supplemental list of works on North American fungi. Harvard Univ. Library Bibliographical Contributions no. 31.
Asa Gray. Ber. Deutsch. Bot. Gesell. 6:XXXI.
(With Seymour, A. B.) A provisional host index of the fungi of the United States. Part I, Polypetalae. Cambridge, Mass.
1889. On some new and imperfectly known algæ of the United States. I. Bull. Torrey Club 16:1.
White huckleberries. Garden and Forest 2:50.
Leaf spots on greenhouse plants. Garden and Forest 2:66.
Appendix to the paper on the life history of *Macrosporium parasiticum*, Thüm. by Kingo Miyabe. Ann. Bot. 3:26.
Notes on fungi. Bot. Gaz. 14:187.
Miles Joseph Berkeley. Garden and Forest 2:410.

1890. (With Seymour, A. B.) A provisional host index of the fungi of the United States. Part II. Cambridge, Mass., 1890.
Poisonous action of *Clathrus columnatus*. Bot. Gaz. 15:45.
Curious case of germination in *Citrus decumana*. Bot. Gaz. 16:179.
Leo Lesquereux. Proc. Amer. Acad. 25:320.
Botany at the University of Montpellier. Garden and Forest 3:378.
1891. Karl Wilhelm von Naegeli. Proc. Amer. Acad. 26:376.
Diseases of trees likely to follow from mechanical injuries. Trans. Mass. Hort. Soc. 1891: 140.
(With Seymour, A. B.) A provisional host index of the fungi of the United States. Part III. Cambridge, Mass., 1891.
1892. Dr. Mayr on the parasitic fungi of North American forest trees. Garden and Forest 5:37.
Notes on collections of cryptogams from the higher mountains of New England. Proc. Boston Soc. Nat. Hist. 25:387.
Notes on fungi; in Sargent, C. S.: The Silva of North America. Under fifty-six host genera, 1891–1898.
Diseases of mushrooms. Garden and Forest 5:590.
1893. The rose of Jericho. Garden and Forest 6 : 23.
The ginger-beer plant. Garden and Forest 6:50 (editorial).
Notes on some algæ in the herbarium of the Long Island Historical Society. Bull. Torrey Club 20:107.
White huckleberries. Garden and Forest 6:363. Also in Agr. Sci. 7:368.
Alphonse de Candolle. Proc. Amer. Acad. 28:406.
1894. Notes for mushroom-eaters. Garden and Forest I, 7:32; II, 7:43; III, 7:52; IV, 7:63; V, 7:72; VI, 7:82.
1895. Note on *Agaricus amygdalinus*, M. A. Curtis. Proc. Boston Soc. Nat. Hist. 26:356.
Mimicry of fungi in insects. Bot. Gaz. 20:547.
Memoir of Edward Tuckerman. Nat. Acad. Sci. Biographical Mem. 3:17.
Biographical memoir of Asa Gray. Nat. Acad. Sci. Biographical Mem. 3:161.
Thallophyta and Bryophyta; in Bryant, H. G.: The Peary auxiliary expedition of 1894. Bull. Geogr. Club Philadelphia I, Append. C:208.
1896. Fungi; in Rusby, H. H.: On the collections of Mr. Miguel Bang in Bolivia. Part III. Mem. Torrey Club 6:130.
A sketch of cryptogamic botany at Harvard University, 1874–1896. Cambridge, Mass., 1896.
1897. Botany; in The Smithsonian Institution 1846–1896, the history of its first half century. G. B. Goode, editor. Washington, 1897.
Poisonous mushrooms. Garden and Forest 10:467.
Algæ; in Trelease, W.; Botanical observations in the Azores. Rept. Mo. Bot. Garden 8:188, 195.
1898. Botany: Marine algæ; in Grabau, A. W., and Woodman, J. E.: Guide to localities illustrating the geology, marine zoology, and botany of the vicinity of Boston. A. A. A. S. P. 97. Fiftieth Anniversary Meeting.
Basidiolichenes and Basidiomycetes; in Hitchcock, A. S.: List of crytogams collected in the Bahamas, Jamacia, and Grand Cayman. Rept. Mo. Bot. Gard. 9:115.
Some edible and poisonous fungi. Bull. U. S. Dept. Agric., Div. Veg. Phys. and Pathol. 15: 453; also in Yearbook U. S. Dept. Agric. 1897:453.
The conception of species as affected by recent investigations on fungi. Amer. Nat. 33:675; also in Proc. Amer. Assoc. Adv. Sci. 47:383.
1899. Three undescribed Californian algæ. Erythea 8:73.
Poisoning by *Agaricus illudens*. Rhodora 1:43.
1900. Address of the president before the American Society of Naturalists, at New Haven, Conn., 28 Dec., 1899. Science n. s. 11:11.
Botanical bibliography. Bot. Gaz. 29:64.
1901. (With MacDougal, D. T., and von Schrenk, H.) Report of the committee on securing better reviews of botanical literature. Pamphlet.
1902. Fungi; in Delabarre, E. B.: Report of the Brown–Harvard expedition to Nachvak, Labrador, in the year 1900. Bull. Phila. Geogr. Soc. 3:201.
Thallophytes and Musci; in Robinson, B. L.: Flora of the Galapagos Islands. Proc. Amer. Acad. 38:82, 102.
Boletus betula and *B. Russelli*; in Lloyd, C. G.: Mycological notes, no. 11.
Hypocrea alutacea; in Lloyd, C. G.: Mycological notes, no. 11.
1905. Bibliographical index of North American fungi, vol. 1, pt. 1, pp. xxxiv+312. Carnegie Institution of Washington, Publ. 8.
Myxomycetes; in Coker, W. C.: Vegetation of the Bahama Islands (Special publ. from The Bahama Islands, G. B. Shattuck, editor). Geogr. Soc. Baltimore, p. 242.
1906. The popular conception of the scientific man at the present day. Science n. s. 23:1.
List of works of Job Bicknell Ellis. Issued August, 1906 (From list of works on North American fungi, revised edition).
1908. Notes on fungi I. Rhodora 10:9.

1910. Jane Loring Gray. Rhodora 12:41.
(With Atkinson, G. F.) The botanical congress at Brussels. Science n. s. 32:104; also in Bot. Gaz. 50: 220.
A consideration of the Species Plantarum of Linnæus as a basis for the starting point of the nomenclature of cryptogams. Amer. Nat. 44:385.
1912. Notes on the chestnut blight. The Pennsylvania Chestnut Blight Conference, p. 70.
The fungus of the chestnut tree blight. Science n. s. 35: 717.
1913. The change from the old to the new botany in the United States. Science n. s. 37:79.
1914. The vegetation of the Sargasso Sea. Proc. Amer. Philos. Soc. 53:257.
Sir Joseph Dalton Hooker. Proc. Amer. Philos. Soc. 53: Minutes, p. xv.
1916. *Rhododendron maximum* in New Hampshire. Rhodora 18:25.
The marine algæ of the Pacific. Proc. Nat. Acad. Sci. 2:424.
Montia sibirica in Massachusetts. Rhodora 18:240.
Jean Baptiste Edouard Bornet. Proc. Amer. Acad. 51:852.

1877–1889. (With C. L. Anderson and D. C. Eaton.) Algæ Exsiccatæ Americæ Borealis.
Fascicle I. Nos. 1–50, 1877.
Fascicle II. Nos. 51–100, Apr. 1878.
Fascicle III. Nos. 101–130, 1879.
Fascicle IV. Nos. 131–180, June, 1881.
Fascicle V. Nos. 181–230, July, 1889.

G K Gilbert

NATIONAL ACADEMY OF SCIENCES

Volume XXI
FIFTH MEMOIR

BIOGRAPHICAL MEMOIR GROVE KARL GILBERT
1843-1918

BY

WILLIAM M. DAVIS

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1922

TABLE OF CONTENTS

ILLUSTRATIONS

GROVE KARL GILBERT

By William M. Davis

CHAPTER I

GILBERT'S ANCESTRY AND YOUTH

FOREWORD

Gilbert gained an exceptional place in the esteem of his colleagues by his appeal alike to their intellect and their affection. He was penetratingly successful in his search for external facts as well as for their mental interpretation. He manifested an extraordinary capacity in the critical analysis of many factors, with patience and impartiality in the just consideration of every one. His published reports inspired confidence, for they were completely free from special pleading, and they were, moreover, presented so clearly, so intelligently, as to satisfy the reader that the observations on which they were based must be full and accurate, and that the conclusions to which they led must be well grounded and trustworthy. He set forth the truth as he found it, and once having had his say he refrained from entering into contentious disputes to maintain his views. While he was always generous in accrediting the work of others, he never demanded that his own work should be similarly recognized; indeed, his silence in this respect reached self-sacrifice. It was from no urgency or insistence on his part that his opinions were adopted, but from the persuasively convincing logic with which they were set forth. It was his habit in presenting a conclusion to expose it as a ball might be placed on the outstretched hand—not gripped as if to prevent its fall, not grasped as if to hurl it at an objector, but poised on the open palm, free to roll off if any breath of disturbing evidence should displace it; yet there it would rest in satisfied stability. Not he, but the facts that he marshaled, clamored for the acceptance of the explanation that he had found for them.

He was always ambitious to do good work, but he never strove for office or for position. The nearer one lived to him and the longer one knew him the clearer it became that his personal nature was as exceptional as his scientific capacity; for in his private life as in his geological tasks he was fair-minded, self-controlled, serene; gentle in his manner, simple in his ways, uncomplaining under trials and disappointments, loyal to his duties, steadfast in his friendships. Little wonder that those already old when he was young should have recognized in him one who would continue the work they had begun and carry it forward into regions of space and of thought they had never entered; or that those still young when he was old should have looked upon him with respect akin to awe as one who, surviving from an heroic age when a western frontier remained to be explored, had discovered there many of the facts and principles which they, on entering geological science, had found embedded in its foundations; or that those of his own generation should have watched and admired his scientific progress as he overcame one problem after another, and have gratefully rejoiced in the ever-increasing recognition of his merit as he advanced from excellence to eminence; or that those favored ones among his contemporaries who lived near him and who had in their own tasks the guidance of his wise counsel and the encouragement of his never-failing sympathy through a long unbroken comradeship, should have esteemed the man himself above his works. Unknown to the multitude, he was the source of an ever-widening current of scientific thought that flowed out to earnest men over all the breadth of our country and beyond. His career covers a remarkable epoch in American science, and it is a great credit to American science that it should have given high rank to a man of his gentle personality.

THE GILBERT FAMILY

Grove Karl Gilbert was born in Rochester, N. Y., May 6, 1843, the son of Grove Sheldon and Eliza Stanley Gilbert. On both the paternal and maternal sides, his forbears were of New England origin. An old record states that John Gilbert, jr., "a brave and honest gentleman," came to America in 1630 and settled with his wife and sons at Dorchester, Mass. One of his descendants, bearing again, a century and a half later, the name of the brave and honest gentleman of 1630, lived in Connecticut and was an officer in the Revolutionary War; he married Theodosia Marsh, and died at Little Falls, N. Y., in 1795. The sixth of his seven children, born in New Hartford in 1782, received the ancestral name and was the grandfather of Grove Karl Gilbert. Ten years after his father's death, this third John Gilbert was established as an ax and tool maker at Clinton, N. Y., and there in 1803 he married Eunice Barnes, daughter of an ingenious bell and clock maker of the same town. He moved in 1811 or 1812 to Le Roy, where in 1824 he invented a rotary steam engine, which was regarded as so promising as to give hope of profitable manufacture; but after going to New York City to develop the machine, he there fell victim to an epidemic of typhoid fever and died April 6, 1825, without having made progress in his plans; thus briefly is recorded the tragic end of a worthy life.

Grove Sheldon Gilbert, son of the third John Gilbert, was born at Clinton, August 5, 1805, and on his father's death became, at the age of 19, the "head of the family." In the same town lived Thaddeus and Betsy Doud Stanley and their 12 children. The father, born at Goshen, Conn., in 1769, was a quiet, industrious citizen, a cooper by trade, much loved and respected; he died in 1843. One of his daughters, Eliza, became on November 30, 1826, the wife of Grove Sheldon Gilbert. The young man had studied medicine and taught school, and for a time the pair had a shifting residence; but most of their married life was spent in Rochester, N. Y., where the husband established himself as a self-taught portrait painter. His success was moderate and hardly yielded him a competence. He strove earnestly in his profession, painting for art rather than for fame, but did not reach his ideals; yet after reluctantly sending, at the urgent request of some of his friends, one of his portraits to an exhibition held by the National Academy of Design in New York in 1847 he was, much to his surprise, elected to honorary membership in that body in the following year.

Grove Karl Gilbert was the fifth born and the fourth son in a family of seven children, only three of whom, an older brother and sister and Karl himself, lived to maturity. His little home in Rochester, known as the "Nutshell," was one of small means and high principles. About the time of Karl's birth, his father and mother both withdrew on account of some deep religious conviction from the Presbyterian Church with which they had been previously connected, and joined no other, even though lack of church membership left them socially isolated in a community of churchgoers. Karl was therefore never sent to Sunday school and did not receive what the Orthodox would have considered proper religious instruction, nor did he form the habit of attending church; but his father was of a deeply religious nature and talked much and earnestly on religious matters with his children. In so far as the son's pure and serene character came from home teaching, his parents must have been well satisfied.

BOYHOOD IN ROCHESTER

But who shall say how far character is affected by early teaching and how far it is inborn? Scientific tastes do not seem to have been especially encouraged by Karl's home influences, yet close observation as a first step in their development was begun early; for when still a little fellow, 5 or 6 years old, he ran in from the garden one morning, calling in excitement to his father: "The onions are up!" The father went out to see them, and finding no onions in sight turned to chide the boy for telling what was not true; but little Karl insisted until his father, kneeling down to see better, detected the minute sprouts hardly above the soil.

Other stories of Karl's boyhood preserved among family memories show a gentleness and forbearance that were characteristic of the man in later life. Once when his classmates were crowding out from a schoolroom, he was pushed down a flight of stairs, and although not seriously hurt by the fall he fainted and much excitement followed. On his return to

consciousness the teacher inquired who had pushed him; Karl did not know. Others were then asked the same question, but Karl objected: "Please, sir, don't find out; I don't want to know who did it." "Why not?" said the teacher. "Because I am afraid I should not like him any more." Again, one winter day while the family was living at Irondequoit, about 3 miles out of Rochester, Karl went skating, leaving only his sister at home in the absence of both parents; he returned earlier than was expected, and the sister did not learn until years afterwards that Karl had broken through the ice and after some difficulty in getting out and ashore had gone to a near-by house, where clothes were lent him while his own were drying; he had said nothing of all this on returning home.

There is no reason to believe that the boy made any record of this adventure at the time, but much more of it than the cold plunge remained in his retentive memory, as appears from a letter that he wrote nearly half a century later—in April, 1901—to the author of an essay on the work of ice in lakes:

In my boyhood I made an observation on the behavior of lake ice which I have never had an opportunity to repeat since I came to have scientific interest in it. . . . Near my old home at Rochester, N. Y., there is a narrow bay [Irondequoit] separated by a bar from Lake Ontario so as to constitute practically a lake. The sides are so irregular that the width varies from ½ to 1 mile and the length is perhaps 3 miles. One cold day, in skating the length of this bay, I found the ice to be divided by open cracks extending from side to side of the bay. There were perhaps a dozen of them in the whole distance. I do not know the thickness of the ice but it was thick enough to be entirely firm. . . . The cracks were several feet wide as I am able to assure myself by certain details of the day's experience. The wind was blowing and a little snow was drifting. This was caught by the water of the cracks so that each one was marked by a line of wet snow. Usually the line of snow was so well frozen as to bridge the crack and enable me to glide over it, but in some cases, after testing with my shinny stick, I did not venture to skate across, but retreated so as to acquire speed, making a flying jump. In another case I trusted unwisely to the ice and fell through. These two facts probably indicate [an] extreme width of crack of from 3 to 5 feet. . . . My recollection that the day was cold is supported by the fact that after I had fallen in, my clothes were frozen before I reached the nearest house. The theoretical interest in the phenomena arises from the fact that the ice sheet as a whole seems to have been shortened by cooling. . . . In the direction across the bay the ice could shrink horizontally by merely drawing away from the shore, but lengthwise of the bay it could not shrink in the same manner because held by the irregularities of the lateral coasts. The shortened length therefore found expression in the cracks.

This is a remarkably fine example of mature reflection superposed on the vivid recollection of a youthful experience; and how delightful it is to think of Gilbert as a boy skating through the wind and making his flying leaps over the cracks in the ice on Irondequoit Bay.

On another occasion, while the family was still living at Irondequoit, Karl's father was ill, and his mother, alarmed by a serious turn in her husband's condition in the night, called Karl and told him to hurry to the city for the doctor. He set out at once and returned so soon that his mother on seeing him exclaimed: "Why Karl, haven't you started yet?" The boy had run most of the way to the doctor's house and back, about 6 miles, returning with medicine and advice before his mother knew he had set out.

Karl must have been a studious boy, for when he was only 10 years old, his father received a most gratifying report from the boy's teacher, who polysyllabically said: "His deportment has been unexceptionable, and he has been a most faithful, industrious, and attentive student, meriting in every way my highest approbation." His acquisitiveness and his memory must have been as good as his deportment, as appears from a letter written to one of his sons from the coast of Massachusetts in August, 1912:

Nothing newer here than that I saw some Mother Carey's chickens this morning, the first I have ever seen. They have a peculiar trick of touching the water with their toes as tho running on it, and I remember to have seen a picture showing the trick when I was a small kid absorbing information from the Penny Magazine.

This reminiscence is followed by a little sketch, not showing the birds he had seen that morning but reproducing by memory the picture he had seen some 60 years before.

Although an exemplary scholar, Karl was also fond of boating, for which good opportunity was offered by the Genesee River in its course near Rochester. He and a companion built several small craft for rowing; one boat of flat-bottom model, called the *Wave*, was so light that it could be "held out at arm's length with one hand." A regatta was planned in 1859, when the

launching of the *Great Eastern* steamship in England was the topic of the hour; and the *Wave*, rechristened the *Great Western* with that spirit of exaggeration which laughs at itself, was entered in the lists in competition with canoes and other boats of professional build; but the owners of these conventional models, having seen Karl practicing, withdrew their entries and he rowed over the course alone. The competitors of the home-made *Great Western* are even remembered to have made the unexpurgated remark: "The damned little thing *can* go." Expertness on the water thus early gained served Gilbert well during western exploration in later years, and he always enjoyed canoeing for exercise. It is said that when Wheeler's exploring party, of which Gilbert was geologist, ascended the Colorado River in 1871, as will be told on a later page, the Indian guides recognized Gilbert's skill as a boatman and thought his boat the safest of the fleet.

But as in the case of his adventure on the ice of Irondequoit Bay, it was not only physical expertness that came from his early excursions; while he was boating on the Genesee his observant mind was stimulated to a reflective activity and even to simple experimental research, which he recalled in a letter to a geological correspondent 40 years later:

> When I was a boy I noticed that by rocking a skiff I gave it a forward motion. That led to the trial of other impulses, and I found that by standing near the stern and alternately bending and straightening my legs, so as to make the skiff rock endwise, I could produce a forward velocity of several yards a minute. If I stood on one side of the medial line, the skiff moved in a curve. The motions I caused directly were strictly reciprocal, the departures from initial position being equaled by the returns. The indirect result of translation was connected with reactions between the water and the oblique surfaces of the boat.

It is characteristic of keen minds to take account of small matters that are unnoticed by the mentally sluggish.

Karl completed the course at the Rochester High School in 1858 and was then, at the age of 15, overtall for his years, thin chested and somewhat delicate, in spite of his prowess on the river; but schoolmates as well as teachers knew him as an earnest and successful student, who did well whatever he undertook. A companion in those years recalls him as "a quiet, modest boy, with pleasant manners, kindly disposition, a lively sense of fun, and of very even temper." Long afterward, when one of his sons asked what he did as a boy, he replied: "I studied a good deal when not working"; from which it may be inferred that no small share of family duties fell upon him at home. He recalled half a century later that his father, wishing to test his capacity in mathematics, set him the following problem: "A loaf of bread is in the form of a hemisphere, with a crust of uniform thickness, the volume of the crust being equal to that of the crumb. What are the dimensions of crumb and crust?" As he solved the problem unaided his father opined that he might be of use in the world, notwithstanding his lack of robustness.

Gilbert's interest in his ancestors was not strongly developed. In his seventieth year he wrote to one of his sons, regarding certain details about his great-grandfather that had been gathered by a relative: "I am not much interested but perhaps you may be sometime, and so I suggest you keep Mrs. M's letters." On the other hand, he always felt and showed a strong affection for the living members of his family. In later years, when his residence was elsewhere, visits to Rochester were frequent; he nearly always halted there on his journeys to the West and back. When returning from the Henry Mountains of Utah in the autumn of 1876 he was in time to attend his parents' golden wedding on November 30. His mother died on February 25, 1883, at the age of 77; and his father on March 23, 1885, at the age of 79. The elder brother, Hiram Roy Gilbert, after whom Grove Karl's second son was named, continued to reside in Rochester until his death in 1902; the elder sister, Emma Gilbert Loomis, survives at her home in Jackson, Mich., where, as will shortly be told, Karl himself had, on leaving college, a short experience in school-teaching, where he repeatedly stopped to see her when crossing the country east or west, and where, while halting there for the last time of many on his way to California, he died May 1, 1918, five days before completing his seventy-fifth year.

GILBERT AT THE AGE OF 19

FOUR YEARS AT COLLEGE

Karl was the only one of the Gilbert family who attended the University of Rochester and gained a college degree; and this was at the cost of difficulty and sacrifice on the part of his father and himself. Regular outdoor exercise was a condition of the opportunity. The youth sometimes had to go shabbily dressed and was too much occupied to join freely in the social life of his companions; but his bravery and steadiness of character were such that he never appeared to be unhappy in consequence of these restrictions. Among his trials was a pair of light blue trousers which a tall friend of his father's had made the mistake of buying, and which when still in good condition were passed on to the tall boy; so good was their quality that they lasted undesirably long.

As elective studies were practically unknown when young Gilbert went to college, he took a prescribed classical course, and received the degree of A. B. in 1862, at the age of 19. His standing was always good, but he was indifferent to college honors; and in this respect the youth foreshadowed the man. The 36 units of his college studies included 8 of mathematics, 6 of Latin, and 7 of Greek, both the ancient languages being continued into the senior year. Rhetoric, logic, and zoology had 2 units each, and nine other subjects, including French, German, and geology, but 1 each. He contributed rhymed skits to a college paper, and always afterwards enjoyed composing verses, more or less humorous, on current or local events. During part of his senior year he was president of the Delphic Society, one of the two literary societies of the college, and he was awarded the Greek oration on graduating. The extended training in mathematics, for which young Gilbert had a natural capacity, served him well in geophysical researches of later years; perhaps his classical studies contributed to the clear style for which his geological reports were famous; they seem also to have determined a tendency to the use of long words of Greek origin and occasionally to the invention of such words, but they did not prevent his later approval of "simplified spelling," which in his case as in so many others was evidently a matter of unconventional temperament, and not of either ignorance or learning.

Gilbert's college teacher in zoology and geology was Henry A. Ward, who came to be widely known for his extensive dealings in natural history specimens, to which he later gave his whole time, as is further told below in the account of "Cosmos Hall." A first acquaintance with geology was thus gained, but unless by the rule of contraries it can not have been the influence of this enthusiastic collector, whose lectures must have been of a matter-of-fact rather than of a philosophical nature, which led Gilbert to say in an address, 20 years later, that the important thing is to train scientists rather than to teach science, and that the "practical questions for the teacher are, whether it is possible by training to improve the guessing faculty, and if so, how it is to be done;" thus implying not so much that, in his own experience, accurate observation is easy, but that successful guessing is difficult. It must also have been not his professor's idea but Gilbert's, prompted perhaps by a remembrance of an over-insistence on the names of things, that the content of a subject is often presented so abundantly in college teaching as to obstruct the communication of its essence, and that the teacher "will do better to contract the phenomenal and to enlarge the logical side of his subject, so as to dwell on the philosophy of the science rather than on its material." [1]

A BRIEF EXPERIENCE IN SCHOOL-TEACHING

On finishing his college course without developing any decided bent toward a special profession or occupation, and without physical strength enough to warrant his enlisting for military duty in the War of the Rebellion—his name was twice in the draft list, but not drawn either time—yet having reached the pedagogically competent age of 19, young Gilbert tried school-teaching at Jackson, Mich., not as the beginning of a life career, but, young American-like, as a means of paying a debt contracted during his undergraduate years. A class photograph at that time shows boyish undevelopment; the neck was overlong, the shaven chin was heavy, almost uncouth; the mouth was not fully resolved; but the upper part of the face

[1] The inculcation of scientific method by example. Presidential address, American Society of Naturalists. Amer. Journ. Sci., XXXI, 1886, 184–299. This address is analysed in a later section.

already had the clear-minded serenity that was so marked a quality of his whole expression in later years. Of his efforts as a teacher, one of his boyhood playmates wrote: "Karl was of too kindly a disposition to make a successful teacher of youngsters; they would take advantage of him." His own opinion, frankly told 50 years later, was that he could not control his pupils, an unruly lot of country boys; so he gave up teaching when the school year was only two-thirds over. This episode has a wholesome moral; it satisfactorily contradicts the current myth that a delicate-minded young master of a village school must needs thrash all the disorderly cubs in his classes as the only means of opening for himself an assured path to future success.

On returning to Rochester the unpugilistic schoolmaster found himself out of employment for a time, and the experience of waiting for work was so distasteful to him that he recalled its unpleasant impression many years afterwards in a letter to his elder son at a time, in 1912, when the son had his turn of waiting at the end of one engagement for another. The father, who in his later years adopted simplified spelling, wrote:

> I've had little experience with being out of a job, but enuf to know it is demoralizing. My slack time was forty-nine years ago, and I recall that I had no hart to do the various things that I had supposed I very much wanted to do when I was too busy to find time. Waiting for something to turn up seems to be an occupation in itself, and anyone who can really utilize the time while he waits is to be congratulated.

APPRENTICESHIP IN COSMOS HALL

The period of unemployment did not last long. Gilbert soon found work in Cosmos Hall, a scientific establishment which his former teacher, Professor Ward, had built on the grounds of the University of Rochester for the assembly and preparation of zoological and geological materials for sale to colleges and museums. Gilbert afterwards wrote of it:

> The establishment thus instituted grew and developed . . . Its work was performed largely by young men of congenial tastes, who there acquired the practical experience which commended them later to the trustees of larger responsibilities. It thus served incidentally as a training school in the natural sciences and especially in certain branches connected with museums.

This apprenticeship does not seem to have been entered upon so much because zoology and geology attracted the youth of 20, as because an assistant was wanted and the youth had nothing else to do; but the work must have proved satisfactory, for the youth kept at it five years—from 1863 to 1868. His duties included the sorting and naming of countless specimens; many thousand labels in the Ward collection, afterwards acquired by the University of Rochester, are in Gilbert's handwriting of that period. During at least part of these years, evenings were spent in home study of mathematics, with readings in anatomy and geology. In his daytime work he must have learned many facts and have profited from the discipline of steady occupation; but the philosophy of science could not have been learned any better by the handling of its material content during these five years of apprenticeship than during the preceding four years of undergraduate study; yet a liking for scientific subjects seems to have grown up during this laborious period and a loyalty to Cosmos Hall also, for he afterwards ranked himself "somewhat proudly" as its senior alumnus. Nevertheless, it is significant that Gilbert mentioned the "practical experience" there acquired rather than the influence of the director of Cosmos Hall as of chief value in preparing the young assistants for larger responsibilities.

THE COHOES MASTODON

Gilbert was occasionally charged by his chief with the installation of exhibits in museums, and this gave him glimpses of the world. It may have been in the course of journeys thus undertaken that he learned something by sight of the Pennsylvania Appalachians, to which he refers in a most appreciative manner in his first western report. Perhaps the most important assignment of this kind came when he went, in 1867 and 1868, to the State museum at Albany to restore and mount the skeleton of a mastodon, discovered a few miles away, at Cohoes, on the Mohawk, in 1866. Probably as a result of this discovery, and as if with a premonition of his work in mounting the skeleton, Gilbert wrote a general account of "The

American mastodon," which, his first published paper, appeared at Rochester in Moore's "Rural New Yorker" for March, 1867. This essay is notable for its effective presentation of good matter in a popular form, as the following extracts show:

> The recent discovery of the entire skeleton of a Mastodon at Cohoes, and the general interest felt by the public in the matter, will perhaps warrant a brief description of this ancient denizen of our forests . . . The large cavities in the front of the elephant's skull, that furnish a firm attachment for the muscles of the trunk, are equally characteristic of the Mastodon, and must have been accompanied by a similar proboscis. In fact, without this flexible nose that serves for hand and drinking cup alike, he must have perished, his projecting tusks keeping him from browsing, and his short neck not enabling him to reach grass or water . . . Each leg bone of the Mastodon is a little longer than the corresponding bone of the elephant, and is, in a greater ratio, thicker. This is but one phase of a general law of nature,—that the small are proportionally stronger than the large. The cricket leaps at one spring thirty times his own length, while the hippopotamus and elephant are too unwieldy to do more than walk or trot. The ant carries in his teeth loads many times heavier than his own body; the black bear is related to have borne in his mouth a carcass of about his own weight; the horse does not easily bear on his back more than half his weight; and the Mastodon required a disproportionate strength of limb to support his own huge body merely. Thus it appears that the latter approaches the limit of size for terrestrial animals . . . The Creator has adapted the teeth of all animals to their food, making them into chisels for the nut-piercing squirrel, hooks and knives for the flesh-devouring lion, shears for the grass-cropping ox, needles for the insect-catching mole and bat, and mill-stones for the twig-eating Mastodon . . . Lyell says that the cataract of Niagara has receded four miles at least since certain bones were deposited on its bank, and it now wears back only a few inches in a year. The Cohoes skeleton, naturally buried eighty-five feet under the earth, probably lay for still longer ages . . .

As the Origin of Species had appeared but a few years before this essay was written, the teleological philosophy of the next-to-last passage is not surprising. The reference to Niagara in the last statement makes one wonder whether the writer recalled his early interest in that subject when, in later years, he became its master.

The Cohoes mastodon skeleton that was to be mounted at Albany had been found in a huge pothole, measuring 40 by 70 feet across and over 60 feet in depth, near Cohoes Falls of the Mohawk River, just above its junction with the Hudson a few miles north of Albany. The work upon it was done under the direction of Prof. James Hall, whom, as director of the State cabinet of natural history, Gilbert and his associate Howell, also from Rochester, thus had advantageous opportunity of meeting. Hall took part in the excavation until he wrenched his hip by a fall in the pothole; then the work was left in charge of Gilbert. The skeleton was somewhat incomplete, and the missing parts had to be reconstructed; hence, as Hall wrote, "after carefully making a list of the bones we possessed, with measurements of the more important ones"—a large part of this labor apparently falling on Gilbert—"the young men were sent to Boston," in order to examine two more perfect mastodon skeletons there on exhibition; one was in the Warren Museum, a private institution in that city founded by Dr. J. Collins Warren; 40 years later this specimen was sold to the American Museum of Natural History in New York, where it is now preserved; the other was then in the anatomical collections formed by Prof. Jeffries Wyman, of Harvard College, Cambridge, and is now in the Harvard University Museum. According to Hall the young men during this visit met Louis Agassiz, Jeffries Wyman, J. Mason Warren (son of the founder of the Warren Museum), and Theodore Lyman; but unhappily there is little record of what these already established seniors and the then developing juniors thought of one another. A brief note in Doctor Warren's diary for June 20, 1867, merely records:

> Three young men, who came from Professor Hall of Albany and were introduced by Dr. Wyman, were engaged a good part of the day in measuring the Mastodon, preparatory to putting up one at Albany, which was found near Cohoe's Falls and which is imperfect, a number of the vertebrae, among other things, being wanting.

The mastodon was naturally more interesting to him than the three young men. No entries were made in Wyman's diary for that summer, and Agassiz kept no diary. So the past fades away. However, it is recalled that, as if in consequence of expertness gained in mounting the mastodon, Gilbert was not long afterward engaged in restoring missing tails for fossil Irish elks in the museums at Albany and Columbia University. Although he had few

contacts with Hall in later years, the memory of his sojourn at Albany while working on the fossil mastodon sufficed to call him back there in 1898, when he was one of the few pallbearers at the funeral of the great paleontologist.

THE MOHAWK GORGE AT COHOES

While in Albany, Gilbert, besides working on the mastodon skeleton, studied the gorge of the Mohawk at Cohoes and prepared an account of it which appeared in Hall's report. This is his first essay based on original field observations; it does not read at all like that of a geological catechumenist. He examined 350 potholes in the river bed above the falls; their typical form was that of a "chemist's test-tube;" the deepest had a vertical measure of 23 feet, with a diameter of 3 feet. A simple conclusion as to the origin of the potholes is conservatively announced: "In my examination I saw nothing to controvert the theory that they were formed by the grinding action of stones moved by water." The huge pothole in which the mastodon skeleton was found is one of a group of much larger dimensions, in the low upland outside of the river gorge and apparently excavated by other currents than those of the Mohawk. Cohoes Falls, descending 57 feet, lie between rapids up and down stream, and are peculiar in that they occur in a series of strongly tilted Hudson River shales of fairly uniform texture; hence the question was raised: "Is it not possible that rapids constitute the normal mode of descent of a river over these upturned shales, and the falls are merely an episode occasioned by preëxistent potholes?"—the potholes thus referred to presumably being members of the upland group. Many years later Gilbert spoke of this study of potholes as having been of so much interest as to lead him to desire further work in geology.

Although all these items are of interest, Gilbert's study here is chiefly significant from a novel quantitative method that he invented for determining the recession of the gorge-side cliffs. "Climbing from below, or lowered by a rope from above," he measured and cut sections of 20 contorted cedars, growing in a cliff and "appearing at a little distance mere bushes, but really very old trees," which had been dwarfed by starvation in the infertile shales, and of which the roots had been "bared by the waste of the cliff during the growth of the trees." An average of 6 sections gave 144 rings of growth to an inch of trunk radius, and the estimated age of the oldest tree sectioned was 716 years; yet the trunk of this famished pre-Columbian settler, who must have began his struggle for existence about the time of Thomas a-Becket of Canterbury, measured only 37.5 inches in circumference, or 6 inches in radius. The relation between age of tree and length of bared and exposed root gave a cliff recession of 12 inches a century; and this led to the estimate of 35,000 years as "a minimum for the time that has elapsed since Cohoes Falls were opposite the mastodon pothole."[2] This laborious method for the determination of cliff recession might have been applied by any patient and painstaking junior under the direction of an experienced master; but that the patient and painstaking junior should himself and on his own initiative have invented the method, as well as applied it, shows him to have possessed exceptionally masterful qualities with regard to natural phenomena, even if he could not master unruly boys in a country school. The thousands of years revealed in the age of the Cohoes gorge by this ingenious determination are hardly so impressive as the evidence that the determination gives of investigational ingenuity on young Gilbert's part.

HALF A CENTURY OF DIARIES

During Gilbert's apprenticeship at Cosmos Hall he formed the habit of keeping a concise diary, and this habit was pursued all through his life. Brief entries were made in small pocket-books concerning the persons he met and the places he visited; and 51 of these consecutive annual records have been preserved, beginning in 1868 and continuing to 1918; the last entry was made only a few days before his death. It is a great privilege to look over the personal records of such a man, not in the way of peering curiosity but in a reverent spirit, with the memory of the man himself constantly present, and with much of the sadness that one feels when standing alone

[2] 21st Ann. Rep. State Cabinet Nat. Hist. [for 1867]. Albany, 1871. 129–148.

and in silence by the grave of a trusted friend. A sincere interest is aroused by every item that teaches something of his habit of thought, something of his inner nature, something of the powerful and beautiful personality that so greatly aided the progress of geology in America and that endeared itself so warmly to all his associates.

Unhappily, entries in the diaries are for the most part colorless records of fact, with very few expressions of opinion or of feeling. There are occasional blank periods, and these are prolonged when the diary was replaced by field notebooks during many seasons of work in the West. Annual summaries of travels and other leading topics are found in many of the later books. Mention is frequently made of stops on journeys westward or eastward at Rochester to see parents or an elder brother; or at Jackson, Mich., to see a sister; but there is nothing written to indicate the warm affection that united the diminishing family. Instead of drifting apart in later years by reason of separated residence, the survivors seemed to grow closer and closer together. Brief extracts from the diaries will be found on later pages, where they occasionally serve to fix the dates of journeys and or to clear up matters that would otherwise remain obscure. The pocket diaries served also as cash accounts, in which items were faithfully entered for many years even to such detail as "car fare—.05"; indeed, in later years, the diaries contain little more than records of receipts and expenditures. The entries were regularly transferred to a carefully kept set of account books through most of Gilbert's life. A payment that closed or "squared" an account was marked in the diaries by a small rectangle. But in spite of all care, the entry of a small sum, usually less than a dollar, as "unaccounted for," not infrequently was needed when a balance was struck; a new start with cash on hand would then be made, headed "O. H."

Among items of larger interest are the subjects of papers at scientific meetings and of occasional lectures at colleges and elsewhere. Thus it is found that Gilbert's study of the Mohawk gorge remained in his mind, for an entry regarding it was made in a diary nearly six years later in Washington, February 7, 1874: "Evening G. & G. Soc. spoke on the Cohoes Cedars as time data." The "G. & G. Soc." is believed to have been an informal gathering of geologists and geographers; but as to that and many other allusions noted in Gilbert's brief records of long-gone years, positive information is lacking.

PREPARATION OF THIS MEMOIR.

Besides the half century of diaries and a large number of published reports and essays, many of Gilbert's field notebooks and more than a score of volumes of his press-copy letter books have been looked over in the preparation of this memoir; and in addition to these sources a large amount of personal material, from which many selections have been made, has been contributed by his friends and correspondents. This material has been extremely helpful, and its value is here gratefully acknowledged. The sources of passages thus secured are, however, seldom indicated; indeed, many extracts and quotations are not given as such but are welded into the text, because it is felt that attention should be concentrated upon the subject of the memoir and that distractions should be as few as possible. A similar principle was followed by Gilbert himself in his memoir of G. H. Cook, State geologist of New Jersey, in the preparation of which he borrowed freely from a number of sources, yet in which "marks of quotation are omitted because consistency would demand their use with a very large number of parts of sentences."

Gilbert's field notebooks and his official letters have been of great assistance in providing authentic record of his activities. Some of his personal letters, submitted by the intimate friends to whom they were written, have been even more valuable. Looking over these sources is like bringing the dead to life again. His written words conjure up his voice; his sentences recall his manner of talking; a humorous phrase brings the echo of his jovial laughter; when a reference to some past sorrow is encountered, it is as if the veil which time lets fall over the sad events of a long life were lifted, and as if the dulled grief were sharpened into pain again. But upon personal matters of this kind, meant by the writer only for the intimate friends to whom they were told, the veil falls again. Deeply as certain times of unhappiness were impressed upon Gilbert's inner self, frankly as they were spoken of to a very few, they were never made known to the greater number of his associates and they must now lie buried with him;

buried all the deeper because his courageous philosophy of life led him to live joyously. He kept his griefs and disappointments to himself and radiated only good cheer upon his comrades.

His field notebooks are more open to quotation, and many extracts from them will be found on later pages. It has been like reviewing a long chapter in the history of American geological exploration to look them over. Detailed sections representing much painstaking observation on desert mountain slopes speak aloud of the fatigues and rewards of outdoor work. As one sees successive items of evidence noted day after day, one may imagine the exhilaration with which new areas were entered. An occasional explanatory phrase suggests the manner in which the writer might have spoken at a scientific gathering, when recounting the enlivening experiences of search and discovery. Official letters have frequently afforded more entertainment than might be expected from such a source; they shed much light on Gilbert's manner of dealing with men as well as with problems, and they repeatedly reveal his inexhaustible generosity as well as his absolute fair-mindedness. His published essays and reports are well known as models of impersonal, logical presentation. The capitalization of certain names, such as "Basin Ranges," which he there adopted has been omitted in accordance with present official practice. It has been indeed a high privilege to enter so deeply into Gilbert's way of thinking as the review of all these records has permitted.

Yet, in spite of much effort, information on various points of interest is not to be had. Truly, if nothing were lost or forgotten biographical memoirs would grow to an unwieldy length; but it is deeply regretted that so much of the innermost and best should vanish beyond recall. Many personal records have disappeared, as the marks of light footsteps disappear from a surface of wind-blown sand. Concerning the mental life of Gilbert's boyhood and early manhood, there are only such glimpses as are set forth on the preceding pages of this chapter; concerning the deeper feelings of later years a few paragraphs will be found on later pages. Only the record of scientific accomplishment is fairly complete. Would that the penciled outlines in the little pocket diaries had been written out elsewhere more at length; and yet how short would have been their endurance as the centuries roll by even had they been engraved on tablets of stone with an iron quill.

Over the infinite prairie of level eternity,
Flying as flies the deer,
Time is pursued by a pitiless, cruel oblivion,
Following fast and near.

Ever and ever the famished coyote is following
Patiently in the rear;
Trifling the interval, yet we are calling it "History—"
Distance from wolf to deer.

CHAPTER II

TWO YEARS ON THE OHIO SURVEY

FIRST EXPERIENCE IN FIELD GEOLOGY

While Gilbert was still working under Ward in the spring of 1869, he learned that a second geological survey of Ohio was about to be organized; whereupon, as if intuitively knowing the value of application in person, he went to the capital of that State and asked, as he himself said "with a lot of cheek," the then governor, Rutherford B. Hayes, later President of the United States, for an appointment as assistant geologist. On being told that appointments would be given only to Ohioans, he went undiscouraged to call on J. S. Newberry, the successful one of several candidates for the office of State geologist, from whom the same refusal was met; but it was happily accompanied by advice to join the survey as a volunteer assistant, with promise of $50 a month for expenses, but no salary. Gilbert at once accepted this opportunity and went to work in July of that year; thus at the age of 26 he became a field geologist. The next year a small salary and a larger responsibility were allowed him.

Not the least advantage of this position was the association that it gave the young volunteer with other geologists; for the survey staff included besides Newberry several men who then and later made their mark on geological science. Edward Orton, afterwards professor of geology at the State university, successor of Newberry as director of the State survey, and president of the American Association for the Advancement of Science at the time of his death, when Gilbert succeeded him in that office; R. D. Irving, who became professor of geology in the University of Wisconsin and while there wrote a notable contribution to the history of pre-Cambrian time, before his death in middle life; E. B. Andrews, whose contributions to the Ohio survey reports constitute some of their best chapters; Henry Newton, who later studied the Black Hills of Dakota for the Powell survey, his posthumous report being edited and in part largely written by Gilbert; and N. H. Winchell, later professor of geology at the University of Minnesota, director of the survey of that State, and for many years editor of the American Geologist.

As a part of his office duties, Gilbert made drawings of fossil plants and fish, which won praise from his chief as being of "a style that has not been surpassed in this country." Portions of two winters were spent in New York City, there also in association with Newberry, who, besides directing the survey of Ohio, then and for many years following occupied the chair of geology and paleontology in the School of Mines at Columbia University, and who found his summer assistant helpful in the preparation of winter lectures. The assistant himself presumably utilized part of the indoor season in writing his reports for the Ohio survey; but records of other subjects than geology are found in the diaries of these winters. The young man was attracted by theaters, sermons, and lectures; of the latter he once heard two on the same day, January 9, 1870; one by the eminent Congregationalist, Henry Ward Beecher, on the "Request of the disciples for more faith," and the other, perhaps as an antidote for the first, by that ill-balanced iconoclast, George Francis Train, on "Old fogies of the Bible." Moreover, through Newberry, Gilbert met several noted men at New Haven: Silliman, Marsh, Norton, and Blake among others; but Dana is not mentioned. In February and April, 1871, the young geologist presented papers at meetings of the New York Lyceum of Natural History, first on the "Surface geology of the Maumee Valley," a subject that is further described below; second on the remains of a mastodon found in Ohio; these appear to be his first communications to a scientific society. Altogether the months in New York must have been enlivening. The intimate association with Newberry, winter and summer for two years, led Gilbert to feel a warm regard for his chief, which was afterwards manifested by frequent visits to him in the course of eastward or westward journeys.

SURFACE GEOLOGY OF THE MAUMEE VALLEY

Gilbert's field work in Ohio appears to have been limited to the northwestern part of the State, where a slightly diversified sheet of glacial deposits permits few exposures of bedrock

and gives a monotonous appearance to the smooth landscape. He prepared separate reports on several counties, following a standard plan for the State as a whole; but in addition he discovered and solved a delicate problem regarding the surface features of his apparently unpromising district. The results thus gained were so novel and so significant that Newberry generously permitted their publication in the American Journal of Science in 1871, two years in advance of their appearance as a chapter of Volume I of the survey reports in 1873. The area concerned is a plain of faint relief, which presents a general and very gentle slope northeastward to the southwestern end of Lake Erie; its materials were described as lacustrine clays, from 50 to 100 feet in thickness, and explained as the deposits from the expanded predecessor of Lake Erie which overflowed southwestward to the Ohio-Mississippi system. The nearly level plain is interrupted by two low and concentric swells or "ridges," both of curved outline, convex to the southwest; the outer and larger one being from 25 to 50 feet high, from 4 to 8 miles wide, and some 200 miles long around its curve, the chord of which measures about 120 miles.

At the close of a second season's field work, after Gilbert had detected the divergent arrangement of the glacial striæ on occasional exposures of bedrock, the occurrence of a southwestward outflow channel for the expanded body of clay-depositing water which proved it to be a lake and not an arm of the sea, and several beaches that mark shore lines temporarily occupied as the expanded lake fell to lower levels, he came upon a fruitful explanation for the curved ridges and the arrangement of the neighboring streams, concerning which he made a concise entry in his diary on November 10, 1870: "Invented the moraine hypothesis for St. Jo and St. Marys rivers"; this brief statement being one of very few of its kind in his long series of annual records; and it is this "moraine hypothesis" that forms the main subject of his special report. It is interesting to note that, as if already unconsciously developing the well-balanced and candid style of presentation which characterized so many of his later writings, Gilbert opens the chief passage concerning his hypothesis, not with a confident assertion of his conclusion as if it were a fact, but with a frank announcement of it as an opinion: "I conceive," he wrote regarding the larger one of the two swells of the surface, "that this ridge is the superficial representation of a terminal glacial moraine, that rests directly upon bed rock, and is covered by a heavy sheet of Erie clay, a subsequent aqueous and iceberg deposit"; yet while he inferred the moraine to be thus buried, he thought that the clays "so far conform to its contour, as to leave it still visible on the face of the country—doubtless in comparatively faint relief, but still so bold as to exert a marked influence on the hydrography of the valleys."

The context shows he had seen that all the little brooks run down the faint slope of the plain on courses which converge northeastward toward the lake; but that on reaching the exterior side of the curved morainic swells, the brooks are gathered into streams that flow along the base of the swell to the axis of the curves, where, uniting in the Maumee, they resume their lakeward flow through open gaps in the swells. A rational treatment was thus accorded to the disposition of drainage lines, and that at a time when the courses of streams were usually treated simply as matters of course, for which the current methods of orthodox geology suggested no explanation. More briefly expressed in the terminology of to-day, the Maumee drainage would be called consequent upon the inclination of the plain and the slopes of the morainic swells. Yet although the arrangement of the streams was discovered to be generically explainable, neither Gilbert nor his later associate, Marvine, who made the same discovery for various streams on the eastward slope of the Front Range of the Rocky Mountains a few years afterwards, thought of giving the streams a generic name indicative of their origin; that happy idea sprang from the inventive, systematizing mind of Powell when he explored the Colorado River of the West.

Gilbert's statement closes with the first announcement of a conclusion of far-reaching importance concerning the lobate margin of the great continental ice sheet, the pattern of a small part of which he had detected:

> We are here furnished partial outlines of the great ice-field, at two stages of its recession. Though but small fractions of the entire outlines, they yet suffice to indicate that the margin was lobed or digitate in conformity with the topography of the country that it traversed.

No finer instance of a mental leap from a particular instance to a broad generalization can be found. It is true that priority in the recognition of drift ridges as terminal moraines

appears to belong to C. A. White, who, as State geologist of Iowa, had somewhat earlier published an account of "two well marked but slight elevations in the general [drift-covered] surface of the country," both of which "seem at least to be accumulations of drift material which mark periodical arrests of the recedence, by melting, of the glaciers to the northward as the glacial epoch was drawing to a close.[1] And it is also true that priority in the detection of local lobation with divergent striations on the ice-sheet margin should be credited to M. C. Read, an associate of Gilbert's on the Ohio survey, although the lobation that he described was due to a northward-opening embayment in the conglomerate-capped uplands of northeastern Ohio into which a salient of the ice fitted,[2] rather than to a more abundant advance of the ice along a broad depression, such as the floor of Lake Erie. Hence even if Gilbert should not be credited with absolute priority of statement, his views concerning moraines and ice-margin lobation must certainly be regarded as better defined and of broader reach than those of his contemporaries. Yet in certain respects his views were incorrect, as he himself later acknowledged; for there was no land barrier by which, as he supposed, lacustrine waters could have been held at so high a level as to have submerged the morainic swells which now guide the rivers; and instead of their being covered by a "subsequent aqueous and iceberg deposit," it is the clayey moraines themselves that form the broad swells of the surface.[3]

As to the lacustrine barrier, Gilbert made a singular error which he afterwards righted. He erroneously assumed that the higher level reached by the expanded Lake Erie was due to an uplift of the land in the region of the St. Lawrence River, an idea which he held with sufficient confidence to mention it briefly again five years later in his report on the Henry Mountains, even though Newberry had added a corrective footnote to the Maumee Valley report reading as follows:

> It should be remembered that the retreating glacier must have, for ages, constituted an ice dam that obstructed the natural lines of drainage, and may have maintained a high surface level in the water basin that succeeded it.

When Gilbert was 15 years older and greatly matured by his experience in the Far West, he returned to the investigation of the Great Lakes region, and then, if not sooner, recognizing the correctness of Newberry's good guess, brought out his masterful essay on the history of Niagara River, as will be further told below.

Another item in the Maumee Valley report deserves mention for its bearing on later studies, as well as for the evidence that its final statement gives of Gilbert's cautious manner of dealing with his problems. He records:

> It is noteworthy that the small streams [which flow from the clay plain into the southwestern end of Lake Erie] . . . occupy, near their mouths, larger channels than it seems natural that they should have opened under the existing conditions. . . . If we suppose that the present water level of the upper [southwestern] end of Lake Erie was immediately preceded by a lower level, we have an easy explanation of the phenomena.

In other words, he recognized that the broadened stream mouths should be explained as slightly drowned valleys, although neither he nor anyone else had at that time used the suggestive phrase, "drowned valleys," in this sense. Similarly, the brief statement: "There is evidence that Lake Ontario at Rochester, N. Y., has stood seventy feet lower than now," suggests that he had recognized the neighboring Irondequoit Bay, familiar in his boyhood, as a partly submerged valley. Then after noting that the upland at the eastern end of Lake Erie, through which Niagara River has cut its gorge, is 38 feet above present lake level, and that wave-work ought to have formed beaches corresponding to that outlet level all around the lake shores, he adds regarding the southwestern end of the lake:

> We must look for the record of this work considerably above, or somewhat below the present coast; the present data do not indicate which is the more probable position.

The problem thus opened he completely solved later. No one else seems to have examined it in the interval.

---

[1] Report, Geol. Survey Iowa, I, 1870, 98.
[2] Geol. Survey of Ohio. Report of Progress, 1870, 471; repeated in Vol. I, 1873, 539.
[3] U. S. Geol. Survey, Monogr. XLI, 1902, 566.

CHAPTER III

THREE YEARS ON THE WHEELER SURVEY

THREE SEASONS IN THE WEST

Before Newberry became director of the State survey of Ohio, he had been geologist of several western exploring expeditions conducted by officers of the United States Army Engineers. It was therefore natural that, when Lieut. G. M. Wheeler, also of the Army Engineers, was organizing the "United States geographical surveys west of the one hundredth meridian" in the winter of 1871, he should ask Newberry to suggest a geologist for the new expedition. Gilbert was recommended and was forthwith appointed as "geological assistant;" thus began rather "late in life," as he himself felt, his career as an exploring geologist in the little-known Far West of those days.

It should be here recalled that, in the years shortly following the War of the Rebellion, the exploration of our western national domain was actively prosecuted. When Wheeler's survey was established, several other independent surveys were already in progress under different departments of the National Government, each one in active competition with the others for funds at Washington, and in ill-concealed rivalry with the others in the West, as a consequence of which an overlapping duplication of field work sometimes occurred. The eventual consolidation of the several surveys, following the recommendation of a committee of the National Academy of Sciences, in a single United States Geological Survey was not accomplished until 1879. It should also be borne in mind that Wheeler's survey was primarily, as its original name indicates, geographical and not geological; and further that Wheeler's conception of the work of a geographical survey was essentially the making of maps, including the determination of latitude, longitude, and altitude for standard points, and the representation of the intermediate areas by hachures or shading. He appears, as far as one may judge from the text of his own reports, to have had no clear conception of physical geography or of geography as a whole, even as it was developed in his time; and regarding geology he does not seem to have been informed at all.

No other geologist was appointed on the Wheeler survey in 1871, but A. R. Marvine, a recent graduate of the short-lived Hooper Mining School of Harvard University, who served under Wheeler primarily as an astronomical assistant and who became an accomplished geologist on the Hayden survey, which he joined the following year, reported on such geological observations as he could make while moving from camp to camp. A year later E. E. Howell, who had no more geological training than he could gather while a fellow worker with Gilbert in Ward's Cosmos Hall at Rochester, was added to the Staff. With these two, as well as with H. W. Henshaw, collector in natural history, Gilbert was closely associated. How different was the preparation of the young geologists for their tasks from that now exacted of new members of our National survey! Not one of them had made or could have had opportunity of making more than an introductory study of geology in college, for no American college then offered advanced teaching in that science. Not one had prepared a thesis, based on original research and replete with citations from the work of earlier geologists, or had passed a formidable oral examination on the general content of geological science for a doctorate in geology; higher degrees in geology were then practically unknown among us. To charge these little-practiced apprentices with the geological exploration of a new country was like authorizing a boy to swim by throwing him overboard into deep water. And yet for those who survived it this rude method led to great results; so great indeed as to make one wish that all young men who now, after a fair beginning as undergraduates, wish to embark on geology as a profession, might have the inspiring opportunity of investigating a little-known region on their own responsibility, as a practical test of their quality and capacity.

It was indeed with a sudden plunge into the deep unknown that Gilbert entered upon the arduous experiences of his first season of western field work, for it began abruptly and continued through eight months of almost continuous movement. Having left Rochester on April 21 and arrived at San Francisco on the 29th, a week before his twenty-eighth birthday, he had only three days there before turning back again and going with various members of the survey as far as Halleck in northeastern Nevada. Two weeks were spent at that point before the unwieldy expedition, which numbered over 40 men with 165 horses and mules, made its first move; later on, it was usually divided into two or three smaller parties. Gilbert's division for a time zigzagged among the ranges of the Great Basin into California on the west and Arizona on the east; then crossing the plateaus south of the Colorado Canyon, it went eastward as far as Mount San Francisco. Return westward was finally made through southern Arizona by the valley of the Gila to Yuma, where a river steamboat was taken down the Colorado to its mouth; thence an ocean-going steamer, running southward through the Gulf of California and northward along the Pacific coast, carried the party to San Francisco on January 3, 1872. During a brief delay there Gilbert called on J. D. Whitney, at that time director of the State survey of California, to examine volcanic rocks, and on Clarence King, director of the Fortieth Parallel survey; and then turning to the East, with stops on the way at Cleveland to see his former chief, Newberry, and at Rochester to see his family, he went for the first time to Washington, where he arrived on January 25. Journeys across or nearly across the continent were repeated many times in later years.

Departure was made from Washington for a second western field season late in June, 1872, this time with the title of chief geologist; and after making stops again at Rochester and Cleveland, Salt Lake City was reached early in July. Thence Gilbert's party, frequently working independently of the main expedition, explored southwestern Utah and northwestern Arizona, thus covering an eastern part of the Great Basin and a western part of the plateaus north of the Colorado Canyon. It was during this season that Gilbert saw the House Range, which he selected nearly 30 years later for closer study as a typical example of a dissected mountain block. Farther east the truly fracturelike cleft of the Virgin River in massive sandstones was examined, and acquaintance was made with the long lines of cliffs, "trending east and west and facing south," by which the northern plateaus are "divided into a series of great terraces." The return journey from Salt Lake City to Washington was begun on December 12.

The third and final season under Wheeler began in July, 1873, at Fort Wingate, N. Mex., which was reached by stage from Pueblo, Colo. Field work extended over western New Mexico and eastern Arizona south of the Colorado River, thus once more including parts of the plateau and Great Basin provinces. Monoclinal flexures and the Zuñi uplift were studied in the plateau region, of which the southwestern and southern margin was traced; several members of the basin-range system were examined, and volcanic phenomena were studied more fully than before. This season closed with Gilbert's return, in late November and early December, by stage to Pueblo and train to Washington; there he spent a large part of the following year in completing his reports. His residence in Washington is described in a later section.

FIELD NOTEBOOKS: PERSONAL EXPERIENCES

It is profitable to know something about the personal methods of work adopted by successful scientists, and the field notebooks of an eminent geologist are therefore of special interest. The theory of note taking in geological field work, especially during a prolonged campaign in a distant and little-known region, demands that the records of observed facts shall be so complete as to leave little to the memory, and advises that full but carefully separated records should be made of the speculations and interpretations excited by the facts. Practice departs largely and variously from theory, and in Gilbert's case conspicuously so. His notes of three field seasons on the Wheeler survey were made in 17 small books, each containing about 140 pages, measuring 6 by 3¾ inches, and usually with from 70 to 100 words to a page, written crosswise. The record of observations for the first year of rapid movement with a large party is only occasionally detailed, more often scanty; a day occupies from 1 to 10 pages. In

subsequent years, when he was freer to move as he wished, records are fuller. Theoretical inferences are rarely found; sketches, profiles, and sections are for the most part incompletely and roughly drawn. The handwriting was rather careful in the first book of 1871, but became more irregular afterwards; in some of the later books grotesque initial letters are often elaborately drawn at the beginning of each day's notes, as if there had been time to spare after breakfast before the party was ready to move. The form of record is simple and direct, often colloquial, and occasionally facetious. The pages contain a mixture of items about persons and places of temporary value; of more serious but irrelevant matters, such as notes on vegetation and mining, useful in building a background of experience for the writer even though no later use is made of them; and of geology proper, in the extension and interpretation of which memory must often have been largely resorted to before the recorded observations could be put into form of value to others than the writer.

The following extract from an entry at a waterless camp in the Mohave desert in August, 1871, is altogether exceptional in its fullness and generalized quality, but it is characteristic of Gilbert's even temper; for in spite of many discomforts and of occasional hardships, no word of complaint is anywhere recorded.

Our dry camp of last night illustrated some phases of human nature, good & bad. There was no conversion of character, but merely a development. Those who customarily exhibited sense remained cool. The feeble-minded were panic stricken. The generous, the selfish, the sanguine, the timid did not change their characters. . . . The greed with which one or two absorbed the public water showed that it would not do to make it common property in case of extremity. The only way to ensure a proper economy & temperance in its use is to have each canteenful private property, & if a larger quantity is transported, to have it issued in rations in some equitable manner.

A sample of frequent notes on plant forms is as follows, from near Ivanpah, Nevada:

The novelties in vegetation have been many. It appears that there are two Spanish bayonets, one trunked & branching with seed pods barely as large as a bk. walnut, the other with paler leaves & pendent seed-vessels three or 4 inches long. These, esp. the 1st with the Palmetto make the plain look like an orchard, so thickly are they set. New cacti of 6 kinds.

Mishaps are often recorded, as on August 27, 1871, in the desert of Nevada:

Today my mule gave out with hunger & fatigue & I had to walk several miles, but she finally recovered so as to bring me into camp at nine o'clock, which was but an hour later than the rest.

Frontier conditions during the era when the West was in the dim dawn of the Star of Empire, before the sunlight of civilization had come over from the East, were illustrated by an incident in Arizona, November 7, 1871:

"In Camp near Prescott. Rumor of the attack on the stage containing H———, L———, and S———.
November 8:
. . . The camp goes on with its regular business notwithstanding the news from Wickenburg. Indeed there is nothing to be done except to write to the friends of the murdered. The at-present accredited version of the affair is: The stage with 7 passengers & 1 driver was attacked 6 m. beyond Wickenburg by white men 10–13 in number. The driver did not halt when ordered & the stage was fired into from behind. Several men were wounded including the driver & the team became unmanageable. . . . . [Two passengers] though both wounded jumped out and escaped by running ahead. . . . At the stage were found the dead bodies of all but H———, who is not yet accounted for. The murderers took hastily some money & retreated. A large amt. of money was overlooked. Twelve men started from Wickenburgh or Vulture Mill in pursuit and a company of cavalry was afterwards dispatched. Later news leaves no hope for H———.

It is interesting to learn from a passage in Wheeler's narrative that a member of the Mohave tribe who accompanied the boat party of the survey into the Colorado Canyon, as described below, aided in discovering the perpetrators of this murderous attack, who were thus found to be not white men but Indians.

Toward the end of the first season, some experience was had of the forlorn conditions prevalent in certain frontier towns. Four days "were spent in Arizona City in a somewhat monotonous manner. . . . Marvine & I practised a little at billiards. . . . Wrote a column for the Free Press. The Free Press office is about 14 ft. square and includes the bed as well as the table and desk of the editor & all hands. Boxes serve as chairs & bottles as candlesticks.

No stove. A dirt floor." A tersely expressed opinion of the editor is added, but is not here quoted. Great changes have taken place since that early time, for in the present era of statehood, Arizona is, according to the competent testimony of one of its own officials, entering upon a career of progress "that shall be equal to none."

The notebooks of the second season contain somewhat fuller records than those of the first. A rather wide range of home reading is suggested by an entry made on August 8, 1872, in a narrow shelter from the glaring sunshine of the Sevier desert:

I write this in the shade of a telegraph pole. "Bless the good Duke of Argyle."

But in case any reader itches for an explanation of this remote and aristocratic allusion, he will be barking up the wrong pole if he consults a telegrapher. An increasing range of field experience appears to have been reached after leaving the arid basin ranges and entering the moister province of the high plateaus, for note was made on October 14 of an item characteristic of practical geological exploration:

Just at camp we had to cross a creek at a steep spot & my saddle went forward. I "nat'rally" went overboard into the creek. No damage reported beyond a wetting.

This confession is illustrated by a faint little pencil outline of the horse stopping on the rapid incline and the rider plunging head first into the water below.

Not long before, on October 3, Gilbert had his first view of the fantastically sculptured slope beneath the south-facing escarpment of one of the high plateaus in southern Utah, which appears to have excited more admiration in the minds of the senior members of his party than was felt by one of the assistants.

Up the Sevier a few miles & then to the left a few miles more until we came suddenly on the grandest of views. We stand on a cliff 1000 ft. high, the "Summit of the Rim" . . . Just before starting down the slope we caught a glimpse of a perfect wilderness of red pinnacles, the stunningest thing out for a picture.

Later on the same day is written, under "Incidents":

When Mr. Hoxie and I reached the jumping off place & were entranced & exclamatory at the grandeur of the view & its topographical excellence, up comes Mr. Kilp & remarks with a smile: "Well, we're nicely caught, aint we?"

This trifling incident merits citation here, because it remained in Gilbert's mind long afterwards and was retold in California with much fuller account, drawn from memory, of the wide prospect than is entered in the notes, "to illustrate the relation of the traveler's appreciation to his point of view," as follows:

One summer afternoon, 35 years ago, I rode along a high plateau in southern Utah. My companions were Hoxie, a young army officer; Weiss, a veteran topographer, who mapped our route as we went, and Kipp, an assistant whose primary duty was to carry a barometer. Not far behind us was a pack-train. We were explorers, studying the geography and geology of a strange land. About us was a forest of pine and fir, but we rode through a lane of sunlit prairie cradled in a shallow valley. Suddenly the floor of the prairie came to an end, and we halted on the crest of a cliff overlooking a vast expanse of desert lowland. The desert was not a monotonous plain, like that of northwestern Utah, but a land of mesas, cañons, buttes, and cliffs, all so bare that the brilliant colors of their rocks shone forth—orange, red, chocolate, blue, and white—fading slowly into the gray of the remote distance. We were looking across the broad barren tract through which the Colorado winds in Glen and Marble cañons, and of which the Painted Desert of Arizona is a minor division. To most of us it was a supreme vision of beauty and grandeur as well as desolation, a scene for which words were inadequate; and we stood spellbound. The silence was at last broken by Kipp, who exclaimed, "Well, we're nicely caught!" and his discordant note so carried us from the sublime to the ridiculous that our tense emotion found first expression in a laugh. . . . Kipp saw only that the cliff at our feet barred further progress in that direction, and all that had appealed most strongly to the others was lost on him." [1]

The wretchedness imposed upon certain settlers who, for reasons that are best not inquired into, seek isolation in a barren desert where living is barely possible, is strikingly described in the notes of November 10, 1872, at a camp by a small spring under the Vermilion cliffs of the plateau province, east of the Kaibab and north of the Marble Canyon of the Colorado:

[1] Sierra Club Bulletin, 1908, 225.

The house of . . . has one long room & about 10 inhabitants. It is half dug in the shale & half built of stone. Two wagons near by serve as sleeping apartments. Before the door is a spring that flows down a steep slope of shale trod to mud by the cattle & devoid of vegetation. Back of it rises the red sandstone cliff & in front stretches the desert plain cut by the Colorado chasm. The largest tree is greasewood [a small shrub] & in fine the picture is one of intense squalor & desolation. Imagination could not invent a more appropriate home for such an outcast.

Better conditions were found a few weeks later in a small Gentile town farther north:

I have returned at night to ———'s Store which is the most comfortable house I have seen for many weeks. It has four rooms and a housekeeper. The table cloth is white. The butter is good & the milk is cream. This is a combination of luxuries unknown in the saintly settlements. *Contra* the wholesome brown bread of Mormon penury is exchanged for white, light, palatable, indigestible biscuits. *Beds.*

The last word suggested a review, which follows:

At Zion we furnished our own blankets & slept on the floor. At Rockville the same except we were furnished pillows. At Mt. Carmel we were given extra blankets & the lee side of a corn stack. At Toquerville I slept in a wagon box with the boy, at Workman's Ranch on the ground with the boy again. At Kanab in a bed on a bedstead alone, at Allendale ditto with the boy. At Circleville, ditto, ditto.

Such are examples of personal experience taken from Gilbert's early records of his western explorations.

NOTES ON SCIENTIFIC TOPICS

Geological notes are usually limited to matters of direct observation, such as the nature of volcanic rocks, sections of stratified formations with record of attitude, composition, and fossils, and estimates of thickness. Surface forms are described briefly, if at all. Reviews and generalizations are rare; by way of exception a good number of observations on springs are collected in summaries on two dates in August, 1871. A bath in Sevier Lake led to a concise note as to the density of its water:

It is not so buoyant as Salt Lake & I infer not so salt. Floated about as in figure. *a b* & *c* are water lines for fresh water, Sevier L. & G. Salt Lake. The water of the latter holds 20% of mineral matter. The second may have 12%–14 %.

The figure referred to shows a man immersed to different depths, indicated by lettered lines.

Theoretical inquiries and speculations were rarely recorded, although it is impossible that a mind as active as Gilbert's should not have indulged in them frequently. A rare example is as follows, November 19, 1871:

. . . There are Problems connected with the +d sandstone [a cross-bedded yellow sandstone, 400 feet thick, in the upper Gila Valley]. 1st. How can it have originated conformably over a large area of limestone? The Potsdam [sandstone resting on crystalline rocks] is easily accounted for. It represents a gradual sinking of the land & is followed by the natural sequence of shale and limestone. If the +d sand also represents a sinking, where is the erosion of the complementary elevation? 2d. How came so great a mass to be cross-stratified in one system from top to bottom? . . . The uniformity of all beds along lines of mesa front and the line of the Colorado indicates that these lines were coast lines or parallel to coast lines during the original deposition. The belt of the +d sandstone may not be a broad one though it is already proven to be 100 miles long at least. Perhaps the bed was laid slowly during a period of constant conditions when a strong shore current bore the sand along to gradually build out a bar. In that case the ocean must have been south of the mesa line [plateau rim] for the dip is that way. . . . Later, having seen more of the +d sandstone I have to limit the description as to cross lamination. The lines do not run through the lines of bedding, though they present that appearance at a distance. At the top of a bed they terminate somewhat abruptly without deflection while at the bottom they become tangent to the line of bedding. The lines may have originally curved at the top also, & have been cut off by the currents that formed the succeeding bed.

The reason for this exceptional deliberation may be found in a preceding entry:

Mules and horses strayed this a. m. & some delay in getting them.

But deliberate as the discussion was, the absence of all suggestion of an aeolian origin for the cross-bedded sandstone is noteworthy.

In the following year, 1872, the peculiar conditions under which the coal beds of the plateau province had been formed excited inquiry:

The alternations of coal and limestone in this section is an anomaly. Not less so is the absence of underclay in the southern coals. There should be some discoverable reason. Is it not that the Cretaceous coal is made from plants so far different from those of the coal measures that they have a different soil or even habitat.

In the same year the best of a very few examples of general discussions was written east of the Kaibab on November 15, concerning the broad denudation of the plateau country:

The detached mesa (of chocolate shales apparently) that we see beyond [south of] the Colorado . . . bears on the problem of denudation. It is an outlier 20 miles at least from the main bed. Such instances are exceptional & while they indicate great denudation do not solve the problem whether the entire Kaibab region has been covered by the banded sandstone or by the yellow sandstone or by the Cre. & Ter. with the facts now at my command I do not see how to solve it. Where cliffs of like age & character face each other on opposite sides of an anticlinal it is not difficult to bridge the chasm in imagination, but where successive cliffs involving 10–1200 feet of strata from Ter down face the metamorphic zone of Arizona & drain toward it, it is hard to say how far they have extended. There are other island mesas of different beds but I can recall none so far removed.

Some examples are then cited from the Great Basin province southwest of the plateau area, regarding which it is said:

These however have been separated first by convulsions & only secondarily by denudation.

Other examples on the plateaus are added, and of these it is said:

The margins of the several strata are remarkably simple & suggest pelagic erosion rather than fluvial, but there is no other evidence tending the same way. No trace of the denuding coast phenomena appears. Perhaps it is better to suppose that the general limits of denudation are the limits of rapid denudation determined by changes in the character of the beds subjected to it.

Convulsions, pelagic erosion, and limits of rapid denudation are encountered rarely if at all on subsequent pages.

It is not a little surprising to find that records of theoretical views as to the origin of the basin ranges are almost wanting. The few that are entered will be quoted in the chapter devoted to that problem, although they are very indefinite as to the main point involved, namely, the occurrence of master faults along or near the base line on one or both sides of the ranges. It might be inferred from this that the discussion of faults was an unfamiliar matter to the recorder, but the accounts of certain dislocated blocks in the plateau province show clearly enough that such was not the case. One of the most explicit of these accounts concerns the displacement, now well known to many observers, of the Vermilion and other cliffs near Pipe Spring, west of Kanab, on the Utah-Arizona line. There, under date of October 25, 1872, the observed topographic and structural facts are shown in sketches and in plan; and the place of the inferred but invisible fault is made clear in two cross sections. Referring to one of these the statement is made:

It is evident that Pipe Spr is precisely on the fault & is determined by the abutting of the banded sand against the chocolate (just here slate) shales.

The cliff on the west of the fault, composed of beds dipping at a moderate angle toward the fault line, decreases in height toward its cut-off termination;

& pipe spring is at its extremity receiving its discharge from the dip in a manner that astonishes.

But there is a significant difference between the well-defined faults by which the northern part of the plateau province was found to be divided into huge blocks and the inferred faults by which the basin ranges were thought to be limited. The faults of the first group were proved by standardized and generally accepted evidence, furnished by the repetition of a clearly exhibited series of identical strata on their two sides; while the faults of the second group were adventurously inferred on unstandardized physiographic evidence, furnished by visible strata only on one side. This evidence was not then clearly formulated even in the mind of its discoverer, and was entirely unknown to geologists in general. Indeed it was not explicitly formulated by its discoverer himself until about 30 years later.

A BOAT TRIP INTO THE COLORADO CANYON

Something of Gilbert's activity and courage in his first year of western work may be learned from a passage in Wheeler's own report of the venturesome penetration of the Colorado Canyon upstream in the autumn of 1871, thus reversing the course of Powell's boat journey down the

river a few years before. After the canyon was entered the chief of the party found valiant support in his geologist at a time of emergency: "No one but Mr. Gilbert and myself," he wrote, "think that the boats can pass the rapid in front of us. . . . Mr. Gilbert and myself propose to reassure the men by taking the first boat across [up] the rapid "; and they succeeded. Boyhood practice in boating at home on the Genesee here served a good purpose.

The leading facts regarding this trip are as follows: While a small division of the expedition went eastward over the plateau, a party of 34 persons, including 8 survey members, 6 boatmen, 6 soldiers, and 14 Mohave Indians, set out on September 16 in three boats and a barge from Camp Mohave, where the southernmost point of Nevada lies between Arizona and California, a short distance up the river from the present crossing of the Santa Fe Railway at Needles; they worked their way against the current 80 miles almost directly northward and 60 miles deviously eastward, passing on the way through several subordinate canyons that trench certain members of the basin range system, and thus reached the "crossing of the Colorado," where some of the party left the boats and went on overland. Above this point the reduced boat party advanced about 50 miles southeastward into the lowermost section of the Grand Canyon as far as Diamond Creek; but one of the boats was sent back with a number of the men when further progress was threatened by some difficult rapids; and the exploration was completed by only 20 of the original 34 members. In the 30 days of travel, the distance along the river was 222 miles, and in this distance 208 rapids were ascended. The boats were left at the mouth of Diamond Creek, a side valley was ascended southward to the plateau, where, after joining the rest of the expedition, the journey was continued eastward over a lava-covered country. Wheeler wrote a narrative description of the boat trip in the first volume of his reports (pp. 157 to 169), illustrated by several good photographs—many others were lost in the river or spoiled in overland transit afterwards—and accompanied by a topographical map on which the 31 camps of the river party are indicated; but as the publication of this volume was delayed by Wheeler's ill health until 1889, it was out of date when it appeared. The following extracts from Gilbert's notes, though they now are even more of a "back number," will give some idea of the observations made and of the difficulties encountered:

September 19, 1871:

. . . P [ainted] cañon is not a very startling affair in point of size but well deserves its name. Its variegated lavas are umber, ochre, black & reddish. Not brilliant colors but in good contrast.

September 21:

. . . The wind was of great service today carrying us along gaily except at three or four rapids. *Contra*, it interfered with photography & kept O——— in a perpetual state of profanity . . . I am a little disappointed in Black cañon as I had based my ideas on Ives' view of the entrance of which I cannot find the original . . . Gibraltar affords data for half of that picture but the other side is wanting.

September 23:

. . . The cañon in this part better accords with the idea I had conceived. The walls are not so steep as fancy (& Ives) had pictured them nor are they so high but they are for considerable distances unclimbable and we found camping ground so scarce that our search for it was prolonged into the darkness.

October 2:

. . . Adjacent to the river are gravel mesas of two distinct epochs, the lower being red. These are in one sense conformable. The red was eroded deeply before the deposition of the other . . . In general we may say that the red was succeeded by a low water system, succeeded in turn by a higher . . .

October 4:

. . . The edge of the Great Carboniferous Mesa [the western margin of the Plateaus] is not due to erosion but to a dislocation with a N. S. trend . . . The wall has the right to all the adjectives (except numerical) that have been given to it.

October 5 [in camp at the Crossing of the Colorado]:

. . . In the course of the afternoon the land parties arrived & were ferried over. Had a long talk with Marvine & Ogden. Packed a box for Truxton Springs. Hence the River party takes but three boats, each with 15 days' rations for its 7 men. Lieut. W., Mr. O'S & I command the boats.

A cross section drawn near this point and showing the dislocation which defines the "western edge of the great Carboniferous mesa" is reproduced in facsimile in Figure 3.

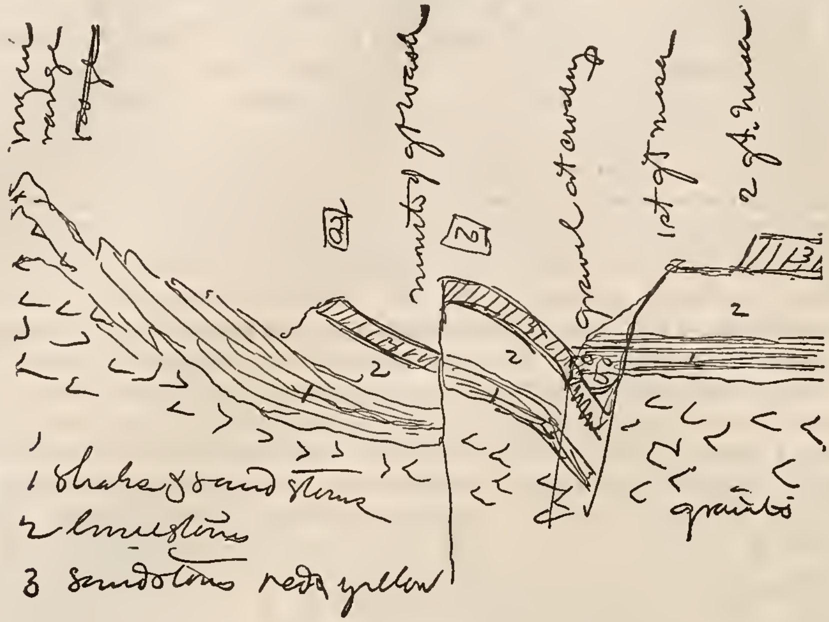


FIG. 3.—Section at the mouth of the Colorado Canyon; from Gilbert's notebook, October 4, 1871.

October 6:

. . . I propose to call our boat (no. 3) the Trilobite. We managed to get off from Camp Crossing at about ten A. M. Mr. Marvine accompanies us so far as to get a glimpse of the mouth of the Cañon & then returns. We camp outside of the Cañon & H——— & I start to climb the wall. H——— sickens (morally) at the first third of the climb & returns. I do not reach the top until after sunset though I started at about 1 P. M. It is the hardest climb I ever undertook. . . .

October 7:

. . . Last night I spent alone on the mountain at the foot of Big [Grand] Cañon. Having no blankets I built a little fire in a sheltered spot among the rocks and hugged it all night, getting little sleep. The first thing that daylight shows me is that I am on only the first terrace & the second rises 5 m. to the east and trends a little S of east. There can be no doubt that it consists of yellow and red sandstone . . . and the best guess I can make at its feet is yellow 600, red 1200; total 1800.

Many years afterwards Gilbert told a friend that he had expected, when looking up at the escarpment from the river, to make both ascent and descent before dark; that he was much alarmed at finding himself compelled to spend the night on the "terrace" because "bad Indians" were about; and that when trying to sleep he was actually frightened by hearing "pat, pat, pat," like footsteps stealthily approaching, but the pats proved to be only the beating of his own heart, audible in the extreme stillness of the solitude.

The notebooks give details of the stratigraphic succession in the escarpment of the plateau:

Here is the limit of disturbance . . . The gravel accumulations in the wash valley are immense & prove that a high barrier has once contained the waters of the valley. . . . Reached Camp [on river bank] at about 1 P. M. with a big tired on. Whiskey, coffee & a rest brought me around however . . . Broke camp about 2 P. M. & worked up the river a few miles, passing springs. The granite that we had at the mouth & which I neglected to collect disappears on todays march & the strata descend so as to bring No. 2 below water." . . .

On October 11, an entry is made about great joints in—

the "red-wall" limestone as I may as well designate the heavy mass that it troubled me so to climb. . . . We find as we proceed two long stretches of rowing water where the granite walls hold the river narrow with very little debris at the foot. A sharp rapid intervenes and at the head of the upper a roaring rapid that gave us one too many. The leading boat,—M——— at the helm; H——— at the pole—wrecked or rather swamped & upset scattering its freight along the bottom & top of the river. I started at once with . . . [four oarsmen] to save the floating debris. Some of the men are demoralized a little by the rapid & tomorrow I have volunteered to steer a boat up. The granite continues to grow higher and is beautifully sculptured by pot-holes & sand action. Much of the surface is smooth and glazed.

FIG. 4.—MOUTH OF THE COLORADO CANYON

Photograph by W. T. Lee, U. S. Geological Survey

On October 12 the fifth notebook of the summer is begun.

This book opens under a cloud at Camp 22 . . . in the Big Cañon, for last night occurred the accident that lost valuable books & papers and this morning all hands are at work repairing & searching . . . In the P. M. after astronomical observations & caulking, we take all things up again to the rapids & with Mr. Wheeler for bowsman I take Boat Picture up the rapid. We ship water . . . but the large force on the ropes pulled us through safely. Our camp at the head of the rapids.

October 13:

. . . From this point & time the boat party is divided, One boat goes down stream with dispatches & exhausted & demoralized men. The Picture & Trilobite go on up with 10 men (7 white & black; 3 red) each. . . . During the day we make some lively transits. One involved a run out with the line on the thole-pin & then a jerk ahead after throwing it off . . . The boat was often so highly inclined on a fall that to go forward one must climb as though up stairs . . . .

October 14:

. . . At noon we encounter again the worst rapid we have met & this time are compelled to make a portage of boats as well as freight. Above the rapid the current for a few rods being too swift to row & the cliff perpendicular so that towing is out of the question. We have "crept" in the old Genesee style & laid a rope to warp up by with loaded boats . . . The principal birds in the cañon are bats! they can be seen at all hours of the day and night. The steep walls do really shorten the day in such manner as to delay us somewhat.

October 15:

. . . Our progress is but four miles over a series of rapids. Toward night we reach a double rapid,—two rapids with a short interval of water that can be crossed. On the lower half the rope broke & let H——— and me drift down stream. We did not discover that we had our line dragging until it anchored us in comparatively slack water below. Then we pulled in the rope and made for the nearest accessible shore. We had shipped some water & put the cargo ashore to save wetting. Made coffee & beds.

October 16:

H——— and I were out of camp (27) last night on acct. of boat accident, and the camp missed us, for we had food & beds & our crew went without either. They had however some bread in the morning when we came up & some of them made up all deficiencies by a good hearty grumble lasting through the day. Tonight Lt. Wheeler puts us on short allowance . . . . Our bacon is gone, & beans & rice are scant; but coffee is in plenty and will outlast every other item. Our flour will hold out at this rate six days & these must bring us to the Diamond River or back to the crossing, the former if possible.

October 18:

. . . 2 accidents & I in each of them. 1st as my boat the Trilobite was taking in her cargo her fastenings gave way & she fell backward over the rapid, bumping her stern severely over the rocks & starting a rapid leakage. R——— H——— and I had the ride down and did not enjoy it. 2d At a very lively rapid an attempt was made to drag up an otherwise empty boat with S——— and me aboard, & we were swamped & upset. Shore was near at hand & we swam to it & hung our clothes on the rocks to dry. Here I found the inconvenience of having no change of raiment . . . In the first accident three carbines were lost & one of them mine. I do not feel very sorry unless I am called on to pay for it; which would be highly unjust. The care of it was onerous & not compensated by any present nor probable use. This forenoon we saw a star (probably Venus) by day probably at 10 or 11 A. M. It appeared just above a 1000-ft. cliff that occulted the Sun, & was plain to be seen. This will go well with the bat matter in describing the gloom of the cañon . . . The roar of the rapids is echoed by the cliffs and in the still of the night has the seeming of a mingling of many voices. As I write it is somewhat musical & reminds me of church-bells in the distance (when alone they are musical).

October 19:

. . . The water has been so swift today that we have had to tow the boats most of the way. . . .

October 20:

. . . Lay in camp at Diamond River all day resting or trying to. The river trip has proved very exhausting & after 24 hours of nothing to do (heavier than solar observations) I still feel as though just out of a threshing machine.

Here a detachment of the overland party came down from the plateau, with mules carrying a supply of food, so that the river party, which had been on short rations for a time, had a full supper.

The only published allusion made by Gilbert to this difficult piece of exploration is an inconspicuous sentence in the geological volume of the Wheeler reports, in connection with a systematic explanation of cliffs and slopes in the terminal part of the canyon:

With the boat party, headed by Lieutenant Wheeler, I ascended this portion of the gorge, and had my attention especially drawn to the rapids and other phenomena of erosion and transportation. (Vol. III, p. 70.)

CHAPTER IV

GEOLOGY IN THE WHEELER REPORTS

REPORTS ON THE GREAT BASIN AND PLATEAU PROVINCES

Gilbert's first season of western work led him rapidly into a region of "mountain intricacies, rigid plateau contours, and desert wastes," as Wheeler described it. The young geologist had to carry on his studies, which were regarded as subsidiary to the main geographical object of the expedition, as well as he could. Naturally and necessarily his work was incomplete. Yet his chief, as if to forestall a reexploration of the region by one of the rival surveys, announced his belief that "the geological matter" gained by Gilbert and his associates "when supplemented, as it soon will be, by a series of geological maps and paleontological reports, will answer all the present needs of the Government and of the industries of these partially inhabited areas, in which, for years to come, geological or other scientific examinations will find but few localities where sectional industrial interests may be healthfully promoted with economy to them or to the Government."

The young geologist was under no such illusion. Although his chief credited him with having "aided to give form to the work of the geological parties," it is plainly to be understood from many pages of his reports that he regarded his journeys as mere reconnaissances. For example, in his first chapter he explicitly states that the Inyo Mountains, one of the larger ranges next east of the southern Sierra Nevada, are "too important and complex to be characterized by our meager data." He felt that it was tantalizing to see 8,000 feet of bedded rocks beautifully displayed in bare mountains, near the middle of the oblique California-Nevada boundary line, and "yet be unable to examine a single stratum." At many points his observations were regarded as "too cursory to warrant individual mention." He understood that it would be premature to attempt a full discussion of the Great Basin "before the characters of the basin ranges shall have received more thorough study than has been possible for us." But the most emphatic statement concerning the limitations of his work is to be found in a printed "Prefatory note," dated 1876, which he prefixed to the unbound copies of his reports on the three seasons of field work, reprinted for personal distribution from Wheeler's Volume III, with the special title: "On the Geology of Portions of Our Western Territory Visited in the Years 1871, 1872, and 1873." The note is in part as follows:

> The observations which form the basis of these reports were hurried in the extreme. The writer, for the most part, accompanied field parties which were specially equipped for rapidity of movement and were crowded to the utmost. Moreover, in a country almost unmapped the demand for geographical information was more urgent than that for geological, and all plans and routes were accordingly, and with propriety, shaped to give the topographer the best opportunities consistent with rapidity of movement, while the geologist gleaned what he could by the way. To study the structure of a region under such circumstances was to read a book while its pages were quickly turned by another, and the result was a larger collection of impressions than of facts. That many of these impressions should be erroneous was inevitable, and no one can be more conscious than I of the fallibility of what I have written. Still I am far from counting my labor lost; for the best presentations that have been given of western geology are not free from error, and I certainly have most honorable company in my imperfection.[1]

---

[1] The remainder of the "Prefatory note" is here given, in order that the corrections which it includes may be added to the original Volume III of the Wheeler Survey by those who possess it uncorrected.

"More than a year has elapsed since the manuscript left my hands and in that time I have again visited Utah. Partly as the result of my new work, and partly by facts which have been developed by others, I have been induced to change some of my ideas and I avail myself of this occasion to make a few retractions."

"On page 132, basalt is erroneously reported to occur near the town of Salina, Utah.

"On page 44, it is stated that an orographic disturbance occurred in the northeast part of the plateau province 'before the deposition of the Cretaceous.' The unconformity which I observed I now know to have arisen after the deposition of the Cretaceous.

"On page 116, the opinion is expressed that artesian water might be found along the eastern base of the Pahvant range. Mr. Howell has since discovered a fault in the strata of that locality which greatly diminishes the probability.

"It is asserted on pages 129, 130, and 525 that the San Francisco lava-field is continuous with the great lava-field of New Mexico. The notes of Dr. Loew show that this is not so.

It is only with respect to the plateau province that Gilbert seems to have regarded his studies as fairly satisfactory, and that not so much because he made a prolonged stay there as because of "the simplicity of the structure, the thoroughness of its drainage, which [in contrast with the Great Basin Range province] rarely permits detritus to accumulate in its valleys, its barrenness, and the wonderful natural sections exposed in its cañons." There one "can trace the slow lithological mutations of strata continuously visible for hundreds of miles; can examine, in visible contact, the strata of nearly the entire geological series, and detect every nonconformity, however slight; and can study the simpler initiatory phases of an embryo mountain system." It is well to bear in mind the contrast thus suggested between the simplicity and visibility of the plateau-province structures and the complications and concealments of the basin range structures, when one examines the reports regarding them.

Gilbert wrote, besides several condensed reports of progress and a number of brief scientific articles on special topics, two final reports, dated July and October, 1874; the first covered the field work of 1871 and 1872, the second, that of 1873; both were prepared in Washington and appeared in "Volume III, Geology" of the Wheeler survey, dated 1875. The method of presentation originally proposed was largely modified, apparently as a result of Gilbert's preference for synthetic treatment. An itinerary, which had been planned to record the bulk of the observed facts in the order of their encounter, was omitted except for the barest outlines, which for the first two years are given in a two-paragraph footnote, and for the third year in a half page of text; the systematic treatment was correspondingly expanded. As Gilbert said in the preface to the first report, "General statements have been put for individual, so far as the material would allow"; and again in the preface to the second, "Wherever the facts at hand have appeared to warrant a general statement, that has been given in preference to the individual facts, in the belief that, even though it shall require future modification, it will be more readily available and in every way of more service to geological science than the enumeration of the local details that were the subjects of direct observations." The preface to the first report also makes generous reference to his associates:

> I have endeavored to acknowledge, in presenting the material, the contributions to it that have been made by gentlemen of the expedition and others; but it is proper to add that these acknowledgments fall far short of expressing my indebtedness to the work of assistants, Messrs. A. R. Marvine and E. E. Howell. The interlocking of our routes has brought their data into such relation to mine, that all my more general statements are, in part, based upon them.

Although it may be agreed that narrative records are generally less satisfactory than classified statements for the presentation of scientific studies, one must here regret the loss of first impressions made by the geology and the physiography of regions so extraordinary as the Great Basin and the plateaus on a mind so sensitive to receive them and so keen to analyze them as Gilbert's.

GEOLOGICAL GENERALIZATIONS AND CONCLUSIONS

The preceding details concerning Gilbert's first seasons of field work in the West have been given with some fullness, in order to show the conditions under which he was formally launched upon his geological career. A similarly detailed analysis of his first official reports,

"There is no good ground for the opinions advanced on pages 86 and 81 that the courses of Kanab and Paria creeks were in part determined by antecedent folds, and that the Aubrey cliff as a topographic feature antedates the Grand Cañon.

"The unhappily large number of typographical errors in Part 1 are due in part to the fact that I was absent in Utah during the proof-reading and did not see the pages until they had been stereotyped. In these few copies that I distribute myself I have corrected many of the errors in the margin. I have also restored in part some words and sentences that were suppressed in the manuscript after it passed from my hands. Certain of the restored passages are necessary to the understanding of the context and others are needed to prevent the impression that I disregarded through ignorance or discourtesy the work of other geologists.

"I have to regret that I cannot present with these unbound pages the engravings which accompany them in the official issue. I can only hope that my friends who receive this extract will obtain also the complete volume."

The chief "restored passages" mentioned in the next-to-last paragraph above, are inserted as interleaved printed slips where they belong and will be reproduced in connection with their context on later pages of this memoir. The marginal manuscript corrections are chiefly as follows: Page 41, "basin ranges" is given capital initial letters, as elsewhere; p. 57, 59, 60, "plateau" is similarly capitalized; p. 75, 8 lines from bottom, for Cherty read Aubrey; p. 83, 176, for Monument read Glen; p. 95, 6 lines from bottom, after subaqueous add clay; p. 99, four additional shell species, collected by Hayden in 1870 and identified by Tryon, are added to the five collected by Gilbert; p. 103, for Anodae read Anodonta; p. 109, 116, for San Francisco read Colorado; p. 172, for Cordillera read Basin Range; p. 173, for New Mexico read Dakota; p. 175, after Newberry add and Major Powell; the lower half of p. 183 and four-fifths of p. 183 should be included in the footnote of p. 182; p. 509, 519, after Newberry add Hayden; p. 512 7 lines from top, for northward read westward.

with respect to their contributions to geology and physiography, will be presented in this and the following sections, not only because of the evidence that they give of the rapid development of his life work, but also because of their importance in the history of geological science. Undoubtedly the best known and most frequently quoted chapters of his reports concern the mountain ranges in and near the Great Basin, to which he was the first to give the general name, basin ranges; yet his discussion of the structure, history, and form of these mountains was less complete than that of the plateaus next adjoining on the east, regarding which his excellent descriptions and well certified conclusions have been less frequently cited. This contrast appears to have a double explanation. Gilbert's conclusions regarding the plateaus agreed essentially with those reached about the same time or later by Powell and Dutton, whose fuller accounts, published in separate volumes, practically superseded his few and brief chapters. On the other hand, his conclusions regarding the basin ranges traversed those reached at about the same time by the geologists of the Fortieth Parallel survey, and therefore became widely known as the subject of a prolonged controversy, which even to-day is not settled to the satisfaction of all concerned. Gilbert's views on this question were highly original, but their first presentation was unfortunately very incomplete. They will be examined in a special section, after the geologic and physiographic problems of the plateau province have been set forth.

The chief geological topics treated in Gilbert's contributions to Volume III of the Wheeler survey reports are: Stratified rocks, their sequence, thickness, composition, and fossils, with some discussion of the conditions of the deposition, their horizontal variations, and their place in the geological series; volcanic rocks, their sequence and distribution; the structures prevailing in each of the two physiographic provinces under examination, which show, first, that the deformed rock masses of the basin ranges and their inferred strong displacement by invisible marginal faults contrast strongly with the prevailingly horizontal strata of the plateaus which are but moderately disturbed by visible faults and flexures; but which show also that the more disturbed blocks in the northern part of the plateau province represent structural transitions between the less disturbed plateaus farther south and the basin range province on the west; in a word, that the plateau province exhibits embryonic stages of the deformation which is more fully developed in the basin range province; processes of deformation, concerning which occasional brief but significant suggestions are offered; certain chapters of historical geology; the absence of general glaciation; erosional processes; and lacustrine records, chiefly those of the great extinct lake to which Gilbert gave the name of Bonneville and on which he later prepared a monograph, to be analyzed on a later page, as his chief publication under the United States Geological Survey. Fuller statements concerning some of the above topics will be given in the following sections, with page references to Volume III.

Gilbert shared with Marvine and Howell the duty of coloring geologically eight of the Wheeler survey topographical maps, prepared with hachures on a scale of 8 miles to an inch. The area covered includes parts of Utah, Nevada, Arizona, and New Mexico, and the colors distinguished eight time divisions and two groups of igneous rocks. Of course the geological boundaries are broadly generalized and without detail.

The chief physiographic results—that is, all discussions of land forms as dependent on rock structures and surface agencies—will be summarized in later sections. They concern the forms produced by stream erosion and by general degradation in plateaus of horizontal structure, by the general erosion of volcanic cones and lava fields, and by the advanced denudation of monoclinal flexures and upheaved domes; little attention was given to the description of forms exhibited by the irregularly deformed structures of the basin ranges. Two general laws of erosion were formulated and applied in a highly suggestive manner. The erosional reduction of highlands or uplands to plains was clearly recognized, and the distinction is intimated between the young forms of a later cycle of erosion introduced by the uplift of a region, and the old forms that had been previously developed in an earlier cycle.

20154°—26——9

STRATIGRAPHY

A large amount of painstaking routine observation on the basin ranges as well as on the plateaus is represented by 21 columnar sections, with measures of the thickness of successive formations and brief accounts of their composition and fossils, Meek being the authority for most of the species named (157–170). The sections include an immense accumulation of sedimentary formations, the time relations of which, conveniently exhibited in graphic form (171), extend from Archean to Quaternary. These well-generalized records are followed by a brief review of each larger time division from younger to older (172–186), which constitutes the most considerable contribution to historical geology that is to be found in any of Gilbert's reports. A brief passage of altogether exceptional nature is here included—

The genus *Cruziana* was first described by A. D'Orbigny from the Lower Silurian of South America. It has since been found in Lower Silurian strata in France and Sweden; in Primordial strata in England, Newfoundland, and Montana; in the Chazy group in Canada, and in the Clinton group (Upper Silurian) in New York. Its known vertical range is thus entirely within the Silurian and its broadest distribution in the Lower Silurian. By these facts [other fossils also being here referred to] I am let to conclude that the Tonto group [lying unconformably on crystalline rocks at the bottom of the Colorado canyon] is certainly Lower Silurian in age and probably Primordial (185, 186).

Whether this passage was borrowed from a report of the paleontologist, Meek, or whether it is a reminiscence of the Gilbert's years in Cosmos Hall is not clear, although the first-person pronoun suggests the latter alternative; but in either case the passage is peculiar, almost unique, in making repeated references to foreign localities. Such references are rarely found in Gilbert's reports. In common with other explorers of the West, he was so overwhelmed by the great mass of new facts there discovered and by the heavy labor of description, and discussion that he seems to have found no time for comparing them or their explanations with more or less similar facts and explanations previously recorded by the geologists of other continents; his work was essentially American.

HISTORICAL GEOLOGY

Among other results of importance determined by Gilbert as a member of Wheeler's survey, as well as by the geologists of rival surveys, were several generalizations regarding the historical geology of the West as contrasted with the previously established standards of the eastern United States; for example, the meagerness of the Upper Silurian and Devonian beds, the marine origin of the Carboniferous, the occurrence of Cretaceous coal—"Whenever there shall be a market for it, coal will be developed in all the indicated areas of Cretaceous outcrop" (546)—and the supposed lacustrine origin of much of the Mesozoic and Tertiary strata in the plateau province. On the last-mentioned topic an important change of opinion was made 20 years later. Curiously enough, the large-scale occurrence and presumably æolian origin of certain cross-bedded sandstones, best exhibited in what he called the Gray Cliffs, now usually known as the White Cliffs, in the northern part of the plateaus, received little attention. The sandstones were long afterwards referred to as exhibiting "superlative cross-bedding" but without a suggestion of origin.[2] Certain paleogeographic changes, indicated by variations of the stratigraphic column from place to place and presumably caused by ancient deformations and emergences, are discussed, for the most part briefly. Thus among other contrasts between the region of the basin ranges and of the plateaus is the emergence of the former in mid-Mesozoic time, so that since then, although possibly suffering progressive deformation, it has been exposed to erosion, while the latter continued to subside and to receive sedimentary deposits, largely derived from the former, as late as the Tertiary era (63, 187).

The present greater altitude of the plateau province resulted from a later movement when its long-continued subsidence was reversed to upheaval. "The Wasatch and the country immediately east of it [the plateaus] have been elevated, relatively to the adjacent portion of the Great Basin, not less than 4,000 feet since the drainage of the Great Tertiary lake" (59, 60). Gilbert's associate, Howell, made an excellent contribution to this subject: he wrote on a later page of Wheeler's Volume III than Gilbert's first report:

When the Cretaceous and Tertiary seas covered the present Plateau region, the Great Basin, as it is now called, was the continent which furnished the material for the heavy beds of rock which were then deposited.

[2] Ripple-marks and cross-bedding. Bull. Geol. Soc. Amer., x, 1899, 135–140.

The present altitude of the plateaus is due to a later uplift, after "the main folding of the range system," on fault lines at the western base and a few miles to the east of the Wasatch Range; "along these two lines have been the main movements which have reversed the position of the two systems; placing the plateau above the plains of the Basin Range system" (252, 253). The great erosion of the basin ranges here implied as following their "main folding" will be referred to again in discussing the explanation of the ranges as fault-block mountains. Gilbert was evidently much impressed by the contrast to the Appalachian revolution of the Atlantic slope that was presented by the almost unbroken continuity of deposition in the plateau province from the "Silurian" to the Tertiary; the chief break in this long sequence is a slight unconformity between the clays of the lower Trias, later called Permian, and an overlying conglomerate: the clays "were somewhat eroded by the current which spread" the conglomerate, "as is shown by the inequality of the surface on which it rests" (175).

THE GREAT UNCONFORMITY

Of wider significance is the discussion of the tremendous unconformity at the base of this great Paleo-Meso-Cenozoic series in the plateau province; it had already been recognized by Newberry and Powell, but was more fully discussed by Gilbert as follows: "In the Grand Cañon of the Colorado . . . the Tonto sandstone [the formation in which the Cruziana, above noted, occurs] rests directly on the plicated and eroded schists and associated granites, and demonstrates them pre-Silurian." Hence "the Archaean strata had been deposited, plicated, raised above water, and eroded, before the epoch of the Tonto group." The deposition of the "Silurian" sandstones with their marine fossils on the underlying crystalline rocks was therefore interpreted in Gilbert's first report to mean that the Paleozoic ocean "slowly encroached upon the Archaean continent, paring its ridges, filling its hollows, and spreading over all . . . the coarse siliceous detritus that constituted the advancing beach" (186, 187). That the encroachment of the ocean was largely due to the subsidence of a preexistent land had been previously pointed out in the statement that the Paleozoic "series, in a great number of instances, exhibits limestone at the top and vitreous sandstone (quartzite) at base, with usually shale between"; and this, following Newberry, is declared to be "the typical sequence of deposits upon a continent slowly sunk beneath the ocean" (183). The amount of sinking which continued from "early Silurian to late Cretaceous," is estimated at "no less than 8,000 feet" (187).

It is interesting to add that, although no mention is made of them in the text, one of Gilbert's sections of the Colorado Canyon wall (p. 184) correctly represents the very ancient but moderately inclined pre-Paleozoic strata, later called Unkar by Walcott, which, repeated by faults in several parts of the canyon, form east-dipping, wedgelike masses, with their under surface resting unconformably upon a remarkably smooth, slanting floor of the still more ancient crystalline rocks, while their upper surface is everywhere obliquely beveled by the great erosional plain, cutting evenly across the moderately inclined Unkar strata and the strongly plicated crystalline rocks alike, which serves as the even floor upon which the Tonto sandstones were so broadly outspread. Gilbert's section here referred to is believed to be the first published illustration in which one of the wedgelike masses is properly represented; the corresponding figure in Powell's Exploration of the Colorado River of the West (p. 212, fig. 79) is seriously incorrect and misleading.

The great "Pre-Silurian" unconformity is more broadly treated in Gilbert's second report, where its occurrence in the southeastern part of the basin range region is strikingly described:

> The break between the Archaean schists and the Paleozoic beds is strongly marked. The Archaean sediments were plicated, were tilted, and were lifted above the ocean and eroded before the Paleozoic were laid down (510).

A general statement is made later:

> There are two general facts in regard to the geological history of the great West that deserve especial mention . . . The first is that the pre-Silurian stratigraphical break is as complete and as universal in the West as it is in the Eastern States and Canada . . . And, second, there is always, at the contact, a contrast

of conditions as regards metamorphism, the Silurian rocks being, usually, merely indurated, and the Archaean invariably highly metamorphic. These two characters of the break serve to show that it represents a vast chasm of time, a chasm, the duration of which may have been greater than that of the ages which have since elapsed. A third character of the break, one that is supported by less evidence, but negatived by none, is that the lowest of the superposed rocks are conglomerates and coarse sandstones. The lowest Paleozoic rocks are Primordial, and the basal portion of the Primordial is everywhere siliceous and of coarse nature. Where the Primordial is absent, and the Carboniferous rests directly on the Archaean, a limestone has been observed at the contact; but this is a local phenomenon, the meaning of which is that certain Archaean mountains were islands in the Silurian sea, and were afterwards covered, or more deeply submerged, by the Carboniferous sea. The conclusion to be drawn from the coarse, fragmental nature of the lower deposits is that the water which spread them was an encroaching ocean, rising to possess land that had long been dry. The recognized interpretation of a widespread sandstone is continental submergence, or, what is the same thing, an advancing coast line; and where the formation is important in depth, as well as breadth, we must suspect, at least, that the shore waves sorted, not merely the detritus which they themselves tore from the cliffs of indurated rock, but other *débris*, which they found already ground, and which needed only redistribution (521, 522).

It is worth remarking that less mention is made in the second report than in the first of the work done by the advancing ocean in paring down Archean ridges; for although marine erosion was, in 1860 and 1870, the usually accepted method of preparing a flat floor for the reception of unconformable sediments, Gilbert had made much progress, as will be shown later, during his successive seasons in the field, toward recognizing that subaerial degradation could, in areas of weak rocks at least, produce a plain without the aid of marine abrasion. It is therefore significant that the abrasive action of shore waves is limited, in the second report, to their attack on "cliffs of indurated rock."

SUBMERGENCE OF THE ARCHEAN CONTINENT

Of still greater importance is a delicately worded protest, next to be quoted, against an old-fashioned teaching that was geologically orthodox in the time of Gilbert's youth—the kind of teaching that Professor Ward pretty surely gave him—to the effect that the Adirondack Mountains in New York and the Laurentian highlands to the north of them represent Archean areas which rose from, not sank into, a wide primordial ocean, and which were as a result sheeted over with a receding succession of strata as the waters withdrew from their emerging flanks. The protest is as follows:

It would be perhaps, out of place to controvert here the familiar presentation of eastern Paleozoic history as an emergence, beginning with the uplift of the Laurentian highlands, but it may be confidently asserted that western Paleozoic history is the reverse of this. There was a time when the Archaean highlands constituted islands in the Paleozoic sea, but this condition was produced, not by the emergence of these islands, as the *nuclei* of a growing continent, but by the submergence of the surrounding area, and the consequent abolition of a continent (522).

The clearness of insight and the originality of interpretation here revealed are thoroughly characteristic of Gilbert's work; and yet in spite of his convincing demonstration, the orthodox belief which it should have promptly supplanted survived for years; it was still maintained, for example, in so standard a work as Dana's Manual of Geology, not only in the third edition of 1880, but also in the fourth of 1895. There is probably no better illustration than this one anywhere to be found of the effect of vivid western facts in freeing a philosophic-minded inquirer from domination by orthodox eastern theory. As with other generalizations, to be mentioned later, this one seems to have been based less upon observations in the Great Basin than in the plateaus; for the smaller and scattered localities of Paleozoic basal unconformity found in the basin ranges are much less impressive than the majestic continuity of its exposure deep in the Grand Canyon. There truly the magnificent display is writ so plain that he must indeed run far who readeth it. Yet this novel and fundamentally important interpretation of a sinking Archean continent, a geological discovery of the first rank, does not seem to have impressed itself deeply upon Gilbert's modest mind; for when, only 10 years afterwards, a correspondent who did not have the above-quoted passage in mind, wrote to him asking if he had not somewhere expressed an opinion about the pre-Paleozoic submergence instead of emergence of the Adirondacks as an outlier of the Laurentian highlands, the reply was, "There is some mistake in regard to my opinion about the Adirondacks, for I have none. I have never seen them."

Then after a page of other matters: "P. S. I find that it is Powell who has an opinion about the Adirondacks"; as if in the multiplicity of new ideas this one had been forgotten. A later instance of a somewhat similar kind will be told in connection with the problem of laccolites.

A digression may be made here to introduce a striking statement concerning the great break between the Archean rocks and the later formations, a statement that is noteworthy for the importance of its content as well as for the strength of its induction. It is taken from a review of Geikie's Textbook of Geology, which Gilbert wrote in 1885:

The unconformability between the Archæan and the Palæozoic [it may be inferred that this classic diphthong was introduced by the editor of Nature] is not mentioned in such a way as to convey an impression of the profoundness of the chronological break. There is no known locality where a newer formation rests conformably upon the Archæan. There are few where the discordance of dip is not great. There are few where the superior formation is not relatively unaltered, and none where the inferior formation is not highly metamorphosed. So far as we know, the Archæan strata were both thrown in great folds and plicated in detail, were universally subjected to a metamorphism such as in later rocks seems to have been accomplished only at a depth beneath the surface, and were subsequently worn away upon a most stupendous scale before they received any sedimentary covering within the regions now accessible for examination. Compared with this all other chronological breaks are trivial, and we may almost say that, compared with this, all other stratigraphical breaks are local.[3]

It would appear from such a passage as this that, although Gilbert was as a rule little occupied with historical geology, he had a fine appreciation of its greater lessons.

VOLCANIC ROCKS AND STRUCTURES

Volcanic forms will be referred to on a later page; volcanic rocks and structures may here be passed over briefly, for although they were duly attended to wherever encountered, they were not in after years subjects of Gilbert's special studies. He rarely concerned himself later with the distinction of trachyte, rhyolite, and basalt; and indeed the mention of these and other kinds of lavas in his early reports is chiefly noteworthy in connection with his endeavor to test, by examination in the field, Richthofen's then recently announced "natural order of sequence" for eruptive rocks which "had been before considered almost exclusively from a chemical and lithological point of view" (131); noteworthy also because, while the repeated references to Richthofen would appear to constitute an exception to the rule that Gilbert did not cite the work of foreign authors, it is not so in reality; for the work here referred to was based chiefly on observations made by the distinguished German geologist and geographer when he was crossing the western United States on his way to China in the sixties, and his "Natural system of volcanic rocks" was first published in the memoirs of the California Academy of Sciences in 1868.

As to Gilbert's field test of the sequence of volcanic rocks in the Great Basin, basalt was always found to overlie trachyte and rhyolite, but the succession of these two seemed variable; the earlier members of the series, propylite and andesite, were rarely met (131, 132). In the plateau region "the invariable order of superposition is: Basalt, sanidin-dolerite, trachyte." A sententious statement of ponderosity hardly paralleled in later writings is made concerning the second member of this series:

The name "sanidin-dolerite" is used, for merely temporary convenience, to designate a rock of considerable importance in Arizona, which seems to fall without our present nomenclature and deserves the careful scrutiny of the lithologist. . . . It is quite possible that when, by the determination of the constitution of its matrix, it is fully defined, it will not appear lithologically entitled to a specific appellation, but the recognition of its individuality finds geological warrant in Arizona (526).

Concerning the relative abundance of different volcanic rocks, it is said that on the plateaus basalt covers large areas in relatively thin sheets and there rivals trachyte in abundance; but in the Great Basin basalt, although assuming an apparent importance on the map from being of latest date, shrinks into insignificance when its volume is compared with the massive eruptions of trachyte and rhyolite; these rocks, rising from few issues, have formed huge bosses, often of great thickness, divided by few or no bedding surfaces. Even though now much reduced by erosion, they still remain in immense masses (127, 128). The use of the German word, "Trass," for a volcanic ash deposit (540) was probably an echo of Cosmos Hall.

[3] Nature, xxvii, 1885, 261.

The considerable duration of the basaltic period—the latest phase of volcanic activity on the plateaus—and the recency of its last eruptions are shown by the contrast between the erosional isolation of several basalt-capped tables and the freshness of certain neighboring cones and flows. Thus the extensive flows that proceed from the many cones named the Marcou Buttes, in western New Mexico, "follow the present surface, and are almost incomparably newer than the Acoma and Mt. Taylor plateaus, the clift borders of which surmount the recent flows by 500 or 1000 feet." Powell is quoted—and this is of interest as showing the free interchange of results between the two explorers before either of them had published his report—regarding the lava cap of Uinkaret Mountain, the body of which is a "Triassic island" or remnant of the Carboniferous "terrace" north of the Colorado Canyon; and from this Gilbert draws the inference that, since the capping flow was erupted, the Vermilion Cliffs of Triassic sandstone, 1,500 feet in height, have retreated 30 miles; he adds that younger cones "dot the intervening plain" (136). The youngest eruption known to Gilbert is found in a group of cones on the Sevier Desert in the basin range region, of which he gives a detailed account. Some of them—

may fairly be called modern, although there is no tradition of their eruption . . . Only the consideration of the extreme aridity of the climate can countenance the possibility that centuries, instead of years merely, may have elapsed since the termination of this eruption . . . Indeed, when we compare the stupendous denudation that has transpired during the period of basaltic vulcanicity in this region, with the differential film that has been removed since this last manifestation, and when we consider, in addition, that intermittence is a characteristic of volcanic activity, we are not merely permitted to think of a renewal of that activity as possible, but logically compelled to regard it as probable (136).

He elsewhere wrote, regarding the recent cones in the Sevier Desert:

In passing, it may be noted, for the benefit of those who base theories on the littoral distribution of volcanoes, that this locality is six hundred miles from the Pacific ocean.[4]

More pertinent to Gilbert's other work is a conclusion concerning the dynamics of volcanic eruptions, which is stated, after an account of some of the larger volcanic structures, as follows:

It is well worthy of note that the majority of these eruptions among the Plateaus rest upon nearly level strata, and that where they are associated with inclined strata, such inclination is seen to pertain to a structure extending far beyond the volcanic outburst, and evidently not dependent on it as a cause . . . This remark applies not merely to the eruptions of basalt, which we know from the narrowness of its dikes and the easy slope of its currents to have been usually a tolerably thin fluid, but also to the most viscous trachyte, which, in the case of San Francisco mountain, for example, has been built, not a scoriaceous mass, but a pyramid of compact lava, to a height of nearly 5,000 feet, with slopes of 10° and 20°. It is by no means impossible, it is probable, rather, that in upheaved ranges, uprising lavas sometimes force apart rock masses, already greatly dislocated so as to open the broad fissures, in which their dikes are occasionally found. But the idea that the ridges of corrugation are lifted by the eruptive rocks that are associated with them—an idea that finds frequent expression in the phrases "upheaved by trap," "upheaved by granite"—appears deserving to be laid on the shelf along with the cognate idea of "craters of upheaval" (130, 131).

In spite of this well-grounded protest, the idea of "volcanic upheavals" is still strongly rooted in the popular mind, as if it were an established geological principle. On the other hand, not only did Gilbert's later work on the Henry Mountains give much countenance to the possibility of surface upheaval by underground intrusions, but in another part of his Wheeler report the deep-seated forces which caused large eruptions in the province of the basin ranges are held to be identical in their subterranean loci and in their action with the deep-seated vertical forces which caused the upheaval of the range fault blocks, as will be pointed out more fully on a later page.

DIASTROPHISM: FRACTURES AND FLEXURES

The displacements of the basin range province will be discussed in a later section. As to the neighboring plateau province, Gilbert, somewhat later than Powell, discovered that some of the great north-south blocks into which the province is divided are separated by rather

[4] Proc. Amer. Assoc. Adv. Sci. for 1874, 1875, Pt. II, 30.

clean-cut fractures which, when traced to the natural section afforded by the east-west course of the Grand Canyon, are found to have vertical planes; but other blocks were seen to be separated by monoclinal flexures, which, as they sometimes become fractures when followed along their length, were closely associated with faults; and concerning this kind of displacement it was said in a special article:

> The monoclinal fold, barely recognized by geologists heretofore, but here the predominant structure, is one of the simplest elements of corrugation, and the fruit of its careful study cannot fail to be of great import to the student of dynamical geology.[5]

Inasmuch as certain flexures, when followed along their length, become fractures, the familiar term, fault, was used with "a somewhat more extended meaning than the one ordinarily given to it," so as to include both kinds of displacement, and no reason was seen why, "regarding the phenomena as the results of a slow-acting force, we may not suppose that in depth, as well as longitudinally, the relation and alternation of fractures and flexures will depend on the nature and condition of the beds affected" (56).

As to the proximate cause of these displacements, Gilbert was most specific. Although he showed that volcanic eruptions did not as a rule heave up the strata through which their lavas rise to the surface, he was nevertheless convinced that forces of vertical upheaval, not of horizontal compression, were responsible for most of the displacements detected in the nearly horizontal fault blocks of the plateau province, as well as in the more strongly tilted fault blocks of the basin ranges. It was recognized that the vertical displacements of the huge plateau blocks may have been in some cases accompanied by small horizontal movements, resulting in a slight diminution of the breadth of the region, "but it is impossible . . . to suppose that the vertical movements have been caused by lateral pressure applied to the strata in which they are manifested. Whatever the place and mode of the remote cause, the immediate acts vertically and from some position beneath the strata we are to examine" (56). The relation of the displacements to the rigidity of the displaced strata is studied. It is at first noted that "stupendous blocks of rock, ten, twenty, or even thirty miles in diameter, and of unknown depth, have changed their relations to other blocks, with which they were once continuous, and have themselves remained rigid, all evidence of movement being at the common boundaries of the dissociated blocks" (558). It is then suggested as a corollary that "the meaning of these movements of the earth, in vast but limited masses, is, that rigidity is an important factor in the determination of the superficial manifestations of subterranean movements." In the case of flexures, "the fact that, at points of differential movement . . . the rocks were not fractured but were flexed, proves that changes were of secular slowness, and the rigidity that resists secular applications of force . . . demands for its interpretation that we shall grant to the rigid masses a depth commensurate with their superficial dimensions, and suppose that the forces which move them are situated still deeper" (559).

UPHEAVAL OF THE ZUÑI DOME

The fine example of diastrophism seen in the Zuñi Mountain dome close to the western border of New Mexico—later studied by Dutton and now familiar to many travelers on the Santa Fe Railway which crosses its northern margin at the continental divide—attracted Gilbert's particular attention, and was explained as due to the "upward transfer of subterranean material" which did not penetrate its cover. The domed structure involves a deep body of fundamental Archean rocks with a heavy cover of Carboniferous, Triassic, and Cretaceous strata, now partly removed, as further stated below; so that while the residual mountain mass measures only 45 by 20 miles, with a local height of 3,000 feet, the original dome would measure 70 by 35 miles, with a height of 6,000 feet. The reconstructed dome is contrasted with the volcanic cone of Mount Taylor, 30 or 40 miles to the east; in that mass, "the rising rock passed through the superstrata, and, piling itself on the surface, built a mountain of its own substance; in the other it moved a comparatively short distance, but lifted all above it, and built a mountain by

[5] Proc. Amer. Assoc. Adv. Sci. for 1874, 1875, Pt. II, 35.

upcurving the superficial strata" (504). In spite of the small vertical distance of the sub-Zuñi rock transfer, the magnitude of the transfer far exceeded that in the companion volcanic eruption; the reconstructed Zuñi dome was calculated to have a volume of 700 cubic miles and the sub-Zuñi rock transfer must have been of similar measure; but the volume of the Mount Taylor cone was estimated to be only about 60 cubic miles.

An example of Gilbert's graceful phrasing follows: Although "assumed by earlier explorers to be a continuation of the Sierra Madre of Mexico . . . the Zuñi range, far from deserving to be entitled the mother of a family of mountains, is a lonely orphan, dissevered from all kindred. It stands, in the midst of the plateau region, a mountain of upheaval; from every side of it the strata stretch in level tables. . . . It is truly a mountain by itself, and in its isolation, in its accessibility, in its simplicity of structure, and in its relation to the local system of the plateaus, it offers a richer harvest to the geologist, who shall give it a thorough study, than any other single mountain with which I am acquainted" (563). The removal of the weaker covering strata, the topographic effects of which will be further considered in a later section, has laid bare the harder core:

"The rapid destruction of the lower Trias shales, and the stubborn resistance of the underlying [Carboniferous] limestone, have led to the baring of a broad area of the upper surface of the limestone, and this great exposure of a single stratum reveals some details of structure that could not otherwise be comprehended without laborious study. The rock appears to be divided into blocks of such magnitude that their superficial areas would be expressed in miles rather than in acres, and these blocks have been inclined with somewhat different dips and directions, so that at their edge they differ in altitude. So far as my limited observation goes they are not separated by faults, but are connected by monoclinal flexures . . . of small throw, and separated by wide intervals. They are not parallel, but bear toward all points of the compass and intersect each other . . . Their study cannot fail to throw light on the function of rigidity as a factor of orographic corrugation" (566).

The Zuñi dome, which was not seen until the third season of field work, gave support to the view that Gilbert had previously reached in his examination of the basin ranges and of the faulted plateaus; for it was concluded that although, as a remote cause of the domelike upheaval, "there may have been horizontal motion of subterranean matter, the immediate cause could only be an upward motion" (564); and this goes well with the conclusion announced in the chapter on the basin ranges in the report on the first and second field seasons:

The movements of the strata by which the ridges have been produced have been in chief part vertical along planes of fracture, and have not involved horizontal compression (42).

Gilbert's tendency toward quantitative methods in geology, which like much of his careful reasoning may be regarded as an outcome of his capacity and training in mathematics, is illustrated not only in the above estimate of the volume of the Zuñi upheaval, but in another aspect of the same problem. He wrote, regarding the deformation of the dome:

If, as is probable, the strata of the Carboniferous limestone are continuous across the Archaean near its crest . . . then the absolute length of the curved strata can be measured and compared with the direct distance between their remote parts; and there is reason to hope that, by a series of such measurements in different parts of the range, an answer can be found to the question whether, in the production of the curve, the remote portions of the strata were brought nearer, or whether the curved portions were stretched (566).

THE UPHEAVAL OF MOUNTAINS

It may be pointed out that Gilbert's views about the upheaval of the Zuñi dome were in a certain sense reactionary, for they were announced at a time when, after long discussion, vertical forces, which were the mainspring of geological movements in an earlier stage of the science, had been largely replaced, especially with respect to mountain masses, by forces of lateral compression. While his views concerning the vertical upheaval of fault blocks in the plateau province did not encounter opposition, it is probable that the outspoken dissent from his explanation of the basin ranges also by vertical forces was favored by the geological theories most in fashion at the time his reports came out.

Reactionary as Gilbert's views in this matter were, they were less revolutionary than a later explanation for high-standing plateaus advanced by Suess, to the effect that such plateaus stand high because the surrounding lands, once at similarly lofty levels, had later subsided. Had this explanation come to be accepted, Gilbert's views would have been wholly contradicted; but as a matter of fact his reactionary views were prophetically correct, inasmuch as they have come to prevail to-day not only for the plateau and the basin range provinces to

which he applied them, but much more extensively; for by following certain physiographic principles in the development of which Gilbert was much concerned, it has been discovered that the present height of many if not most mountain systems is due largely to one or more broad upheavals of relatively modern date, sometimes accompanied by strong faulting but not by marked folding, while the pronounced folding of the mountain rocks must be ascribed to horizontal compressive forces of date so ancient that the relief and height then given to the writhing surface had been greatly reduced if not obliterated by long periods of erosion before the later upheavals gave the worn-down mountain mass its present height and permitted its present deep dissection; hence many mountains that we actually see as existing topographic features seem after all to owe their visible height largely if not wholly to vertical forces.

HOT SPRINGS AND DIASTROPHISM

In spite of the confidence that Gilbert exhibited in the action of vertical forces, he undertook a study of the distribution of hot springs in the United States with the object of determining whether their high temperature was due to the development of subterranean heat by mountain corrugation under horizontal pressure, as "advocated by Messrs. Hunt, Mallet, and Le Conte" (146); this citation being one of the few references to other geologists than those who had worked in our western field. The result gained by charting nearly 150 hot springs was that their distribution coincides "very exactly with that of corrugation" (148); but the hot springs of the Appalachian region, where corrugation and eruption are things of the past, are regarded as depending on mountain-making deformation only in so far as it furnished fractures by which their waters might reach the surface; their temperature is ascribed, following Rogers, entirely to the normal increase of earth-crust temperature with depth. It is only in the West, where corrugation and eruption "have persisted to so late a period that we have good reason to believe they have not ceased" (148) that heat due to rock crushing under the action of mountain-making compressions is believed to control the occurrence of hot springs; and the hottest ones are found in volcanic districts.

Gilbert's views as to the diastrophic relations of the apparently unlike provinces of the basin ranges and the plateaus is clearly expressed in the following extract. After pointing out that in both provinces the deforming forces were deep-seated in position and nearly vertical in direction, he goes on:

> A single short step brings us to the important conclusion that the forces were identical, (except in time and distribution); that the whole phenomena belong to one great system of mountain formation, of which the ranges exemplify the advanced, and the plateau faults the initial stages. If this be granted, as I think it must, then it is impossible to over-estimate the value of this field for the study of what may be called the embryology of mountain building. . . . The field is a broad one and its study has but begun; but with its progress I conceive there will accrue to the science of orographic geology a more valuable body of geological data than has been added since the Messrs. Rogers developed the structure of the Appalachians" (61).

THE COLORADO PLATEAU AS A FIELD FOR GEOLOGICAL STUDY

An excellent summary of Gilbert's ideas about the plateau province is to be found in an article with the above title that was published after he had taken a position under Powell, whose influence is repeatedly manifest, although the influence had not then gone so far as to cut off the terminal -al from geological. Extracts from the article are presented here, but it deserves attentive reading as a whole, so forcibly did it set forth a large body of new knowledge. Both the climate and the drainage of the region favor its study. As to climate; the thinness of the soil and the usual absence of trees facilitate observation to a degree unsuspected by workers in a moister climate:

> From a commanding eminence one may see spread before him, like a chart to be read almost without effort, the structure of many miles of country, and in a brief space of time may reach conclusions, which, in a humid region, would reward only protracted and laborious observation and patient generalization. There is no need to search for exposures where everything is exposed.

As to drainage:

> The Colorado and its branches flow across the Plateaus in deeply carved, narrow cañons. . . . Empowered by the rapidity of its descent, each tributary river has carved a cañon of its own, and so too has each branch and creek tributary to a river, until the whole tract is divided by a labyrinth of ramifying cañons. . . . Thus does drainage conspire with aridity to prepare for the geologist a land of naked rock.

The material for study is then summarized under four heads; mountain building by displacement, mountain building by eruption, stratigraphy, and erosion. As to displacements in the plateau province—

faults and folds abound through its whole extent, but they are comparatively of great simplicity. They are indeed so simple that they can be completely known. Their entire phenomena may be comprehended, measured, described and delineated. The course of many a fault can be traced from end to end, and its throw measured at every step. The form of many a fold can be determined throughout, and pictured or modelled in miniature, with every detail of flexure.

Special attention is given to the monoclinal fold, then a novelty, although in the plateau province it is "a characteristic type of displacement and is rivalled in frequency only by the fault." Similarly, the huge blocks of earth crust, dislocated but undeformed between the flexures or faults, are presented as a class of displacements "so little known heretofore that it has not found place in the manuals of geology." It is believed that when all blocks are studied out, the generalizations reached "will not be inferior in value to any single contribution that has been made to our knowledge of the results of orogenic movements."

As to mountains resulting from eruption, two types, the Uinkaret and the Henry Mountains, are presented in addition to the ordinary volcano. The Uinkaret type, as described by Powell north of the Colorado Canyon near the western border of the plateau country, consists of ancient lava flows, eroded in the form of high mesas and then mantled by later flows "so as to give the appearance at first glance of a range made up entirely of volcanic matter." The Henry Mountains, which Gilbert was then studying and concerning which a full account will be given here in a later chapter, were briefly stated to consist of "a number of bubble-shaped domes, one for each individual mountain of the group." Both these novel types are declared to "diverge most widely in character from those with which geologists are already familiar."

Stratigraphy is briefly treated. By reason of the numerous deep canyons strata may be examined "not merely along a simple line, but throughout an extended area. With such exposures . . . the history of a system of sediments can be made out with a completeness that surely can not be excelled elsewhere." Yet in spite of their visibility, neither in this article nor in other reports are novel results announced regarding stratified formations. The problem of deposition was little considered as compared to the problem of erosion. This problem, evidently a favorite with Gilbert, is given a detailed analytical treatment quite unlike that of the three preceding topics but closely similar to that of the chapter on "Land sculpture" in the Henry Mountains report, to be summarized below. Perhaps the most significant statement here is that concerning the contrast between the widespread degradation which the uplands of the province have suffered, estimated at 5,000 feet, and the depth of erosion in the narrow canyons, which is of similar amount; but hardly a hint is given of a great movement of elevation between the times of broad degradation and deep erosion.

CHAPTER V

PHYSIOGRAPHY IN THE WHEELER REPORTS

THE GRADUAL GROWTH OF PHYSIOGRAPHY

It has often been remarked that the early geological explorers of the West were impelled to consider surface forms in close association with underground structures because of the manifest relations of the two in a treeless region. This was never truer than in Gilbert's case. He does not appear ever to have had any teaching in physical geography; and indeed, even if he had, the subject as it was taught in his boyhood and youth, 60 years ago, would have given him little or no understanding of the causal connection between the rock structures beneath the surface forms and the surface forms of the rock structures. Furthermore, his two years' experience on the drift plain of northwestern Ohio can not have helped him much to learn the dependence of form on structure, although his keen power of observation and his analytical turn of mind did enable him to gain an understanding of certain surface features in that district of faint relief, as has been told above. But his three seasons in the West made him a physiographer.

It is curious to recall, when one reviews the relations between geology and physical geography for something more than a century past, how promising a beginning of close association between the two sciences was made, when both were young, by Hutton in his Theory of the Earth; how far apart they drifted 50 years later when geology, as it seasoned and matured, nevertheless became largely the science of the crustal structure and history, while physical geography was stagnating in empiricism; and how intimately the two rejoined each other after the exploration of the West began. For geology, then recognizing that it must attend to the surface of the earth as well as to its under structure, gave new life to the physical geography of the lands, and this vivifying influence was happily applied at about the same time that the doctrine of evolution enlivened and invigorated all aspects of organic geography. It is gratifying to see how largely Gilbert's western work contributed to the extension of the physiographic line of geological investigation into the rich but little cultivated field of the geography of land forms.

Hutton was a leader among those who, 130 years ago, at the beginning of the association between geology and geography, set forth in very simple terms certain relations between structure, erosion, and form. Had the foundation then laid been continuously built upon, the physiography of the lands would not have been so modern a science as is actually the case. The old master pointed out that, in various mountain forms,

> we find the original structure of the mass influencing the present shape in conjunction with the destructive causes. . . . Now, this original shape is no other than that of beds or strata of solid resisting rock, which may be regularly disposed in a mountain, either horizontally, vertically, or in an inclined position; and those solid beds may then affect the shape of the mountain in some regular or distinguishable manner. . . . Thus, a horizontal bed of rock forms a table mountain. . . . An inclined rock of this kind forms a mountain sloping on the one side, and having a precipice on the upper part of the other side, with a slope of fallen earth at the bottom. . . . Were it vertical, again, it would form a rocky ridge extended in length, and having its sides equally sloped' so far as the other circumstances of the place would permit. Therefore, whether we suppose the mountain formed of a rock in mass, or in that of regular beds, this must have an influence in the form of this decaying surface of the earth, and may be distinguished in the shape of the mountains.

The observant Scotch theorist was indeed so convinced of the truth of the principle that form is dependent upon structure that he reversed it and inferred structure from form:

> In distinguishing, at a distance, those regular causes in the form of mountains, we may not be able to tell, with certainty, what the substance is of which the mountain is composed, yet, with regard to the internal structure of that part of the earth, a person of knowledge and experience in the subject, may form a judgment in which, for coming at truth, there is more than accident; there is even more than probable conjecture.[1]

[1] Theory of the Earth, Edinburgh, 1795; pages 411, 412, 413.

What a leap earth science would have made at the end of the eighteenth century if this founder of the uniformitarian school could have seen the plateaus of northern Arizona?

There was, however, no lack of opportunity for the application of Hutton's physiographic principle in various well-known parts of the Old World. Northeastern France is unparalleled as a field for the study of those unsymmetrical ridges, which are coming to be known as cuestas, "sloping on the one side, and having a precipice on the upper part of the other side," because they are developed on a series of gently inclined strata; yet although these cuestas have long been familiarly known in their geological relations, they have been rarely studied physiographically, perhaps by very reason of their familiarity. England also affords excellent and early known examples of the relation between structure and form in the cuestas that traverse its southeastern half, to say nothing of such striking though small features as the typically Alleghenian zigzag by which the ridge known as the "Wenlock Edge" in Shropshire is offset and extended southwestward past Ludlow near the Welsh border; but with the development of geological specialists, first in stratigraphy and paleontology and later in petrography, the close examination of small rock outcrops that these sciences demanded seems to have distracted attention from a broad grasp of the plant-covered landscape in which the structures, fractionally disclosed in the outcrops, are often generalized. As to English geographers, they were indifferent to geology through most of the nineteenth century and treated cuestas and other physiographic features empirically if at all. It is true that Lyell ably applied and extended through the middle of the nineteenth century the principles of uniformitarian geology that Hutton had established at the end of the century before, but Hutton's beginnings in rational physiography had no equally eminent exponent to carry them forward; witness the slowness of British geographers to recognize the chalk escarpments around the Weald in southeastern England as cliffs of subaerial denudation, not of marine abrasion; witness also their failure to describe the beautiful and familiar embayments of Cornwall on the Atlantic coast of British Europe as partly submerged valleys, until after this simple explanation for such features had been applied by Dana, as a member of the United States exploring expedition under Wilkes, to the far-away Pacific coast of British America.

During the same period in the New World, the influence of the New York survey under the leadership of Hall was dominantly stratigraphic and paleontologic; and even the brilliant correlation of structure and form established for the Alleghenies of Pennsylvania by Rogers and Leslie had, in their time, no extended application elsewhere; perhaps because it was not accompanied by a sufficiently rational treatment of deformational and erosional processes. It was not until the West was penetrated that, in the absence of heavy vegetation, the relation of surface form and underground structure, manifest for miles around in every extended view, came to be understood as the systematical result of erosional processes acting slowly and persistently through long-lasting time; and by none of the western explorers was this relation more helpfully explained than by Gilbert. Apart from his views on the origin of the basin ranges, it was in this physiographic field that his early contributions to earth science showed the most originality.

Gilbert's physiographic work naturally found its best opportunity in the treeless parts of plateau province, where the sequence from simple to moderately complicated structures is so clear, where the application of erosional processes in the production of surface forms is so immediately visible, and where the change of eroded form with the change of structure and the passage of time is so convincingly manifest. The rational treatment of land forms, already begun elsewhere, here progressed rapidly; and the results to which such treatment soon led are now so widely accepted under the evolutionary philosophy by which modern geography is like other sciences dominated, that the present generation of geologists and geographers may find it difficult to realize how large a share of the fundamental principles on which the physiography of the lands has grown and flourished were implied or outlined or formulated no longer ago than in Gilbert's first report. Young as he then was, he was either abreast or ahead of the earth science of the time.

However, Gilbert was not the first observer to detect the main features of the plateau region; that had been done by Newberry, Gilbert's chief in Ohio, who, as geologist of the Ives expedition to the Colorado River of the west in 1857–58, had crossed the plateaus south of the Grand Canyon, and had reached well-grounded views on many points. He saw the fundamental crystalline rocks deep in the canyon, lying unconformably beneath their heavy sedimentary cover; he understood the importance and efficacy of ordinary erosional processes, not only in the excavation of narrow canyons beneath the plateau by larger or smaller streams but also in the broad recession of cliffs upon the plateau surface; indeed, he regarded the opening of broad upland valleys, such as that of the Little Colorado above its canyon, as "a much grander monument of the power of aqueous action than even the stupendous cañon of the Colorado." Great honor is due to one whose vision was so broad! Gilbert had also been preceded by a few years in the plateau province north of the Grand Canyon by Powell, where the senior explorer had gained an understanding of its extraordinary cliffs of erosion and cliffs of fracture somewhat earlier than the junior, although their reports were published in the same year. But there was never any question of priority between these two comrades in science; they shared their facts and their fancies in perfect confidence, and each always felt that his results were at the service of the other. Indeed, their results were so freely interchanged that neither one knew or ever sought to claim just what he had contributed to the total, as will appear later.

PROCESSES AND PRODUCTS OF STREAM EROSION

The processes of erosion are so intimately associated with the forms that they produce that the two may be well considered together as we select the most significant items concerning both topics from Gilbert's early reports. As to processes, emphasis was given to the importance of suspended sediments in river scouring, the source of the sediments being beautifully analyzed (71, 72). The contrast between the plateau province and the Great Basin with regard to stream erosion was clearly brought out; in the plateaus the valleys have been formed by erosion and are still being deepened, while the tables between the valleys are residual; in the Great Basin the intermont valleys are residual between mountain ranges uplifted in parallel lines, and the valleys, initially of greater depth, have for a long time been filling with detritus eroded in and transported from the mountains (63). Among other items of interest is an appreciative but brief account of piedmont detrital fans, features that, although familiar enough to-day as characterizing the basin ranges, were little known to eastern geologists and geographers 50 years ago:

> The débris of the mountain is brought to its margin in gorges or cañons, from the mouths of which it is spread in broad, low talus-cones, which make up the foot-slope. The stream that flows from the cañon, whether transient or perennial, distributes the detritus over the cone by shifting its bed from time to time as the sediments clog it. As the cañon wears deeper at its mouth, and the stream discharges at a lower level, the upper portion of the cone is excavated and a new one is modeled with lower apex and lower grade (65).

To-day, a writer might pass over such matters as familiar to the point of being trite, but such was not the case 50 years ago.

It was, however, chiefly from the plateau province that Gilbert took his examples of erosion. He evidently reveled in the opportunity for the investigation of surface forms that this extraordinary region afforded. He wrote of it:

> The cañons of the Colorado and of its tributaries, and the country which they intersect, are unsurpassed as a field for the study of river denudation. Not merely do they exhibit the grandest and most impressive results, but they show the agent by which they have been wrought, still in vigorous activity (67).

Hence, although the theory that profound canyons have been eroded by their rivers was not regarded as a fully established truth by all geologists at the time of the Wheeler survey, Gilbert, like his predecessors and associates in western exploration, found the field evidence for this theory so convincing that it was illustrated rather than discussed in his reports. The most cleftlike canyon that he examined was that of the Virgin River, now included in Zion National Park, near the western border of the plateau province, about 100 miles north of the Grand Canyon. While passing through this cleft, 2,000 feet deep in massive sandstones, "the

most wonderful defile" it had been his fortune to behold, he noted that "many times our upward view was completely cut off by the interlocking of the walls, which, remaining nearly parallel to each other, warped in and out as they ascended." His section of this defile, reproduced on the cover of Leconte's Geology, has been mistaken by some as representing the Colorado Canyon.

NORMAL HANGING VALLEYS

Hanging lateral valleys were detected in the narrow incision of Zion Canyon, for many of the side canyons "at their mouths are not cut so deep" as the main cleft, and "discharge at various heights above the river"; and these hanging side canyons were acutely adduced in an important argument: for beneath their mouths, the sandstones of the main canyon were seen in "perfect continuity and integrity . . . a continuity that cannot be seen in the main cañon, since its bed is everywhere covered by detritus." The unbroken continuity of the sandstones beneath the side canyons was evidently taken to certify to their similarly unbroken continuity below the main canyon bed, and thus to warrant the interpretation of the canyon not as an example of a fracture, but as "an example—and a peculiarly differentiated example—of downward erosion by sand-bearing water. The principle on which the cutting depends is almost identical with that of the marble saw, but the sand grains, instead of being imbedded in rigid iron, are carried by a flexible stream of water." Inasmuch as the plateaus were found to be traversed by several great faults, this critical argument for the nonfaulted structure of the heavy sandstones cleft by the Virgin River is all the more pertinent. It is noted further that, as the side canyons are "worn by smaller streams . . . their bottoms are of steeper grade" (79), but it is not explicitly stated, that for the same reason, their depth is less than that of the main cleft; yet this principle was surely understood. In any case, hanging lateral valleys of normal erosion had rarely been described in those days, and had been still more rarely mentioned so understandingly in published reports.

CATARACTS AND RAPIDS

Erosional processes and principles were so grandly exemplified by the Colorado River in its canyon that, although many of them were fairly understood by the few explorers of the region, they still merited explicit statement for the benefit of geologists and geographers in general. The extinction of cataracts, for example, is introduced by a reference to the prediction concerning the future of Niagara made by Hall, whom Gilbert had known in Albany, to the effect that "there will come a time when the fall can no longer be maintained . . . and will be replaced by a rapid." Then applying this principle, Gilbert remarks:

> In the Grand and Marble cañons [of the Colorado] this stage has been reached, and the whole descent of 1,600 feet [along the river] accomplished entirely by rapids. The stratigraphic conditions to the formation of a cataract are indeed not wanting . . . But the river passes the hard beds and the soft with almost equal pace . . . At no place does the river fall from a ledge of rock into a pool below (75).

Evidently the exceptional cascades found by Powell where dams had been formed by lava streams that plunged down the side walls after the canyon had been eroded can not have come within Gilbert's field of observation.

The numerous rapids which beset the river, and which, by a literal interpretation of a passage quoted above, might be regarded as almost extinguished, hard-stratum cataracts, are shown to be of other origin. "Some of the most violent rapids" are due to large rock masses which have fallen from the walls, so that they "locally obstruct the channel"; but the majority of the rapids are found, as Powell had noted, where the occasional floods of steep tributary canyons sweep down blocks of rock, sometimes 10 or 15 feet in diameter, and drop them at their mouths, thus half closing the river channel with bowlder deltas or—

> dams, that must often be of great depth. Over each of these the water [of the main river] finds passage at the edge opposite the tributary, and descends the lower slope with swift current and broken surface. . . . To roll, jostle, break, and finally grind up and remove these boulders is the task—perhaps the chief task—of the river, and until it removes them it can perform no work on the solid rock which underlies. . . . In the current cycle of events within the gorge [canyon], there are times when each of these dams in turn is removed. . . . While the dams will occur at the same localities and with the same characters, they cannot be regarded as strictly permanent (71).

The similarity of the results here presented with those reached by Powell is manifest, and it is explained in Gilbert's reprint of his reports by an interleaved addition to a footnote on page 70, in which the localities that he visited are named as follows:

I take this occasion to specify the data acquired by my own party that I may at the same time credit to the observations and photographs of my friend Professor Powell and his party whatever other information is communicated in these pages upon the Grand Cañon. We have interchanged ideas so freely in conversation that I find it impossible in writing to avoid basing conclusions in part upon his unpublished material.

The intimate relations begun between these two explorers when they were members of different surveys was long continued.

GRADED RIVERS

The conception of a river at "grade"—to introduce at once a term that Gilbert himself suggested some 30 years later—is recognized as involving a permanently maintained rather than a slowly varying gradient: As "the river sinks deeper below the plateau, there will accompany a gradual diminution of the inclination of its bed, of the velocity of its current, and, in consequence, of its erosive power, until finally it can no longer clear its bottom of introduced detritus, and, its downward progress being arrested, the widening of the channel will begin" (75). The fact that the load of detritus to be transported will decrease in quantity and in coarseness of texture with the advancing age of the river and thus permit a long-continued diminution of its fall was not perceived. Indeed, in view of the statement that the amount of detritus "abraded from the bottom of the cañon is too insignificant to demand more than mention" in comparison with that which is received by rock falls and slides from the walls and by inwash from the side streams, Gilbert might be supposed to have thought that the arrest of downward erosion is already nearly reached by the Colorado; but on this point his opinion was clearly otherwise:

Of the time that will elapse before this consummation we can form little conception, but it can hardly be less than that consumed in the excavation already accomplished, so slowly will the work proceed as it approaches completion.

He then turns to a quantitative consideration of the duration of river erosion, a subject which he had already treated at Cohoes, in New York, and to which he returned in later years at Niagara.

Of the time already consumed [in the erosion of the Colorado Canyon] we may some time have an approximate estimate in years, for so rapidly does the sand carve away the rock, that I believe it perfectly feasible to ascertain its rate by observation, and, by considering what part of the rock-bed is exposed and what protected, to assign, within reasonable limits, the present rate of degradation of the cañon. To pass from this to the average past rate would require the consideration of somewhat involved conditions, and the result would not be so satisfactory as that obtained from the secession [recession?] of Niagara Falls, but it would be of great interest to obtain even a crude estimate in centuries of a period of time commencing, as I believe, before the close of the Tertiary age (75).

Two comments are suggested by the above extracts. First, that the brief mentions of Hall and Niagara, both known in Gilbert's earlier experience, are among the few references made to the studies of other observers or to the features of other rivers. Second, that several of the principles concerning the erosional activities of rivers, which are enunciated as if they were novelties, had been previously recognized and announced by European observers. Hence in departing from a narrative of his journey and adopting a more generalized form of report, the young geologist incurred responsibilities that he did not altogether meet. But it is an old story that our earlier scientific explorers of the West were so engrossed with the results of their own observations that they had little or no time to explore the results already gained by other explorers in foreign fields.

CLIFFS AND SLOPES IN CANYON WALLS

Although not often quoted, Gilbert's explanatory account of the relations between the stronger and weaker strata of the plateau and the cliffs and slopes in the side walls of the Colorado Canyon, published in the same year, 1875, with Powell's discussion of the same subject in his

"Explorations" of the river, must be regarded as the more explicit of the two. Gilbert's recognition of the general relation between stratigraphy and topography is nicely shown in his numerous columnar sections, referred to above, for they are indented on one side so as to distinguish the cliff-making and the slope-making strata; an excellent device that might well be generally followed. With regard to the Grand Canyon, the general statement, "In every profile of the cañon the positions of the hard-massive beds are marked by precipices, and of the soft by slopes" (68), is followed by detailed descriptions and illustrated by true-scale cross sections of the canyon in different parts of its length (69); and these sections include also several examples of a third element of canyon form, namely, cliff-top platforms or terraces, which are so clearly represented that, although they receive no specific explanation in the text, it can hardly be doubted that they were understood to have resulted from the faster recession of medium-strong cliff-making strata that overlie the back of the platform as compared to the slower recession of the stronger cliff-making strata that underlie its front. Regarding the intricate pattern of the side canyons and of the cliffs in their separating spurs, as seen in plan, it is natural that nothing was said in the early days of physiography.

Another and less generally understood principle of valley erosion was also recognized: The transformation of a narrow canyon into a well-opened valley where an eroding river, as it is followed downstream, passes from a body of strong cliff-making strata into an underlying body of weak slope-making strata. Thus the change from the cleftlike Zion Canyon of the Virgin, cut in massive sandstones, to the following open and habitable stretch of its valley is explained as occurring where the river cuts down into the underlying variegated marls before it again canyons in the still lower limestones (79); similarly, the Colorado, crossing the same series of weak strata where they are gently inclined against its flow, has a broadly opened valley at the mouth of Paria Creek, between two narrow and deep canyons, the one upstream being cut in the massive overlying sandstones, and the one downstream in heavy underlying limestones.

RETREATING ESCARPMENTS

The plateau province is well described as being "divided into a series of great terraces, by lines of cliffs trending east and west, facing south, and composed severally of the harder strata of the geological series" (44) from Tertiary to Carboniferous; the broadest terrace being the lowest member of the series in which the Colorado Canyon is cut. Each line of cliffs is described empirically in some detail. In view of the clear explanations already given for the erosion of canyons and for the forms of their walls, it is surprising to find here a less explicit discussion of the great terrace-edged cliffs than was given by Powell who, in an elaborate account of retreating escarpments, contrasted them with fault cliffs in the following suggestive terms:

> The cliffs of erosion are very irregular in direction, but somewhat constant in vertical outline; and the cliffs of displacement are somewhat regular in direction, but very inconstant in vertical outline.[2]

A corresponding statement is made by Gilbert only in explaining a specific fault scarp, as noted in the next section; but the context of his report leaves no doubt that the problem here considered was well understood and that the plateau terraces were seen to be the result of a vast denudation. Indeed, a sentence that is buried in the abstract of a paper on the "Recency of certain volcanoes of the western United States," presented to the American Association for the Advancement of Science in 1874, suffices to show that, had not Gilbert chosen for some reason to make his report brief, he could have been as explicit as Powell; for while explaining the long duration of volcanic activity in the West, as indicated by the amount of erosion that had taken place between the earliest and the latest eruptions, he introduced an excellent phrase regarding the Triassic cliffs that face southward across the Carboniferous plateau:

> An erosion of infinite slowness is carrying these cliffs back toward the north and thus increasing the Carboniferous area at the expense of the Triassic.

This subject will be further considered below in the analysis of Gilbert's views on the degradation of highlands to lowlands, and on the occurrence of more than one cycle of erosion in the plateau region. A single but highly significant passage may be quoted here, as it shows

[2] Explorations of the Colorado River of the West, 1875, 191

a marked advance from the doubts expressed as to widespread denudation in the field notes of 1872, cited above; for the huge cliff-edged terraces came later to be well understood as subordinate incidents in the vast erosion that the plateau province has suffered:

Of the immensity of the denudation that has reduced the Plateaus to their present condition, we have unmistakable, and at the same time unexpected evidence, in the existence of insular masses of strata, remote from the mesas [terraces?] of which they once formed part. The most important of these are found . . . [in the Uinkaret mountains not far north of the western part of the Colorado canyon, and again to the south of the canyon, farther east], and consist of limited tables of Triassic rocks, resting on the broad Carboniferous floor [of the lowest terrace], and surviving the general destruction in virtue of protecting mantles of lava (81).

It is interesting to note that in Gilbert's reprints of his reports, an interleaved statement supplants this quoted passage, as follows:

Professor Powell discovered in the Uinkaret Mountains an island of Triassic strata, from which the corresponding cliffs have retreated twenty-five or thirty miles; and has surmised with much plausibility that Red Butte, south of the Grand Cañon and fifty miles west of the nearest point of the Triassic escarpment, is similarly constituted.

This is probably one of the "sentences that were suppressed in the manuscript"; it was thus restored by Gilbert to prevent the impression that he "disregarded through ignorance or discourtesy the work of other geologists." He never did that; yet scrupulously honest as he was in acknowledging his own indebtedness, he exacted no such acknowledgment from his associates with regard to the overflowing abundance of helpful suggestions that he gave them all through his generous life; not even if they, after a longer or shorter interval of semi-conscious assimilation, sometimes gave forth his ideas as their own.

TOPOGRAPHY OF FRACTURES AND FLEXURES

The unlike forms of retreating escarpments due to long-continued denudation and of fault scarps due to recent fracturing are described only in very general terms in the first report, and are but briefly mentioned in the second report in describing a specific feature, the scarp of the lava-capped Natanes Plateau beyond the southernmost part of the plateau province.

There, instead of the scalloped figure, made up of convex curves, that results when erosion controls [the form of lava-capped mesas], we have a straight line, interrupted only by angular embayments, where it is intersected by waterways; and the steepest cliffs, instead of overhanging the points of most rapid present erosion, are along the rectilinear front, which faces a broad, streamless valley. This character maintains for twenty miles, and is unquestionably due to a fault—a fault of not less than 2000 feet throw (528).

This example will be referred to again in connection with the basin ranges, as the Natanes Plateau is assigned to that province. The quotation suffices to show that Gilbert recognized the contrast between young fault scarps and far-retreated escarpments as clearly as Powell did; but the statement, clear as it is, loses much of its value by being associated with a special locality of moderate dimensions, instead of being placed under the account of the northern plateaus.

The great fractures and flexures of the plateau province, which were regarded with good reason in the first report as the unlike but possible contemporaneous effects of similar vertical forces, are briefly described as to their displacements and surface forms (48–57), but unfortunately the descriptions are so phrased as to give the reader little understanding of the immense denudation that the displaced structures have suffered. It is indeed too often implied in the report on the first and second seasons of field work that the topographic features now seen along the lines of these flexed or fractured faults are largely the result of the fracturing or flexuring. For example, a summary regarding the plateaus states that they are "subdivided by longitudinal—north and south—cliffs, produced by faults" (57); and in an account of the long Echo Cliffs flexure of the Triassic sandstones, which comes up from the south and crosses the Colorado at Lees Ferry, it is noted that "on both sides of the river the fold produces conspicuous topographic features" (51); but as a matter of fact the topographic features are the product of an enormous denudation of the flexed strata. Moreover, although a generalized section of the

plateau blocks shows the Grand Wash Cliffs, by which the plateau province is limited on the west, to have retreated moderately from the great fault at their base, the Hurricane and Toroweap scarps that separate the three western blocks are drawn directly on the line of their faults, thus confirming the impression given by the passages just quoted, that the topographic features of these faults result from displacement little modified by erosion, although this is by no means true. It is not until later mention is made of the vast denudation by which the southern plateaus have been stripped of the higher strata which form the clift terraces in the north (81), that the original topographic expression of the fractures and flexures may be inferred to have been greatly modified by the deep and wide-spread erosion which has worn the present surface thousands of feet below the original surface; and even this inference is not well assured because the faulting is not explicitly stated to have preceded the widespread erosion.

However, the topographic features associated with certain faults—for example, the long flexed fault of the Sevier Valley which was seen at intervals for a distance of 225 miles without reaching either end of it—is represented by cross sections in which a considerable amount of post-faulting erosion is indicated, so that the reader is not left altogether uninformed as to a significant measure of change in the surface forms of the faulted structures. Indeed, although it is unguardedly said on one page that "the Kaibab fold throws the [Triassic] belt twenty-five miles to the north" (176), it is briefly and more truthfully explained on an earlier page that the northward shift of certain cliffs on the east side of a fault as compared to the west side is "not due to any horizontal displacement along the line of fault, but merely to the fact that the eastern portions, being lifted higher than the western, became subject to different conditions of denudation" (51); but this explanation is so incomplete, not to say obscure, that it has to be worked out by the reader. On the whole one must conclude, and with some surprise, that the physiographic treatment of fractures—a subject in which Gilbert has usually been regarded as a leader and a master—is not always illuminating.

The physiographic treatment of flexures is in certain respects more satisfactory than that of fractures. The structure of some of the flexures is described clearly and quantitatively; thus along the margins of the Kaibab Plateau, the highest of the blocks in the plateau province, the heavy Carboniferous limestone is said to have been flexed on a curve of from 2 to 3 miles radius; the massive Triassic sandstone, 1,000 feet thick, is said to be bent on the line of the long Sevier Valley fault through an arc of 15° or 20°; and in the Paria flexure farther east, the same heavy sandstone is described as "seamed throughout, as though it had been crushed and reunited, like the bars of ice in Professor Tyndall's celebrated experiments on regelation" (56). There is no correspondingly clear statement in the first report of the degradation that the flexures have suffered, although it may be inferred from certain generalized sections (fig. 26, p. 51; fig. 29, p. 53).

The second report briefly describes a monoclinal flexure in the southern part of the plateau province, trending northwest and producing a throw of 1,500 to 2,000 feet to the southwest in Cretaceous and Triassic strata; and the relation of form to structure is represented in a section from which it appears that the flexure had been reduced to small relief before a basalt flow was poured over it, for a remnant of the flow fortunately survives in a mesa that unconformably covers the eroded edges of the upturned strata on the line of maximum bending. Yet even here emphasis is given in the text to "the antiquity of the eruption," which "is measured by the general degradation of the country of more than 500 feet," with the resulting isolation of the basalt mesa; and little is said of the much greater prebasalt degradation, for although this is well represented in the section, the text merely states that "the fold is older than the basalt" (557). A somewhat fuller account is given of the Nutria flexure, with a throw of from 3,500 to 4,000 feet, along the southwestern side of the Zuni uplift. The great erosion that the flexure has suffered is well represented in several sections, and is clearly set forth in the text in connection with the denudation of the Zuñi dome, as will be shown below. It was a matter of deep regret to all the American members of the Transcontinental Excursion of 1912, when their special train, while making the final eastward turn of its long circuit, stopped in the gap of the vertical Triassic sandstones that follow the axis of this superb flexure not far east of Gallup on

the Santa Fe Railway—as it indeed had been their regret near the beginning of the excursion at Niagara and near its middle in the Great Basin—that, on account of ill health, Gilbert could not be of the party and tell to the European guests something of his early work in the Far West.

VOLCANIC FEATURES

Features of volcanic origin, already known in part from the reports of earlier observers, were found by Gilbert in abundance and variety and given illuminating description, although as his later work led him away from volcanic problems, his name is not usually associated with studies of this kind. The young cinder cones of the Sevier Desert have already been mentioned; they are practically unchanged by erosion. "The weathering of the frail scoria, that caps the crater rim, does not seem to have been begun; the taffylike pellets that, spattered from the bubbling caldron, fell half cooled upon its wall seem as though congealed but yesterday" (136). In the surrounding lava beds, the "most interesting feature is the existence of a number of caves, produced by the escape of lavas from their channels, after the formation of a self-sustaining crust. The caves lie entirely below the general level of the lava field, and we discovered them only where portions of their roofs had fallen." The tubular aperture of one cave was followed "for one or two hundred feet. . . . The width . . . averaged 30 feet and the depth 18, and in length it extended indefinitely beyond the section we explored" (141).

Mount San Francisco, on the plateau south of the Colorado Canyon and perhaps the youngest of the larger volcanoes, was ascended in the summer of 1871 and 65 cinder cones were counted from its summit. Farther southeast, in the Mogollon area, a rich variety of volcanic features was found in Sierra Blanca, composed of massive trachyte of imperfectly conical form, nearly 3,000 feet in local height, with—

a remarkably low angle of slope . . . long slopes of sanidin-dolerite, that appear to have flowed from side fissures . . . spread in successive sheets over the plain . . . to the east . . . for ten or fifteen miles, and to the west for thirty miles. . . . Scattered over these broad sheets are rounded cinder cones, not exceeding a few hundred feet in height, and with some of them are associated *coulees* of balsalt. The depth of the water-worn gorges upon the flanks of Sierra Blanca, attest the antiquity of its chief mass, and in some of these gorges have run streams of basalt. In the valley of White Mountain river . . . are vestiges of three distinct lava flows, which entered at as many different epochs in the progress of the excavation of the valley, and have been successively cut through by the stream (527).

Lava sheets of various dimensions were seen in all stages of preservation and erosion. Some of the most recent examples were found near the southern rim of the plateaus, where "the flowing lavas have in part overrun the cliff and poured into the valleys of the Verde and its tributaries. The principal roads connecting the upper and lower countries avoid the precipice . . . by following the easy grades of these black congealed rivers" (130). Another recent flow lies in the valley of the San Jose, a western branch of the Rio Grande; many travelers on the Santa Fe Railway, which now follows this valley, will agree that "the convolutions of the viscous current, presented as perfectly as though cooled but yesterday, afford there a wonderful and impressive spectacle" (533). Another flow of greater age followed the valley of Zuñi River, a branch of the Little Colorado, for 50 miles, where it crosses the New Mexico-Arizona boundary, and caused "a curious duplication of the valley, which has been deepened by later erosion on both sides of the lava" (533). Not until the third season of exploration was it known that "within the borders of Arizona and New Mexico lies one of the great lava tracts of the world, second in magnitude in our country only to the great northwestern lava field, and fifteen times as large as the classical district of extinct volcanoes in Central France" (525); this mention of a small European prototype of a large American feature being one of the few of its kind in Gilbert's reports.

A number of lava-capped plateaus and mesas are described, among the larger ones being those of Mount Taylor and Acoma (534, 554), the latter measuring 30 by 15 miles. Two examples in far-advanced stages of erosion are illustrated by views and sections. One of these, in the valley of the San Jose, is a "lava cone on a pedestal of sandstone and shale. . . . Whatever sheet of basalt surrounded the cone has been undermined and destroyed up to its very base, where the increasing thickness of the cover has retarded the work." The other, near

the Mount Taylor Plateau, is a steep-sided butte, which was recognized to be "the flue through which an eruption reached the surface. . . . It is a cast in lava, of which the mold was the conduit of a volcano, now not only extinct but demolished. . . . The Cabezon and other similar pinnacles on the opposite [northern] side of the Taylor plateau" were similarly explained (534–536). At a point where the Gila River has eroded a deep valley in lava beds, a view of one valley wall from the cliff top of the other revealed a cinder cone, 600 or 800 feet high, resting on older flows and completely buried under later outpourings (538).

A delicate item of truly Gilbertian quality, an "illustration of the principle of rhythm in nature," remains to be mentioned. It is the occurrence of "wave-like heaps" of basalt fragments on the long and gentle slope beneath many mesa cliffs; each heap represents a large slab of lava that has been detached from the cliff by undermining and has then slowly settled, "without notable horizontal shifting, as the subjacent material is eaten away by percolating waters." When the heaps are of large size, a little swale with moister soil occurs behind each of them; the swales are sometimes cultivated by the Indians. As the size of the slabs, when they break off after undermining has progressed sufficiently, is fairly constant for a given sheet of lava, there arises "a rhythmic uniformity of result, as nearly perfect, perhaps, as that of the analogous waves of the sea" (537).

VARIOUS MINOR TOPICS

The richness of the western field is well known. It incited Gilbert to discuss, briefly, it is true, various topics that lay somewhat aside from his main lines of study. The arid areas afforded abundant illustrations of "the efficiency of dry sand as an erosive agent, when borne by the wind," as had already been noted by earlier explorers, and Gilbert was thus led "to attach considerable importance to this agent of terrestrial denudation. . . . Such wearing cuts no cañons, and leaves no grand monuments of the magnitude of its results, but it is nevertheless a true denudation, applied to broad areas, and, where water is deficient, is no inconsiderable factor in the sculpture of the land" (83). Two instances of such scuplture may be here referred to because they both concern localities where other significant matters curiously enough received no consideration. In one instance attention is called to the conspicuous irregularity of the fanciful and grotesque forms into which certain cross-bedded cliff-making sandstones are wind-carved; but, as noted above, nothing is said either there or elsewhere about the possible origin of the cross-bedding in the sandstone by ancient aggradational wind action. In the other instance an account is given of the action of the wind in removing weak shales from beneath the Triassic sandstones of the Vermilion Cliffs, west of the junction of Paria Creek with the Colorado at Lees Ferry; but although this district is described in some detail in field notebooks, and is mentioned repeatedly in the first report (52, 67, 83, 84), nothing is said of the huge and disorderly landslides that are strewn for miles along the cliff base; but Powell and Dutton were equally inattentive to these tumultuous downfalls, perhaps because they are only subordinate details among the colossal cliffs, platforms, and canyons of this large-featured region.

A more explicit treatment of some phases of wind erosion was given at the summer meeting of the American Association for the Advancement of Science, at Hartford, in 1874, which Gilbert had the opportunity of attending because he did not go west that year and also because he was then attracted into New England for personal reasons. Although his paper was published only as an abstract, with a footnote intimating that its substance would "appear officially and more fully" in the Wheeler report, this intimation is not borne out in the pages of Volume III. Wind-borne sand was described as—

> a denuding agent worthy to be mentioned in the list with frost, and flood, and wave. . . . It undermines cliffs; it scours mountain passes; and it reduces open plains. . . . The degradation of plains by the wind cannot be measured, because it leaves no such monuments as does denudation by water. Water is a leveller in the sense that it transfers material from higher places to lower; but, where it erodes, it . . . leaves ridges and islands, by which its results can be measured. The wind, on the contrary, works most diligently upon salients, and strives to smooth away every vestige of the surface it remodels.

It is singular that no mention was made of the analogy between the channel bed on which denuding streams of water flow and the general surface of the land on which degrading currents of air blow; and also that not even Gilbert's philosophic mind seems then to have recognized that, given time, the "ridges and islands" left in the early stages of water erosion must disappear in the later stages, and leave the degraded surface as free from vestigial mountains as if it had been swept smooth by the broadly diligent wind.

An example of that well-marked group of physiographic forms known as intermont basins was observed between certain members of the basin ranges near the head of the Gila River in southwestern New Mexico. "The building of the Tulerosa and Mimbres ranges separated the basin of the upper Gila from the lower valleys of the stream, and the way which the waters opened for their escape is a profound cañon" through the first-named mountains.

> Before the cutting of the cañon to its present depth that part of the basin which lies nearest the outlet was filled by erupted and transported materials to a great depth and a lake-like plain was produced, the proportions of which can still be grasped by a bird's-eye view. It was about fifteen miles by twenty-five in size, and sloped very gently toward the outlet. By the deepening of the draining cañon, . . . the water-ways have been carried [carved?] below the floor of this plain, and a system of narrow gorges, 500 to 1,000 feet in depth, now traverse and exhibit the filling of the valley. The filling consists of basalt, tuff, and gravel, with nearly horizontal bedding. . . . In some localities basalt has run into the intersecting gorges since the beginning of their excavation (530).

The fact that this basin of deformation was called a "valley" and that its branching gorges were characterized as "intersecting" shows that less progress was made in the terminology of land forms than in their explanation.

Attention to small matters is indicated by the account of numerous low mounds at the eastern base of the Sierra Blanca, near the southern margin of the plateaus, at an altitude of 7,000 feet; the mounds are "usually one or two rods broad and less than a foot high, and separated by interspaces several times as broad as themselves. . . . The grass on the mounds is distinguished . . . by a deeper green. Viewed from a commanding position, the effect is singularly beautiful, the green spots dappling the plain like the figure of a carpet. . . . There is little question that they [the mounds] are vestiges of hummocks thrown up by prairie dogs, or other burrowing animals"; but as prairie dogs are not now found on the high plain, "if the mounds are the work of that species, they may point to a climate, in very recent time, of even greater warmth and aridity than the present" (540).

SUBSEQUENT VALLEYS

How far Gilbert consciously understood the origin of monoclinal or subsequent valleys is not clear, although he makes repeated mention of them. For example, House Rock Valley "lies in the monoclinal" between the horizontal Carboniferous limestones of the upheaved Kaibab Plateau and the horizontal Triassic sandstones of the Vermilion Cliffs next to the east, the intervening monoclinal flexture being occupied by weak beds (53); again, a valley is later described "which, along the southern base of the Zuñi uplift, marks the place of the soft Triassic clays between the Carboniferous limestone of the main mountain and the Triassic sandstone" (533); and once more, a "broad flat valley" is said to be the topographic indication of the same clays near Nutria, on the west side of the Zuñi uplift (553). The southwestern border of the plateau province in Arizona, where the strata of its broad, lowermost "terrace" are gently inclined to the northeast, is drained by several monoclinal valleys excavated along the weak strata between the Carboniferous limestones and the basal Tonto sandstone (60, 80). A striking example was earlier described in the brief account of the Timpahute Range, an east-dipping faulted monocline in the Great Basin, in which "the quartzites at the west, and the limestones at the east, by their superior hardness, maintain parallel ridges, while the intervening shales have been denuded so as to form a valley within the range," opening southward (38); and similarly in the Santa Rita Range "the weathering of the shale has opened a valley between the outcrops of the limestones" which dip gently to the southwest (516).

Although no general explanation is offered for the origin of these monoclinal valleys, it may be inferred from the context that their excavation by the normal action of degradational processes along the strike of weak strata between resistant underlying and overlying strata was so well understood by Gilbert that it was taken as a matter of course; nevertheless, the failure to make explicit statement of such origin and of the manner in which valleys and streams thus formed replace a preexistent drainage is regretable, for the recognition of an important class of valleys was thereby unduly delayed. The omission may be regarded as a result of the unfortunate but perhaps inevitable inattention to the work of foreign geologists; for Jukes had, 10 years before Gilbert's western work, very clearly explained certain monoclinal valleys in southern Ireland as the result of the headward or retrogressive erosion in weak strata, "subsequent" to the erosion of the transverse valleys which they join. On the other hand, as Gilbert did not enter into the discussion of valley origin, he was not led into the error made by both Powell and Dutton of regarding manifestly subsequent valleys as the work of antecedent streams. And yet in the able account that Gilbert wrote of "The Colorado plateau province as a field for geological study," above cited, when he takes up the "Problem of inconsequent drainage," he mentions only antecedent and superimposed streams in addition to consequent streams, and gives no suggestion whatever that a third kind of "inconsequent" stream may exist.

PLANATION BY SUBAERIAL EROSION

The physiographic interpretation of land forms is greatly aided by the recognition of three fundamental generalizations; first, that subaerial erosion will, if continued without interruption for a very long period of time, wear down any land mass, whatever its original structure, form, and height, to a surface of small relief; second, that the forms developed during the progress of uninterrupted erosion will exhibit a somewhat systematic sequence of changes; third, that the continuity of erosional work may be interrupted at any stage of its progress by an upheaval of the land mass concerned, whereupon a new period of deeper erosion will be begun. Although these generalizations had not been formulated at the time of Gilbert's service on the Wheeler survey, they have become well established since then, and all three of them are essential in reaching what is now regarded by those acquainted with the region that he studied as its true interpretation. It is therefore of general as well as of personal interest to inquire how far the above generalizations were either implicitly included or explicitly announced in Gilbert's early reports.

In undertaking this inquiry it is desirable to remember that, while the importance of subaerial erosion in the production of uneven land surfaces had come to be understood in a general way by the geologists of half a century ago, few if any of them realized that the continued action of erosion would in time extinguish the inequalities of form that it had previously produced. They knew that the erosion of valleys would leave intervalley hills, but they did not perceive that after the valley deepening had almost ceased the erosive processes would be chiefly expended upon the hills and directed to their very slow obliteration. Among British geologists about the middle of the nineteenth century, Lyell gave little attention to land sculpture, although his leading principle of uniformitarianism might have led him far beyond the erosion of mere valleys. Greenwood, a valiant advocate of the efficacy of "rain and rivers" (1857) in the erosion of valleys, marshaled his arguments chiefly against the earlier view, which then still found much acceptance, that valleys had been carved by marine currents; he failed completely to see that valley-excavating processes would in time consume the adjoining hills. Geikie (1868) recognized the possibility that plains of denudation might be produced not only by marine abrasion, as was then generally believed, but also by subaerial erosion; but he usually limited subaerial agencies to the excavation of open valleys, and still regarded waves and currents as the most effective agencies of planation.

Among American geologists, Lesley, the leader during his earlier years of work on the Alleghenies of Pennsylvania, in explaining the relation of underground structure and surface form (1856), was a catastrophist who ascribed the carving of his open valleys and narrow water gaps to a vast ocean flood, which, rushing southward from the Arctic, worked "with infinite

force and speed, and ceased forever"; for although he was then one of the first to recognize the important but negelected phenomenon of soil creep, it was not until 10 years later that he became a uniformitarian so far as to explain the excavation of valleys by slow erosion. On the other hand, Powell, earlier by a few years in the western field than Gilbert, but of the same date with him in the delayed publication of his first important report, was satisfactorily explicit regarding the possibilities of subaerial processes; his report on "Exploration of the Colorado River of the West" (1875) explained that "the first work of rain and rivers is to cut channels and divide the country into hills, and perhaps mountains, by many meandering grooves or watercourses, and when these have reached their local base levels, under the existing conditions, the hills are washed down, but not entirely carried away" (204). The clear recognition here given to the natural sequence of valley erosion and hill degradation, as well as to the relation of both to "base level," marks the real opening of the modern epoch of rational physiography.

TWO LAWS OF EROSION

Gilbert's early contributions to the problem of subaerial planation are chiefly as follows: An important approach to the first generalization noted at the beginning of the preceding section was made in his second report by formulating, in simple but sharply pointed phrases, two "laws of erosion" or "general principles everywhere manifested. The first is that soft material is worn more rapidly than hard and the second that high points are worn more rapidly than low—or, more strictly, that steep acclivities suffer more than gentle. The tendency of the first principle is to variety of surface, of the second, to uniformity; and the two are complementary" (554). The value of these two principles, which Gilbert himself characterized as "familiar" rather than as novel, was to him largely physiographic rather than geologic, for they were illustrated much more by examples of eroded forms than of erosional processes. It is especially significant in the present connection that explicit mention was made of the tendency of the second principle to bring about a uniformity of surface; that is, to produce plains of degradation. Moreover, the final clause regarding the complementary relation of the two principles should be interpreted to mean that the ridges or highlands of resistant rock which survive for a time between valleys or lowlands excavated on belts of weak rock in accordance with the first principle will later be themselves worn down to lowlands in accordance with the second principle. But unfortunately no examples of this kind were brought forward, and it may be questioned whether the readers of 50 years ago gathered, without the illumination that explicit examples would have given, the full meaning that is latent in the overterse final clause. If these principles are to-day regarded as elementary, that only shows how much progress has been made in the half century since they were formulated. Yet helpful as their early presentation and illustration was, their value was lessened by the omission of direct statement as to the attitude in which a plain of degradation should stand with respect to the imaginary surface of control which physiographers, following Powell's lead, later came to know as the "baselevel of erosion."

The tendency of erosional processes eventually to produce uniformity of surface had already been shown in Gilbert's first report to be realized over the vast extent of surface occupied by the lowest one of the great "terraces" or "benches" of the plateau province, across which the Colorado Canyon is trenched. This "terrace" measures about 130 by 300 miles, and its production by the removal of overlying strata, thousands of feet in thickness, involved the "immensity of denudation" (81), to which reference has already been made, although the principles involved in its planation were not then stated. In the second report several additional statements are found concerning realized or nearly realized surfaces of planation. All these surfaces are in areas of relatively weak rocks, yet they seem to show that their observer and describer understood that the final result of subaerial erosion acting upon highlands of any structure and of any original form must be a surface of faint relief; not only so, he must also have recognized that this final result was preceded by earlier stages characterized at first by narrow canyons and later by open valleys. Hence he must have been mentally aware of the second generalization of the three above stated, although he did not formulate it. Indeed, he had noted in his first report "the well-recognized fact in the natural history of rivers that their first work

of erosion, where they have rapid fall, is upon their beds, and that it is only when they have so far reduced their grades as greatly to reduce their transporting and cutting power, that they begin wearing their banks and widening their channels, so as to render flood-plains possible" (67); therefore it seems fair to interpret a later statement, that certain worn-down areas "have been denuded *evenly*, instead of being deeply scored along the chief lines of drainage" (554), as meaning that the early stage of deep scoring had been past before the late stage of even degradation was reached. Nevertheless Gilbert's statement of this aspect of the problem is not so clear as Powell's, quoted above.

EXAMPLES OF SUBAERIAL DEGRADATION

Besides the Carboniferous "terrace" or plateau just cited, the following examples of worn-down areas may be instanced, chiefly from the southern part of the plateau province, where a large part of the surface is occupied by strata of moderate or small resistance. Certain Cretaceous areas in western New Mexico are described, above which stand the lava-capped tables of the Mount Taylor and Acoma Plateaus, "and from the entire surface of which it is demonstrated a thousand feet of rock have been razed" in the production of their present surface of small relief; and to this it is added that, when "standing upon the edge of one of these tables and viewing a broad stretch of country . . . one can appreciate the fact that erosion is the great agent in the production of all details of surface, and that the disposition and hardness of rocks are only modifying conditions" (554, 555). An account of the Zuñi Mountains, which have already been cited as occupying an elongated dome of upheaval, and in which it is noted that "the antagonism and the concurrent result of the two laws of erosion are illustrated," is still more significant, although the manifest meaning of its lines constantly arouses the wish that the deeper meaning between them had been more fully written out. "On every side the strata dip away from the axis, and the soft [Mesozoic] formations that have been eroded from the dome now outcrop in a series of concentric elliptical belts" (563), the weakest members of which are shown in accompanying sections to have been worn down to faint relief. "If only the law of altitude were obeyed, (which would have been the case if the Paleozoic and Archaean rocks were no harder than the Mesozoic), there could be no mountain at all, and the uplift would be marked only by concentric annular outcrops of the several strata" (555). It is difficult to imagine that the writer of these lines, in which an explanation of the present form of the mountains was the first consideration, did not perceive that, given more time, the Paleozoic and Archaean rocks, resistant as they may be, would also be worn down low; and yet in the absence of a completed statement to that effect one must remain in some doubt as to how fully Gilbert had then solved the problem of subaerial planation.

A passage in the chapter of the second report on the basin ranges tends to resolve this doubt in Gilbert's favor. The passage concerns the "sterile and remote" Pyramid Range in southwestern New Mexico, composed of eruptive rocks traversed by quartz veins. "The whole range has an appearance of great antiquity, being reduced nearly to the level of the surrounding plain by an erosion, the present progress of which is of exceeding slowness. . . . The purest quartz veins, resisting the destructive agents by which the country rock is degraded, project above the ground surface in long, ragged walls"; but as the planation here accomplished is explained by "the easy disintegration of the ancient lava" (514), it hardly reaches the case of resistant rocks. The planation of such rocks is best exemplified, not in any surface forms of to-day, but in the ancient land surface of small relief on which, in both the plateau and the basin range provinces, the Paleozoic formations rest in strong unconformity. An example of such a surface in the southeastern part of the basin range province shows that, where the Archean and Paleozoic rocks are in contact, the pre-Paleozoic "degradation of the Archaean mountain was carried so far" as to produce "a plane and originally level [but now tilted] surface"; yet here an additional clause concerning the exposure of the ancient land "to the waves of the Paleozoic shore" harks back to the idea of marine abrasion (510). The account of the same great unconformity revealed deep in the Colorado Canyon has already been cited; it may be more confidently interpreted as recognizing the possibility of subaerial planation of resistant rocks, although no explicit statement to that effect was made.

SUCCESSIVE PERIODS OF EROSION

Regarding the third physiographic generalization mentioned above, it should be recalled that, at the time of Gilbert's early work, few if any geologists had discussed the second upheaval and deeper erosion of a region which had previously broadly degraded in consequence of a long-preceding first upheaval. Uplands of even skyline, but dissected by valleys, were then interpreted, if interpreted at all, as uplifted plains of marine denudation in their first period of subaerial erosion. An intermediate period of subaerial planation between two long-separated uplifts was not conceived; residual hills were regarded as unconsumed remnants of a first uplift. Even in Gilbert's descriptions of the plateau and the basin ranges provinces, no explicit statements concerning two uplifts separated by a long period of erosion are to be found; yet certain passages in the second report indicate that such a succession of events had been more or less consciously present in his mind. True, the immense denudation that followed the general uplift of the region and "reduced the plateaus to their present condition" (81) was not at the time of these early surveys understood by Gilbert—or by Powell either for that matter—to have been followed by a later uplift which permitted the much smaller erosional work seen in the Colorado Canyon, huge as the canyon is; Dutton appears to have been the first to have recognized the necessity of a twofold elevation.

Yet Gilbert made the suggestion that the Aubrey Cliff, which "now rises from three to five miles back from the brink of the cañon" in its western part—thus leaving the platform of intermediate height which Dutton later called the esplanade—"may be supposed to have retired to that position by slow waste during the excavation of the cañon" (81); and if that were understood it would seem that the ten or twenty fold greater recession "by slow waste" of the Triassic and other cliffs to the north ought to have been referred to a precanyon period of erosion when the whole region stood lower; but no such conclusion is explicitly announced. Indeed, a statement made regarding the strong lateral retreat of the Triassic cliffs from the narrow cleft known as the Marble Canyon, cut by the Colorado in the resistant Carboniferous limestones east of the Kaibab (68), permits the belief that the above suggested conclusion had not been reached. Certainly a short statement already quoted in connection with notes on historical geology, to the effect that the plateau province has "been elevated, relatively to the adjacent portion of the Great Basin, not less than 4000 feet since the drainage of the great Tertiary lake" (60), and a similar but briefer statement that "the Plateau region . . . has been bodily uplifted" (187), give no intimation of two movements of upheaval, separated by a long period of immense denudation.

A clearer implication of the concept that two periods of erosion were separated by an upheaval is made in an account of a district near Camp Apache in eastern Arizona, on the borderland of the plateau and basin range provinces. The broadly eroded edges of the Carboniferous and Triassic formations, inclining gently northeastward, are unconformably covered by some 500 feet of gravel, and this is overspread by 70 feet of basalt. Post-basaltic erosion has excavated "a valley several miles broad and 1200 feet deep" (135).

The evenness of the basalt sheets that spread over its [the gravel's] original surface, indicate that the formation floored a plain, and suggest a relation of altitudes far different from the present. The region is now so elevated that its erosion is very rapid. Streams have sunk their channels to a depth of two thousand feet below the old plain, and carried the eroded material to the modern plain of the Lower Gila, which lies so little above the ocean level, that its slopes are slightly inclined, and its arroyos shallow" (172).

An uplift after the formation of the gravel-covered and basalt-capped plain and before the erosion of the modern valleys is clearly implied. The quoted passage is further interesting in containing one of the few references to what would now be called the normal baselevel of erosion.

Another passage in which a revival of erosion is implied concerns the Acoma and Mount Taylor Plateaus, already referred to.

The Cretaceous field southwest of the Acoma plateau has been reduced nearly or perhaps quite a thousand feet since the eruption of the Acoma lava, but it has been reduced so evenly, that its surface is now as near level as that upon which the Acoma lava was spread. And the same may be said of the field north of the Mt. Taylor plateau, which has been degraded even a greater amount (554).

The phrasing here employed would suggest that after a district had been worn down to a nearly level surface, its even degradation was continued to lower and lower levels.

THE GILA CONGLOMERATE

The idea of an uplift of the continental interior after the time of basin-range faulting is met in connection with the dissection of the intermont basin by the Gila headwaters, described in a preceding section. A phrase omitted from the quotation there made is here supplied:

By the deepening of the draining cañon, an excavation probably connected with a broad continental oscillation to which further allusion will be made in the sequel, the water ways have been carried below the floor of the plain (530).

But unhappily a close scrutiny of the sequel finds no further reference to this important subject than is given in an account of other parts of the Gila River and of the so-called Gila conglomerate, as follows: The upper valleys of the Gila and its branches, declining southwestward from the clift southern rim of the plateaus, are occupied by an extensive deposit of conglomerate, that slopes down to the gravel plains between the basin ranges, but unlike these low-level deposits, which are as a rule still accumulating, the conglomerate is now trenched by valleys to a depth of 1,000 or 1,500 feet.

It is in its relation to the rivers that it [the conglomerate] is chiefly interesting; in the accumulation, and subsequent excavation of the beds, there is recorded a reversal of conditions, that may have a broad meaning. . . . There is no difficulty in comprehending the present action [of the rivers], for it is the usual habit of swift-flowing streams to cut their channels deeper; but to account for the period of accumulation there must be assumed some condition that has ceased to exist. Such a condition might be, either a barrier, somewhere below the region in question, determining the discharge of the water at a higher level than at present, or it might be a general depression of the region, in virtue of which the ocean (now three hundred miles away) became a virtual barrier. With either hypothesis, a change of more than 1,000 feet must be considered (540, 541).

Here we find not only the intimation of a possible subrecent upheaval of the region, following a time of lower stand or "depression," but again a reference to the ocean as a "virtual barrier" to the erosion of valleys. But the ocean seems to have been in general so remote a contingency that it was seldom mentioned. A possible explanation of the valleys in the conglomerate as the result of a change of relation between load and stream power, due either to variation of climate or to deformation in the headwater region, is not considered.

SUMMARY

Although Gilbert adopted a systematic instead of a narrative order in the presentation of his results, he was writing only a report not a textbook, and therefore made no attempt to complete the scheme in which all his various items of observation might have been contained. Each item was reasonably explained, and in certain cases the explanation included some account of earlier and of later stages which preceded and followed the observed stage of physiographic evolution, thus introducing the idea of a systematic sequence in the sculpturing of land forms; but the fundamental principle here involved was not emphasized, perhaps because it was not perceived in its entirety. A few steps at a time seems to be the order of progress. The large steps which Gilbert took were in the direction of recognizing that every element of the surface of the lands is the product of some reasonable processes, and of holding the physiographer responsible for the elucidation of those processes. Gilbert's steps, like those taken by Powell, were manifestly the forerunners of others which later generalized the relations of the various processes as applied to various structures in a more comprehensive evolutionary scheme; and it is for this reason that all later progress in physiography is so deeply indebted to the work of these pioneers in western exploration.

CHAPTER VI

THE BASIN RANGES IN THE WHEELER REPORTS

THE PROBLEM OF THE BASIN RANGES

The review of various features and problems of the plateau province, concerning which Gilbert's results have been practically undisputed, being now concluded, his discussion of the basin ranges, which led to a long controversy, may be taken up. Two phases of this discussion deserve attention. One, of general scientific interest, is the share that Gilbert contributed to the conclusions later reached regarding the origin of the ranges; the other, of more personal interest, concerns the first statement of his contribution, as found in the Wheeler report. As to the final conclusions, certain general statements are made in an essay, upon which Gilbert was engaged at the time of his death; and as these should be borne in mind while their first outline and later evolution are considered in this and following sections, a brief summary of them may be here presented, as follows: The region of the basin ranges was deformed by compression about the close of Jurassic time; the mountains then formed were greatly eroded and for the most part degraded to small relief in Cretaceous and Tertiary time; the worn-down region of deformed rocks was then broken into great blocks by numerous, subparallel, generally north-south faults; and the fault blocks of deformed and degraded rocks were upheaved and diversely tilted by uplifting forces without significant lateral compression and probably with lateral extension. The higher parts of the upheaved blocks, now more or less eroded, constitute the basin ranges of to-day; the relatively depressed intervening areas constitute the intermont troughs or "valleys," which are more or less heavily aggraded with detritus from the ranges. Complications due to the extrusion of volcanic rocks are not here considered, but will be briefly referred to in a later section. In a few words, the mountains of post-Jurassic compression have been essentially obliterated, and the existing ranges are relatively modern fault-block fragments of the earlier, worn-down mountain region, upheaved by vertical or extensional forces and as a rule imperfectly degraded.

Gilbert's original theory was much more simple: The region was broken into great north-south blocks on vertical fissures about the close of Jurassic time; the blocks were uneven, upheaved and warped into monoclinal or moderately bent structures; and the higher blocks were gradually carved into the existing ranges, while the lower blocks were buried under the detritus eroded from the higher ones. But for certain members of the basin ranges in Utah and Nevada another simple theory had already been proposed by the geologists of the Fortieth Parallel survey; namely, that the existing mountains are the unconsumed remnants of much greater mountains that were produced by post-Jurassic compressional folding, the intermont depressions being regarded as chiefly the work of later erosion. Thus explained, the basin ranges would be classed as monogenetic, because only a single period of deformation was concerned in their production; but under Gilbert's original explanation also, as well as under a later combination of both theories, the existing mountains are monogenetic, because only a single though possibly a prolonged period of deformation was concerned in their production as visible topographic features. It is not the existing mountains, but the deformed rock structures within the mountains that are polygenetic in Dana's sense of that term. Gilbert's theory as first proposed and as later modified will now be analyzed.

PHYSIOGRAPHIC PRINCIPLES

It has already been pointed out that many of the more novel truths learned by Gilbert in the plateau province were physiographic in being concerned largely with the production and description of existing surface forms, rather than geologic in being concerned largely with the discussion of underground structures and the conditions of their production. The same may be said and with even greater emphasis regarding his work on the basin ranges; for the chief

truth of his basin-range theory resulted from the inclusion in it of a new and essentially physiographic principle of his own discovery. Those who had advanced the opposing theory were uninformed as to this principle and could therefore make small use of it; and disagreement necessarily followed. The leading characteristic of Gilbert's principle in its application to the basin-range problem was that it led him to take fuller and more reasonable account of surface features than was the habit of the time. He was not satisfied merely to ascribe the existing forms of the ranges to unspecified erosion, as was then the fashion; he sought to discover the conditions under which the observed forms could be systematically accounted for by the progressive action of erosion upon definite structural masses; and as these sought-for conditions included not only the changing attitude of the structural masses, but also the passage of time, they naturally constituted, when discovered, essential contributions to the history of the mountains concerned.

It is chiefly by reason of his new principle that Gilbert's work on the basin ranges occupies an important place in the history of earth science, and especially of its physiographic chapter. There was an earlier time when geographers took little or no account of the origin of mountains and other features that they described, and when it was the habit of geologists also to give little heed to surface forms and to occupy themselves chiefly with underground structures. Next followed a middle time when, geographers still remaining for the most part indifferent to the rational aspects of land sculpture, geologists at least recognized that surface forms are the result of erosion, although they seldom specified its amount or traced its action. This was truly an advance from a stage of greater indifference, but the advance did not suffice to provide thoroughgoing explanations for land forms of different kinds; and as to the progressive evolution of land forms through a systematic sequence of changes by the work of erosional processes upon one structural mass or another, no such philosophical scheme was broached by the geologists of that physiographically medieval era; or as Gilbert would have said in characteristic western phrase: "You can't prove it by them." Then came a later time when physiographers—that is, geologists with a geographical leaning, or geographers with a geological training—undertook, while accepting crustal structures without particular inquiry into the manner of their origin or deformation, to give special attention to the origin and description of all surface forms as resulting from various amounts of erosion by various kinds of erosional processes upon various sorts of structures; and eventually the erosional processes and the eroded forms were not only systematized as to their kinds and their limiting baselevels, but also as to their variations with the passage of time. Thus the evolutionary physiography of land forms has come to replace the old-fashioned empirical physical geography of the lands. Gilbert's contributions to this advance were invaluable; they "started a ferment in men's minds"; but they were not all made during his membership on the Wheeler survey.

THREEFOLD TREATMENT OF SURFACE FORMS

It is true that certain essential steps of the advance were not clearly apprehended, much less explicitly formulated, by Gilbert in his first studies in the West, even though their partial recognition furnished him with a new means of interpreting mountain history. Nevertheless, the evolution of rational physiography, as above outlined, was greatly promoted by the beginning that he then made; for as already intimated, his results imply that he more or less consciously possessed the knowledge and made the application of an important physiographic principle which, even to-day, is by no means so widely understood or so generally employed as it should be; namely, that every structural mass of the earth's crust must have a surface form which is subject to change by deformation and erosion; and consequently, that a physiographer is responsible for the explanatory description of the existing surface form of every crustal mass in terms of its latest deformation and its present stage of erosion, just as a geologist is responsible for the description of its underground structure in terms of its original accumulation and its later deformation; the geologist and the physiographer each being free, of course, to make whatever use of the facts and inferences in the other's special province that may be helpful in reaching his own object.

In view of this principle, a physiographic description does not go far enough in merely saying that an observed land form results from the action of erosion, undefined in amount and stage, upon a structural mass, unspecified as to its surface form and attitude at the time of its last deformation or upheaval. It is necessary, if the description is to be clearly intelligible, to explain the observed or actual form by the systematic action of sculpturing agencies through a certain period of time upon a structural mass, the surface of which had a certain form in a a certain attitude at the beginning of the erosional period under consideration. In dealing with the upheaval or fracture of a crustal mass, a geologist might be satisfied to state the measure, date, and cause of the movement; but if such upheaval or fracture is to enter properly into a physiographic description, it must constitute the second member in a threefold sequence of treatment: The first member must explain the surface form that the crustal mass had acquired before it was deformed; the second member must then define the effect that the deformation had upon the predeformational surface form as well as upon the crustal mass; and the third member must state how far the predeformational forms have been destroyed and new forms have been introduced by later erosion. This explanation may seem so plain to those who are accustomed to employ the threefold sequence of treatment that it involves, as to be hardly worth announcing here; but unhappily this scheme of treatment is still unknown to or neglected by certain geologists—to say nothing of physiographers—who undertake to write physiographic descriptions; and as a consequence their descriptions are imperfectly intelligible.

AGE AND STRUCTURE OF THE RANGES: FIRST WHEELER REPORT

If Gilbert's first report is now examined with the principles and methods outlined in the preceding paragraphs in mind, it will prove interesting to search out the successive announcements of his theoretical views on the origin of his mountains, even though the incompleteness of their statement may occasion surprise. It may be here explained, by way of introduction, that very little is to be found concerning the theoretical aspects of the basin-range problem in any of the three seasons' notebooks. Where the problem was touched upon at all in the field records, the treatment is either so brief or so vague as to give little indication of the many questions that it involves. For example, it was noted on September 15, 1872, that the structure of a range near Beaver River in southwestern Utah led "to the opinion that the whole is in general a monoclinal uplift with Dip to the W. & composed of the quartzite & limestone of the Silurian." An earlier and somewhat more explicit statement is made regarding the Fish Spring and House Ranges, nearer the Utah-Nevada boundary, where they are both treated under the latter name. The northern and smaller member, or Fish Spring Range, was recognized to have a westward dip with an east-facing escarpment, while the much longer southern member, or House Range, was described as having an eastward dip with a west-facing escarpment; the two monoclinal masses being on opposite sides of the nearly direct line on which the two escarpments fall. These facts led to an important conclusion noted on August 25, 1872:

> The peculiar characters of the House Range suggest that it marks a N–S crack the western lip of which [Fish Spring Range] is uplifted North of Dry Pass [Sand Pass on the topographic map the Fish Springs quadrangle] while the eastern lip [the House Range proper] is uplifted S of the pass. We could see yesterday that the two uplifts are not quite in line, but have an offset at Dry Pass.

Furthermore, two months later Gilbert's party was in the plateau province and had followed the canyon of Kanab Creek southward to its junction with the canyon of the Colorado, where he found by the way, two of Major Powell's boats "& a great variety of things scattered about on the bank," and where the structure and erosion of the plateau afforded matter for many pages of notes; and there an unexpected reference is made to the basin-range problem, showing that it was still borne in mind; on October 29, a diagram, here reproduced, is labelled: "An attempt to sketch the Fish Spring-House Range to illustrate its structure"; but that is all. The foreground member of the group presumably represents the Confusion Range on the South. Unfortunately the excellent device of adding a vertical section to the foreground, adopted a few years later by Holmes in his illustrations for Powell's

Colorado River report, and by Gilbert himself in his block diagrams of Powell's Uinta Mountains and of his own Henry Mountains, was not here employed.

Gilbert's published reports may be next examined. After introducing the term, basin ranges, for "all that system of short ridges separated by trough-like valleys which lie west of the Plateau system, without reference to its drainage conditions," and thus indicating that this "orographic province, which has its type in the Great Basin, . . . is not coincident with it" (22), he turns, before describing individual ranges, to a general consideration of their age or epoch of upheaval. This must lie for each range between "the age of the newest rocks uplifted with it, and the age of the oldest rocks which rest unconformably upon them" (24). Following this rule, the age of many ranges can be only vaguely defined. For example, those of southwestern Arizona consist of highly crystalline schists unconformably overlaid by Quaternary gravels; hence for these "only the most indefinite idea of the epoch of upheaval" is reached. Certain ranges standing near the plateaus and including deformed Paleozoic strata that rest unconformably upon Archaean rocks, are said "to have been first upheaved at some time anterior to the Carboniferous, and again at some time subsequent to the Carboniferous"; but this statement was misleading, for it is not to the ranges but to their rocks that it properly applies. However, in other ranges which include a fuller stratigraphic series, it was presumed that "their principal elevation was coeval with the first and chief elevation of the Wasatch Mountains and the Sierra Nevada, proved by Whitney and King to have occurred at the close of the Jurassic period" (24). It thus appears that no basin ranges are treated here as having a later epoch of upheaval than mid-Mesozoic; for while post-Eocene disturbances were detected in the Wasatch Range and the plateaus farther east, nothing definite could be predicated "from the stratigraphical data" in hand concerning the extension of these relatively modern disturbances into the basin-range province.

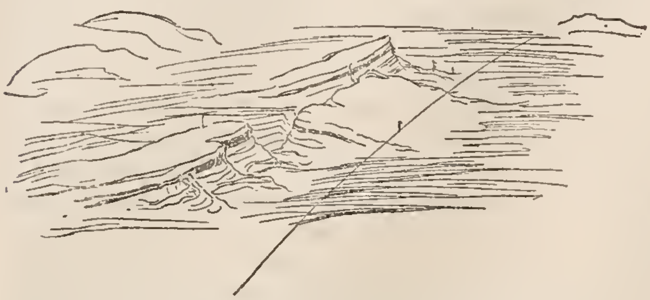


FIG. 5.—Ideal diagram of Confusion, House, and Fish-spring Ranges; from Gilbert's notebook, October 29, 1872

The structure of nearly a score of ranges—frequently called ridges—is then concisely described in less than as many pages, after which a one-page summary states that "the great majority of the ranges . . . exhibit in cross section but a single direction of dip," with or without faulting; these are called "simple and compound monoclinals" (40). But "anticlinals and synclinals also occur as subsidiary features within some ranges." Monoclinal escarpments or outcrop faces are mentioned, but without emphasizing their greater steepness than that of the opposite or dip slope of the mountain side. "A few most remarkable ranges present escarpments on both faces" (41). It is to be regretted that more details were not given.

BEARING OF RANGE FORM ON RANGE ORIGIN

The effort is then made to discover the origin of the ranges, and it is here that the physiographic relation of structure, erosion, and form appears to have been utilized with most novel results. The form of the ranges is first used to show that they can not have been produced by the erosion of broadly corrugated structures. As Gilbert was already familiar with the features of the Pennsylvania Alleghenies, he wrote:

> I entered the field with the expectation of finding in the ridges of Nevada a like structure, and it was only with the accumulation of difficulties that I reluctantly abandoned the idea (41).

The morphological principles latent in that highly significant confession deserved fuller exposition, for it suggests that Gilbert had more or less clearly in mind the method of threefold analysis above mentioned as essential to a proper explanation of land forms. In more expanded phrasing, his meaning must have been: The nearly level depositional surface of the Paleozoic strata in Pennsylvania before their folding was deformed into a series of great folds or corrugations by compression; but the deformation took place so long ago that contemporary and sub-

sequent erosion has completely destroyed the corrugated surface, and nothing now remains of the upheaved folds but the long, narrow ridges which precisely reveal the attitude of the most resistant strata, while the valleys and lowlands are worn down on the weaker intervening strata. It is only after understanding this threefold sequence of considerations that the full significance of Gilbert's next sentence can be measured; for he goes on to say: "It is impossible, by any hypothetical denudation, to formulate the Basin Ranges as remnants of a system of anticlinal and synclinal folds"; that is, a system of folds not traversed by dominant north-south faults. "The simple monoclinal may indeed be explained as the side of an anticlinal, by the harsh assumption that the remaining parts have been removed below the level of the adjacent valley [intermont trough], but the explanation will not apply to the compound monoclinal" (41).

Unfortunately, these brief assertions were not supplemented by analytical exposition, and no justification was offered for calling the explanation of the simple monoclinal ranges as remnants of broad anticlinals—an explanation that had been tacitly accepted as satisfactory by some of Gilbert's contemporaries—a "harsh assumption." Other similarly brief assertions immediately follow. The erosion is not "conceivable that would carve Worthington mountain . . . from an anticlinal" (41), for this is a 15-mile range consisting of a narrow segment of nearly horizontal strata which outcrop in the steep slopes that limit it on both sides; and concerning the same remarkable range, it had already been noted, when its singular structure and form were described, that no erosion could be conceived of "that should have left this thin segment as the remnant of an inclined table or of a fold" (37). If this idea had been elaborated so far as to describe the form of a mountain that could have been produced by the erosion of an extensive table or fold, and if such a form had then been contrasted with that of the observed range, the argument might have been better understood.

Other terse statements are found a page later: "Ridge [range] lines are more persistent than structures. In the same continuous ridge [range] are monoclinals with opposed dip"; that is, warped monoclinals; and other ranges are both "monoclinal and anticlinal" (42). The same comment may be made here as above; a whole volume of meaning is packed into these too short sentences, which even the illustrative examples next adduced did not suffice to make fully intelligible to geologists whose physiographic eyes had not been opened. In illustration of the important generalization that the ranges do not trend with the structure of their rocks, the section of Spring Mountain Range at Ivanpah in southern Nevada is selected:

> The mountain there shows an axis of granite, flanked on each side by limestone, but the trend of the anticlinal is oblique to that of the range and it quickly runs out, the granite giving place at the north to the eastern mass of limestone, which rises and, as an eastward dipping monoclinal, there constitutes the entire range, while the western limestone mass becomes, in the same manner, supreme at the south (42).

The meaning here is evidently that, if this range were the eroded residual of an unfaulted anticlinal fold, the granitic axis and the limestones on its two flanks ought to continue indefinitely along the strike of the fold; but that as "there is not on a grand scale that close dependence of form on durability that must maintain were the great features of the country carved by denuding agents" (41), some other explanation for the ranges than nonfaulted folding and sequential erosion must be sought for.

THE BASIN RANGES AS UPHEAVED AND WARPED FAULT BLOCKS

The idea that the basin ranges are the eroded remnants of great anticlinal and synclinal folds was therefore dismissed; and after a concise generalization of the leading features of the ranges, Gilbert's own views are announced.

> To begin with the simplest generalization, the ranges are a system; not indeed formed at the same time, but exhibiting certain common characters, over a great area. They are parallel; they recur with some regularity of interval; they are of moderate dimensions.

Then comes the new theory:

> The ridges [ranges] of the system occupy *loci* of upheaval and are not mere residua of denudation; the valleys of the system [the intermont troughs] are not valleys of erosion, but mere intervals between lines of maximum uplift (41). The movements of the strata by which the ridges [ranges] have been produced have been in

chief part vertical along planes of fracture, and have not involved great horizontal compression. . . . The forces which have been concerned in the upheaval of the Basin ranges have manifested themselves at the surface as simple agents of uplift, acting in vertical, or nearly vertical planes (42).

In little more than these phases was a new theory of mountain making first announced.

Additional statements are given under "General considerations," on a later page, after a description of the moderately dislocated, north-south fault blocks in the plateau province. It is there again announced that the forces which produced the basin ranges acted "by nearly vertical upheaval; and that they were deep-seated," and it is then added that, as forces of the same kind displaced the plateau blocks, "a single short step brings us to the important conclusion that the forces were identical (except in time and distribution); that the whole phenomena belong to one great system of mountain formation, of which the ranges exemplify advanced, and the plateau faults the initial, stages." It is therefore "impossible to overestimate the advantages of this field for the study of what may be called the embryology of mountain building. In it can be found differentiated the simplest initiatory phenomena, not obscured, but rather exposed by denudation"—that is, in the little-disturbed plateau blocks, the structure and displacements of which are so well brought to light by the transecting canyons—"and the process can be followed from step to step, until the complicated results of successive deformations and erosions"—that is, in the basin ranges—"baffle analysis" (60, 61). The analogy of the northern plateau blocks to the ranges is especially indicated a few pages later:

A portion of the valleys of the Plateau country, and especially those of the upper Sevier, are, like the troughs of the Range region, structural, and lie between the monoclinal ridges produced by the system of faults (67).

Returning now to the earlier page, there soon follows an illuminating comparison which throws a much-needed light on the mechanism of Gilbert's theory:

In the Appalachians corrugation has been produced commonly by folding, exceptionally by faulting; in the Basin Ranges, commonly by faulting, exceptionally by flexure. The regular alternation of curved synclinals and anticlinals [in the former] is contrasted with rigid bodies of inclined strata, bounded by parallel faults [in the latter]. The former demand the assumption of great horizontal diminution of the space covered by the disturbed strata, and suggest lateral pressure as the immediate force concerned; the latter involve little horizontal diminution, and suggest the application of vertical pressure from below (61).

The author recognizes that much is yet to be learned about the basin ranges and that it would therefore be premature "to attempt a reconciliation of these antithetical phenomena," but he—

cannot forbear a brief suggestion before leaving the subject. It is, that in the case of the Appalachians the primary phenomena are superficial; and in that of the Basin Ranges they are deep-seated, the superficial being secondary; that such a force as has crowded together the strata of the Appalachians . . . has acted in the Ranges on some of the earth's crust beneath the immediate surface; and the upper strata, by continually adapting themselves, under gravity, to the inequalities of the lower, have assumed the forms we see. Such a hypothesis . . supposes that a ridge, created below, and slowly upheaving the superposed strata, would find them at one point coherent and flexible, and there produce an anticlinal; at another hard and rigid, and there uplift a fractured monoclinal (62).

This explanation was regarded as according particularly well "with the persistence of ridges where structures are changed" (62); it plainly credits the vertical forces with deforming as well as upheaving the fault-block strata.

Not only was a mechanical cause thus outlined for the upheavals, but indication was also given of their effect in deforming a preexistent surface and producing a new surface, the defining of which is the object of the second member in the threefold sequence of physiographic treatment. This was done very explicitly in a chapter contributed by Gilbert to Wheeler's progress report for 1872, in which a generalized cross section of the region "discounting denudation," represents a number of diversely deformed fault blocks, bounded by nearly vertical fault planes and unevenly upheaved.[1] A more philosophical presentation of the same topic is given in the main report in the following sentence:

[1] Engineer Department, United States Army. Progress report upon geographical and geological explorations west of the one hundredth meridian, in 1872. . . . Washington, 1874, p. 50.

What may have been the original altitude of the ranges we have no means of knowing, but there is evidence, along the margin of the system, that their elevation was not all accomplished at once, and it is not impossible that progressive elevation and denudation, as they have opposed, have also measurably counterbalanced each other (63).

No specific statement of the measure of upheaval is given, apparently for the good reason later noted that "all estimates of the magnitude of mountain movements are so involved with considerations of erosion, that it would avail little, even were the material at hand, to attempt a full presentation of individual examples" (126). Reference to the evidence for progressive upheaval above noted as provided by certain marginal ranges will be alluded to in a later section.

UPHEAVALS AND ERUPTIONS.

Reference has already been made in the account of the Zuñi dome to the somewhat reactionary nature of Gilbert's views regarding the origin of mountains by upheaval through the operation of deep-seated, vertical forces, instead of by compression through that more of superficial, horizontal forces. These views evidently apply even more fully in the case of the basin ranges, as the foregoing paragraphs have shown. Some additional passages may now be introduced to show that the deep-seated forces, of which the upheaved domes and fault blocks were taken to be the surface effects, were believed to have been closely associated, in the western part of the basin range province at least, with the deep-seated forces that produced the extensive volcanic eruptions of that region. After several pages descriptive of the lava overflow in Nevada, which "over large areas have buried all other rock masses," the following generalizations are presented:

A most important feature of these eruptions is their association with the ridges [ranges] of corrugation. The great majority of vents are along lines of upheaval. . . . The law that the distribution of lavas is in sympathy with the ridge [range] structure, rests on too broad a basis of facts to be vitiated by its exceptions. More than this, there seems reason to believe that uplift and extrusion are, in a certain degree, mutually complementary, or equivalent. The highest ranges, as a rule, are (comparatively) non-volcanic, and those ranges, or portions of ranges, which exhibit the greatest eruptions are endowed with but low nuclei of other material. Where, in tracing a range, we find its crest exchanging non-volcanic rocks for volcanic, we do not find the latter heaped upon an undiminished ridge of the former, but rather replacing it, as it gradually or suddenly diminishes in height; and the case is strengthened by the consideration that, while the low, buried portion of the nucleus has been guarded by its mantle against the forces of denudation, the higher part has been exposed to a continuous waste. . . . It does not necessarily follow from the coincidence, in place, of uplift and eruption, that the subterranean *loci* of the action which has produced corrugation are identical with the volcanic sources. If, however, it be shown that along lines of disturbance there is an inverse quantitative relation between uplift and outflow, a strong argument is adduced, not merely for the identity in location, but for the absolute identity of the upheaving and volcanic forces; for, if the two modes of mountain building are complementary actions, they must be regarded as co-ordinate manifestations of the same agency.

These passages close with an illustration of the admirable candor which later became more and more a characteristic of Gilbert's style of presentation:

It is by no means easy to demonstrate this interrelation of upheaval and eruption. My own confidence that it exists is derived from the comprehensive review of my notes, referring to about fifty of the Basin Ranges, and is a result of inspection rather than analysis. I know not how to present the material to the reader—without special pleading—so that it shall have the same force (125–126).

EROSION OF THE UPHEAVED RANGES

An incautious phrase, quoted with its context near the end of the second preceding section to the effect that "the upper strata, by continually adapting themselves, under gravity, to the inequalities of the lower, have assumed the forms we see," might be mistaken to imply that the upheaved basin-range blocks still retained their surfaces and scarps of upheaval little changed, were it not that the work done by post-faulting erosion is elsewhere especially considered. That such erosion was understood to be of prime importance in fashioning the existing ranges is plainly shown by a later quotation in the same section. This essential factor had, furthermore, been already briefly touched upon in connection with an earlier statement

concerning "*loci* of upheaval"; for while the intermont valleys were taken to represent aggraded belts of relative depression, and hence the work of deposition rather than of erosion, it was clearly announced that "within the ranges there are indeed eroded valleys, and the details of relief show the inequality of erosion due to unequal resistance of the rocks" (41). Moreover, it is later added that "since the close of the Jurassic period" the upheaved masses "have been subjected to the unceasing play of atmospheric eroding agents, wearing away their summits, furrowing their flanks, and conveying their substance to the intervening valleys" (63). The aggradational forms there produced have already been noted.

Evidence of the great volume of post-faulting erosion is further given by references to the vast amount of the detritus accumulated in the arid intermont troughs. "The principal deserts of the region are relatively depressed regions, marked by excessive accumulations of detritus, which have so filled the valleys [the troughs marking the less uplifted fault blocks] as to connect them in a continuous plain, beneath which the minor ranges are completely buried, and through which the peaks of the more lofty jut as islands" (65). Again, in describing the lower-lying portion of the range region that is occupied by the desert of Great Salt Lake, it is noted that "the lowest part of the included depression has been filled with a sea of detritus, until some of its ranges are completely submerged and others protrude only insular buttes to mark where they are sunk. If these hidden mountains rise as high above their bases as do their neighbors on the rim of the basin, . . . the desert sediments . . . may have a maximum thickness of 5,000 or 6,000 feet" (65, 66). Yet great as the volume of eroded material thus seems to be, it was held that "erosion, which began in the Ranges and the Plateaus, as they were successively exposed to the atmosphere . . . has accomplished only a small fraction of its task" (187).

BASIN RANGES IN THE SECOND WHEELER REPORT

All the foregoing discussion of Gilbert's basin-range theory is based upon the report of his first two seasons of field work, 1871, 1872; for although the report is dated July, 1874, its main conclusions had been earlier formulated, as appears from the pages which he contributed to Wheeler's report of progress for 1872, already cited. Gilbert's second report is concisely phrased and does not generalize for the region as a whole. So far as the basin ranges are concerned, it considers only a few far southeastern examples, most of which have northwest-southeast trends. "The usual structure is monoclinal, demonstrably due to faulting in the Chiricahui and Pinal ranges, and presumably so in all the others" (517); and the faulting is of later date than the comparatively modern lavas of which certain monoclines are largely composed. Of the two ranges just mentioned, the first lies in the southeastern corner of Arizona; the second is farther northwest and is traversed at its middle by Salt River, in a notch, the present site of the famous Roosevelt Dam. Another example is the Gila Range, north of the first above named and east of the second; quartzite was seen at its southwestern base, but its greater mass is made of—

> trachytes and trachyte conglomerates, unevenly bedded and surmounted by basalt. These are all exhibited in section in the southwestern face, which is steep, while the opposite face is constituted by the upper lavas, which, dipping in that direction, are continuous to the Bonito river, five miles away. The range is at this point a monoclinal mass of bedded lavas, whose eruption took place before the dislocation which produced the ridge, and the same structure probably continues to the northwest for fifteen miles (514).

The Mimbres Range, in southwestern New Mexico, more specifically described than most of the others, also affords significant structural elements; it consists largely of heavy lava beds, the uppermost of which is seen in a continuous sheet, wonderfully uniform in texture and habit, for 60 miles along the western slope, while Paleozoic rocks outcrop in the eastern slope. The whole mass was regarded as a monoclinal uplift with a fault along the eastern base, where the Paleozoic rocks are disclosed (519). If the Paleozoic rocks in this and in the Gila Range are more steeply deformed than the moderately inclined lava sheets, as is the case elsewhere in that district, and if the contact between the Paleozoic and the lavas is unconformable, as is elsewhere said to be the case, it might have been inferred that the first, presumably late Juras-

sic, deformation of the region affected only the ancient sedimentary strata; that a long period of erosion then followed by which the deformed sedimentary strata were reduced to the moderate relief that they show beneath the lavas which eventually sealed them over; and that the monoclinal tilting, by which the basin-range structure was produced, was so recent as to be later than the lava outpourings.

Had the geological history of these two ranges been thus worked out, it would probably have been found similar to that deciphered by Louderback 30 years later for the west Humboldt Range in northern Nevada; but unfortunately, the inferences above made, simple as they now seem, were not made in 1873, perhaps for the reason that Gilbert saw the Gila Range imperfectly, and that he saw only the western lava-sheeted slope of the Mimbres Range; the occurrence of Paleozoic rocks along the eastern base of this range was reported to him by other members of the survey. On a later page, the Natanes Plateau, a gently inclined mass capped with sanidin-dolerite and lying in the upper basin of the Gila, is provisionally "regarded as a member of the Basin Range system"; it has a southwest-facing escarpment about 1,500 feet in height and 25 miles in length, "unquestionably due to a fault . . . of not less than 2,000 feet throw, . . . which has occurred since the eruption of the sanidin-dolerite." This is the example already cited, in the scarp of which, "instead of the scalloped figure, made up of convex curves, that results when erosion controls, we have a straight line, interrupted only by angular embayments, where it is intersected by water-ways"; and it is the only example in which these characteristics of a slightly dissected fault scarp, in contrast to a retreating escarpment of erosion, are specifically stated (528). It thus appears that the results gained during the third field season, as presented in the second report, do not materially modify the conclusions announced in the first report; although, had it been possible to take full advantage of the structure that appears to characterize the Mimbres Range, important modifications of those conclusions might have been presented.

PHYSIOGRAPHY AND GEOLOGY IN THE BASIN-RANGE THEORY

The physiographic competency of Gilbert's basin-range theory, as stated chiefly in his first report, may be tested, as it has been in part already, by confronting its statement concerning the origin of the visible mountain forms with the requirements of the threefold sequence of physiographic treatment. It thus appears, first, that the preupheaval form for most of the region was, by implication, the smooth uppermost surface of its heavy sedimentary series; second, that the new forms introduced by deforming upheavals, denudation being "discounted" for the moment, would have been a system of huge north-south fault blocks, with inclined or warped upper surfaces, some standing higher, some lower; all the higher ones having nearly vertical fault-scarp sides; or, more truthfully, that the upheavals being regarded as slow and prolonged, so that much erosion went on during their progress, the warped upper surface and the scarped sides of the blocks would be continuously dissected as they were slowly raised; and third, that as to post-upheaval erosion so much has been accomplished, as just intimated, that the "details of relief show the inequality of erosion due to unequal resistance," although a great erosional task still remains to be completed. The leading feature of the theory, namely, the limitation of the ranges by faults on one side at least, was thus reasonably connected with the leading fact of their present form, namely, the discordance of their marginal lines with their structures. It is true that a demonstrated identification of the theoretical fault lines with the visible marginal lines was not systematically presented in the published discussion, but there can be no question that it was understood.

On the other hand, the above extracts disclose a striking unlikeness between the geological elements of the basin-range theory in its original form and the form that it later assumed, largely through supplementary suggestions proposed by others and accepted by Gilbert. In the original theory, not only was the monoclinal tilting of the range strata thought to be caused by the vertical forces of upheaval, but also the anticlines and synclines which occur as subsidiary features in some of the ranges were believed to have been produced at the same time and by the same upheaving forces, or rather by gravity in combination with the upheaving forces. This

singular conclusion was emphasized by the statement that corrugation, which was produced in the Appalachians chiefly by folding, was produced in the basin ranges chiefly by faulting. No hint was given of an earlier period of folding separated by a long erosion interval from a later period of faulting. The recognition of a long interval, but without mention of its erosional work, was made in 1878 by King, who, after referring to his previous description of the basin ranges as a "series of folds," and then crediting Powell and Gilbert—priority of naming being here given to the senior geologist rather than to the junior as author of the fault-block theory—with having "called attention to the abundant evidence of local vertical faults and the resultant dislocation into blocks," added: "Yet when we come to examine with greater detail the structure of the individual mountain ranges, it is seen that this vertical dislocation took place after the whole area was compressed into a great region of anticlinals with intermediate synclinals. In other words, it was a region of enormous and complicated folds, riven in later time by a vast series of vertical displacements, which have partly cleft the anticlinals down through their geological axes, and partly cut the old folds diagonally or perpendicularly to their axes." [2] King must have known that more or less erosion would have taken place between the times of folding and faulting, but he did not mention it specifically, probably because he, like many of the older geologists, was more interested in subsurface structure than in surface form.

VIEWS OF POWELL AND DUTTON

It was, then, the custom of the time that no special attention should be given to the erosion that must have taken place between the folding and the faulting, or to the forms produced by it. Yet the recognition of that essential phase of the problem had been previously announced by Powell, who briefly but clearly stated that, although the ranges of the Great Basin consist largely of Eozoic and Paleozoic rocks, their form and height show that they are of very late upheaval, the Great Basin before their upheaval having been "a comparatively low plain, constituting a general base level of erosion to which that region had been denuded in Mesozoic and early Tertiary time when it was an area of dry land." [3] A somewhat more explicit statement to the same effect was made by Dutton a few years later:

> The flexures of the Basin Range strata are not, so far as can be discovered, associated with the building of the existing mountains in such a manner as to justify the inference that the flexing and the rearing of the ranges are correlatively associated. On the contrary, the flexures are in the main older than the mountains, and the mountains were blocked out by faults from a platform [the rocks of] which had been plicated long before, and after the inequalities due to such pre-existing flexures had been nearly obliterated by erosion.[4]

The recognition thus given to a period of erosion between an earlier epoch of mountain making by folding and a later epoch of mountain making by faulting marks the beginning of a modern conception of mountains that has found wide application in later years; so wide, indeed, that it is now difficult to find any mountain range which exhibits forms due to folding and erosion alone, without a later period of upheaval and more erosion. Fundamental as this conception of two epochs of deformation separated by a long period of erosion is in the explanation of the basin ranges, it is manifestly not due to Gilbert. He, however, cites the extensions of his original theory proposed by King and Dutton—but not the extension proposed by Powell—in his posthumous essay and adds:

> The idea that the Great Basin district, corrugated by folding at the close of the Jurassic, had been reduced by erosion to a condition of low relief, aids the conception that the mountains of today were created by the later and disruptive deformation. It is distinctly Dutton's addition, although King had paved the way for it.

Then, after noting the confirmatory evidence found by Russell in the young fault-block ranges of southern Oregon, he goes on:

> It is a remarkable fact that during the development of a theory as to the essential structure of the ranges, the observers [including himself] who reported on the existence of faults gave no adequate statement of the evidence on which their determinations were based.

[2] Geological Survey of the Fortieth Parallel, I, 1878, 735.
[3] Geology of the . . . Uinta Mountains, 1876, 32.
[4] Geology of the High Plateaus of Utah, 1880, 47; also 7.

Just why Powell's contribution to the basin-range problem was not quoted, and why Dutton alone was credited with the two-cycle origin of the ranges is not clear; nor is it immediately manifest why Gilbert himself took no public part in the discussion which was for a time actively prosecuted among the geologists of the several governmental surveys in Washington. It is impossible to believe that he was not interested in the discussion, and that he was not personally acquainted with Powell's and Dutton's views before they were published; for the three men were closely associated during the very years when the discussion was at its height. Indeed, in spite of Gilbert's making no claim in his posthumous essay to any share of the new ideas which he there credited to King and Dutton, it is eminently possible that he had had a significant share in developing them, but that having had his own say in his report he left the announcement of the new ideas to his seniors. Such an interpretation of the case is much more consistent with the activity of his intellect and the generosity of his character than the supposition that he had dismissed the problem from his mind, or to imagine that, while carrying it in his mind, he made no contribution to it. Moreover, his silence was consistent with his known dislike of controversial discussions. For many years the basin ranges were mentioned in his reports only incidentally, when they were needed to make a setting for some other problem. For example, in a discussion, to be mentioned again in a later section, of the "origin of jointed structures" with especial relation to the jointing of the Bonneville clays, it was briefly announced that although there is evidence of post-Quaternary displacements in the region, the "movements were small and vertical, and the type of structure exhibited by all the surrounding mountains is one implying vertical displacement and no lateral compression." [5] Such a statement merely repeated what had been said before, without developing a new point of view.

GILBERT ON THE ORIGIN OF THE SIERRA NEVADA

Fortunately, however, there is a brief statement published in 1883 concerning the origin of the Sierra Nevada, which makes it clear that Gilbert had then become aware of the two-cycle development of that mountain range, and from this it may be fairly inferred that he had at the same time come to recognize the probability of a two-cycle development for the basin ranges also. This statement is to be found in a review [6] which he wrote of Whitney's "Climatic changes of later geological times," and to which further reference will be made in a later section. Whitney, following the geological philosophy generally accepted in his day, had assumed that the Sierras had been uplifted once for all in Middle Mesozoic time, and that their present relief represents simply the unconsumed residuum of the primitive uplift; Gilbert, on the other hand, interpreted the present Sierras as exhibiting the work of revived erosion following renewed uplift after the more or less complete degradation of their Mesozoic predecessors. He saw that the Sierran highland is an inclined plain; "its plateau character is not given by a continuous stratum of hard rock parallel to the general surface, but has been produced by the uniform erosion of a system of plicated strata. Such uniform erosion could only have been produced by streams flowing at a low angle"; and since the time when they flowed in that manner, the mountain mass has been uplifted with a slant to the west. The recency of the uplift is shown by its incompleteness, as attested by recent earthquake-making displacements along its eastern base. The same subject appears to have been further discussed under the title, "Stages of geologic history of the Sierra Nevada," before the Philosophical Society of Washington in 1887, but no adequate record of that communication is preserved.

INCOMPLETE STATEMENT OF THE BASIN-RANGE THEORY

The deficiency of explanatory exposition in Gilbert's early accounts of the basin-range faults is not easy to understand, unless it may be accounted for by the difficulty that is always attendant upon completely thinking out all the elements involved in a new theory, and the associated difficulty of writing them all down in precise phrases. As to the difficulty of precisely writing down his ideas, no one who is familiar with Gilbert's later reports would imagine

[5] Amer. Journ. Sci., xxiv, 1882, 50–53.
[6] Science, i, 1883, 141–142, 169–173, 192–195.

that he had ever been troubled about expressing himself clearly; yet one of his associates on the Wheeler Survey records that he was at that time "not a ready writer," and that it was only "as a result of care and diligent labor that he acquired the singularly simple and lucid style which later distinguished all his communications." As to the antecedent difficulty of completely thinking out the various elements of a theory, that also would not be attributed to Gilbert by those who knew his power of exceptionally keen and clear analysis as displayed in later years; yet such analytical thinking appears to have been in this case a serious task in his earlier life.

There is, however, another possible explanation of the deficiency of physiographic presentation. The idea that the ranges were upheaved and more or less carved fault blocks may, after its invention, have appeared to its inventor so simple, so self-evident, that it hardly needed demonstration or explanation. He was only a young and little trained explorer; and if he had come upon this idea in his first field season in the West, or, better said, if the idea had so early in his western inexperience forced itself upon him in place of the incompetent theory that he had brought from the East, how could it fail to be accepted by other explorers! He had surely talked it over with Powell, and Powell was at once convinced of its value and verity. Indeed how could anyone, on seeing the discordance between the trends of the mountain margins and the mountain structures, imagine that the ranges could be the erosional remnants of great folds! How could any one fail to see that each range is a unit, sometimes truly a complex unit, of displacement along great fractures that are independent of the range structures! After the truth was once perceived and briefly stated, why spend time in expanding and expounding a matter so manifest!

And there is also a third possible explanation, the clew to which is found in a letter that Gilbert, after finishing his second report for Wheeler, wrote late in 1874 to Powell when about to join his survey of the Rocky Mountain region:

> My application for permission to publish some of my data (whether the official report had appeared or not) was negatived by General Humphreys [under whose direction, as chief of engineers, Wheeler conducted his survey], and I feel little ambition to write anything for publication with the uncertainty that would hang about the date of its appearance.

Gilbert had enjoyed the satisfaction of prompt publication of his Maumee Valley study, which Newberry had approved in the liberally administered survey of Ohio; and the more rigid administration of the Wheeler survey under Army regulations was displeasing and discouraging to him. The report on even his first two seasons' field work had not then been printed; and although it is dated July, 1874, there is reason to believe that it was essentially completed at a much earlier date. It is therefore probable, that under these conditions of formality and delay, the active-minded young scientist was not tempted to elaborate his theoretical views. That he could have done so had he wished to is sufficiently proved by the fullness and promptness with which he completed a critical analysis of the Henry Mountains' problem three years later, when he enjoyed the favoring conditions offered by Powell's survey, as will be told below.

Hence under any explanation, one must read between the lines if he would appreciate Gilbert's full physiographic meaning; but in thus attempting to discover the tacit basis of the written passages, one runs the risk that attends the composition of all commentaries; the risk of, at a later date, reading into an author's words a larger understanding than he had in mind when he wrote them. Nevertheless, some interpretative comment on the first statement of so interesting and important a problem as the origin of the basin ranges by so able and original an observer and thinker as Gilbert is permissible, even necessary, when the problem is reviewed in the light of nearly 50 following years; and it has therefore been here undertaken particularly with regard to the physiographic aspects of his work. On that side, it may be confidently believed that his understanding was much greater than his presentation explicitly announced. If it be objected that, if this were true, he should have in his later writings made claim of the larger understanding that he had originally possessed, it may be confidently answered that, however much more he had known than he had said, he would have let his early reports stand at their face value; for, as already noted, the making of claims was not in his nature.

As to the geological phases of his work, no warrant can be found for an interlineation to the effect that he recognized, even though he did not announce, two periods of Great Basin deformation and their separation by a long interval of erosion. His views on that side of the subject were definitely enough expressed, and they were, as he himself clearly acknowledged, defective. It is for this reason declared, at the opening of the present review of the basin-range problem, that the more novel truths he contributed to it were physiographic and not geologic. Yet, curiously enough, a clue to the overlooked elements of the theory was presented near the eastern margin of the basin-range province in the Pahvan Range, which according to his first report should, "perhaps, be considered a southern continuation of the Oquirrh" Range, and which appears to be the example previously referred to without name, as affording evidence that the elevation of the ranges "was not all accomplished at once" (63). This range was described and figured as including in its western part a large underbody of deformed and deeply denuded Paleozoic rocks, the moderately uneven eastward slope of which is covered unconformably by Tertiary strata, dipping gently eastward; and from these occurrences the points made by King and by Powell and Dutton might have been inferred; but no such inferences were made.

Gilbert's theory of the basin ranges in its original form must therefore be regarded as seriously incomplete; so incomplete, indeed, that one may feel surprise at the importance it attained. But quite as remarkable as the incompleteness of the theory was the failure of his contemporaries to recognize its incompleteness. The strong objections that were urged against it by other observers were not advanced because certain essential elements were omitted, but because certain elements that it announced were believed to be wrong; and the theory that these objectors adopted was as incomplete as Gilbert's was. The fact is that both the theories of the basin ranges then current represented geological science as it was developed at that time; both were largely concerned with structure and little concerned with erosion. To-day erosion enters as an essential element in the explanation of the basin ranges, and no theory of their origin can be regarded as complete which does not give to this external process as full a measure of the attention due to it as is given to the internal deforming process. But this comment, so easily made half a century after the theory was propounded, could not have been made at the time of propounding the theory. Viewed in the light of its own epoch, the theory was an important step in geological progress, because it took account of the upheaval of individual mountain ranges without compression at a time when mountain ranges were believed to be the result of lateral compression.

Gilbert's return to the basin-range problem in 1901 is described in a later section.

CHAPTER VII

FIRST WINTERS IN WASHINGTON

EXTENSION OF SCIENTIFIC ACQUAINTANCE

Although Washington in the early seventies was by no means the scientific center that it has since become, it was already the residence of a good number of distinguished men of science holding Government positions, and in this respect it must have presented an enlivening contrast to the other cities that Gilbert knew; for Rochester and Columbus were at that time of small size and of somewhat rural quality, and even in metropolitan New York, science was then more hidden under trade and traffic than it was submerged beneath politics in the National Capital. Hence, after the young geologist, returning late in his twenty-eighth year from his first season of field work in the West, arrived in Washington near the end of January, 1872, and reported at Lieutenant Wheeler's office, 1813 F Street NW., he soon found opportunity for forming acquaintance with his scientific elders, among them Baird, Newcomb, Powell, Hayden, Meek, Hilgard, Harkness, Abbe, Dutton, and others whom he met and learned to know at meetings of the Philosophical Society, organized only a year before, and elsewhere. Judging by what followed in after years, it is not to be doubted that he made a good impression on all his seniors. Indeed, brief entries in his diaries record that on May 18, 1872, Gilbert himself spoke at one of these meetings on "Nev. and A. T." (Nevada and Arizona Territory); a year later he addressed the same society on "Weighing the Earth by Col. Cañon," and in February, 1874, he harked back to 1868 and gave the "G. & G. Soc.," apparently a combination of geologists and geographers later organized separately, an account of "the Cohoes Cedars time data," as already noted. The entries in his diaries for this year and the next also indicate that a good number of evening calls were made on his new friends; and one of the earliest of them, Baird, then Secretary of the Smithsonian, invited him to establish his office in that institution, an invitation to which Wheeler naturally enough objected. Many days in the first months of 1872 are summarized with the single word, "Boning," at the top of the page, which presumably means "Working on field notes," for the manuscript of a preliminary report was completed March 17. In February the study of German was taken up, a subject in which progress does not appear to have been great; it was taken up again 12 years later, with no greater success.

But lest it may be thought that the young man was altogether given over to the more serious aspects of life, some lines entered in his diary under date of March 28, 1872, and therefore after two months of elevating scientific associations, may be here quoted: "Afternoon Marvine and I threw ball near the market until stopped by the police. Evening heard Nillson in Faust from negroes' heaven. Music and acting was great." Then comes a note, "Steamed oysters are a trifle better than coagulated clams," which might rival the famous mystery of "chops and tomato sauce," were it not followed by an entry in the petty cash account habitually kept near the bottom of each daily record: "Std. oys. . . . 80," from which it appears that the evening must have been closed with a post-operatic revel. Evidently, geology was not all absorbing. Indeed in the following winter Gilbert and Howell took dancing lessons with a view to cultivating the social amenities to which neither of them seem previously to have paid much attention. Truly, that a hardy devotee of science in the summer should in the winter not only carry his disorderly conduct so far that it had to be restrained by the public guardians of good order, but should even attempt to "trip the light fantastic toe" as a means of ingratiating himself in gentle company, is not what a reader of his sententious geological reports feels that he has a right to expect! However, Gilbert's alma mater does not appear to have been disturbed either by hearing of or by foreseeing these pranks and dissipations, for in the summer of 1872 the University of Rochester awarded him the degree of A. M.

After the season of 1873 in the West, scientific comradeship had a fine illustration. Gilbert, Howell, and Henshaw returned from Salt Lake City to Washington by train together and decided on the way to establish winter quarters in common near their chief's office; and as

Gilbert was the originator of the plan he was put in charge of the billeting. It may be doubted whether the memoir of any other academician will ever contain record of so daring an exploit as his, now to be narrated; indeed the "cheek" with which he had approached the Governor of Ohio three years before pales in comparison with the boldfaced seductiveness that he must have here displayed in the siege of several ladies much older than himself; for he had to make the round of the selected neighborhood, and call at each one of the more attractive-looking residences, none of which were of such quality as to set the sign, "Rooms to let," in a front window; and then having presented himself as ingratiatingly as possible to the lady of the house, inform her that he and two other young men of scientific occupations desired, if they should prove acceptable, to become lodgers under her roof. The astonishment not infrequently inclining to indignation that was exhibited by several of the matrons when thus interviewed by a total stranger without a letter of introduction formed the subject of successive reports of progress by the scout to his companions, and furnished them with much hilarity for several days. It surely speaks volumes for Gilbert's appearance and poise that not a single absolute refusal was encountered; but the lady who was finally chosen by the adventurers to be their hostess—her residence was the brick dwelling house still standing at the northeast corner of Eighteenth and G Streets—firmly stipulated that she must see the other partners also before agreeing to house the trio for the season. So, clad in their best raiment, they made a formal call, Gilbert presenting the other two; the arrangement was thereupon pronounced agreeable to all concerned, and so it continued until another field season opened. At the same time a boarding house was chosen on A Street between Sixth and Seventh, distant nearly 2 miles, with a view of securing exercise if not appetite by walking back and forth, rain or shine, twice a day.

These good companions gave part of nearly every Sunday of their winter in Washington to cross-country walks, usually with invited company. A favorite district for their rambles ay beyond the Eastern Branch of the Potomac, where patches of primitive woodland then alternated with small farms; it stretched southward from the end of a street-car line that crossed an upstream bridge, to a line that returned by another bridge farther downstream. Another route of preference led across the Potomac at Georgetown and up the southwest bank of the river toward Great Falls as far as inclination prompted or as time permitted. Boating on the river was also a frequent diversion; time for that was found especially in the summer of 1874, which, unlike summers earlier and later, was mostly spent in Washington. For entertainment indoors, Gilbert enjoyed playing cards and excelled at euchre and whist; and he was particularly fond of inventing new and eccentric ways of playing old games. He often read aloud to his intimate friends, much to their pleasure, the book being chosen for entertainment rather than for information. Probably few who, in those years of long ago, knew Gilbert only on the scientific side realized his inner nature to be so emotional that if, while reading aloud, he came upon a pathetic passage, even his strong self-control could not wholly master tear ducts and vocal chords; his eyes would overflow and his voice would choke, so that he must hand the book to another to continue the story. For the same reason, while enjoying the theater, he avoided distressing melodramas, as he did not like to "make a spectacle of himself" in public.

Gilbert was known among his Washington associates of this period as a man of cheerful and buoyant nature, of large vision and keen thought, modest in disposition, kind and courteous in behavior. His power of unbiased observation and unprejudiced discussion was recognized by his colleagues, who saw that he could attack a new problem with an open mind; that he never tried to prove a preconceived theory but only to find out the truth. Although engrossed in his work during working hours he was also sociably inclined, as has been told; and his friends, mostly of the "scientific set," greatly enjoyed his company; for he had, apart from a serious interest in serious matters, a lighter side that was shown in a cheerful manner, a fund of good stories and a jovial laugh. When he cared to express himself, he was simple, direct, unconventional, and outspoken; yet he had withal a quiet reserve that guarded him from saying too much, a reserve that later developed into a very gracious dignity. The even temper that had carried him through the many discomforts and difficulties of western field work was developed into a philosophy of self-control which taught him that anger was

an unwise passion, not only unscientific and useless but detrimental; and he therefore did not allow himself to be angered. His power of will, thus shown in a small way, was hardly called upon to resist the greater temptations which beset many mortals, for they did not assail him. His was one of those happy dispositions that was pure-minded and honorable by nature. His good sense early ripened to wisdom and showed him the better paths of life, from which he never departed.

MARRIAGE AND HOME MAKING

It was probably not Gilbert's scientific acumen so much as the other side of his nature that made him an engaging companion in the social circles he frequented; unconventional circles, that had, be it noted, no relation to what even the Washington of those simpler days would have called "Society" with a large S. In any case he does not seem to have lacked agreeable partners at evening dances or on Sunday walks. His diary for 1874 contains the following brief entry among many others of less significance; January 10: "Met Fannie Porter"; and according to the best judgment of the few survivors of those years, this meeting probably took place at a dance at Powell's house, for the "Major," an experienced ethnologist, regarded dancing a proper pastime for the human young, and dances at his house were not infrequent. Three other entries are correspondingly suggestive in view of subsequent events. Sunday January 25: "A long walk to Soldiers Home &c. with Miss Fannie Porter"; Sunday, February 1: "P. M. with Miss Porter crossed the Long Bridge and returned via Arlington & Georgetown," Wednesday, February 4: "Called (PPC) on Miss Porter." The Long Bridge that was crossed over the Potomac would seem to have run in about the same direction as the Long Walk on Boston Common that was followed in equally acceptable, indeed accepting, company by Holmes's Autocrat of the Breakfast Table; for 10 months later, when Gilbert was in his thirty-first year, his associates in Washington, even the most intimate of them, were surprised by the announcement of his marriage to Fannie Porter at her home in Cambridge, Mass.

Miss Porter was the younger sister of the wife of Gilbert's friend, Archibald Marvine, who was then a member of the Hayden survey and resident in Washington; and it must have been on the occasion of a visit of the younger sister to the elder that the meeting briefly recorded on January 10, the Sunday walks, and the leave-taking call on February 4 took place. Although Gilbert did not spend the Sundays of February and later months in solitude, he visited New England in the following summer; and on August 4 noted in his diary: "A search for Winthrop Sq. Hippodrome &c. Barnum." The interpretation of the first entry is that Winthrop Square was the residence of Mrs. Porter and her daughter in Cambridge; and after that is known the second entry hardly needs elucidation. The next day's record is: "Phaeton AM. Fresh Pond PM.", and between the leaves of the diary which inclose these dates there is still pressed a little three-leaved maple seedling, such as may to-day be gathered on the wooded slopes which border Fresh Pond on one side. Four days later the two young persons, not so very young either, but apparently suitor and suited, went to Winchendon, near the New Hampshire border, to visit Miss Porter's eldest sister, Mrs. James A. Whitman, there resident; and while still in that pleasant town the diary records on August 10: "A drive past the springs. Croquet. Under the Trees. A boat ride." The next day return was made to Cambridge. Several days following were spent by the suitor alone in a gathering of scientists at Hartford, where the summer meeting of the American Association was held, as already told; and after that a fortnight was given to Rochester, when "Writing for the Tribune" on subjects unknown, took some of the mornings. Sunday, August 30, was enjoyed playing with the "Full Family at the Nutshell, The Intellectual Game," whatever that may have been; and the next day contains the entry: "Loomis-Gilbert. No Cards." Thus the brother briefly records the marriage of his sister. Washington was reached a few days later; and if questions arose there as to the cause of the month's absence, Hartford and Rochester must have fully answered them.

Two months later, on Tuesday, November 10, 1874, Grove Karl Gilbert and Fanny Loretta Porter were married in Cambridge. The bride was the third daughter of Joseph Porter, jr., who had died 15 years before, and Susan Maria (Bent) Porter, who died December 21, 1874, six weeks after her daughter's departure. The Porter family was well represented at

the wedding; but the Gilberts were represented by the groom alone. The wedding journey was extended to Rochester where Thanksgiving Day was spent at the "Nutshell," and then the pair went to Washington for the winter. The other two of the three bachelor contubernals of the previous winter were invited, in the letters by which they were first informed of Gilbert's defection, to continue the plan of house partnership; but they, with a prudence as great as the generosity that had prompted the invitation, declined on the grounds, facetiously urged, that the fourth partner was a stranger to them, that they had not been consulted in choosing her, and that they could therefore assume no responsibility for the new enterprise; but they appear to have enjoyed the opportunity of frequent and friendly visits at the benedict's home.

Washington was thereafter Gilbert's residence, except for a year, 1880–81, when, after the organization of the United States Geological Survey, he resided in Salt Lake City in charge of the division of the Great Basin. During the summer of 1875, which he spent in the high plateaus of Utah, as will be told below, his wife was with her relatives in Winchendon, Mass., where Gilbert rejoined her in mid-September, and where their first child, Betsy Bent Gilbert, was born on October 13. Shortly after returning to Washington a month later, boarding in the city was exchanged for housekeeping in Le Droit Park, a suburb frequented by survey members of that time. There, as before, Gilbert, always cordial and hospitable to his many friends, led a quiet and simple life. The diaries of this period contain occasional items which show that the exploring geologist could, when the time came for it, attend faithfully to domestic duties, perhaps all the more so because his means were restricted and his housekeeping was necessarily on a very moderate scale. Like many another young husband, he opened a set of books for double-entry accounts, but like very few others he continued to keep them regularly for over 40 years. His last balance was struck with much labor a short time before his death. In the Le Droit household the sums set aside for domestic expenses were charged to "Dame Durden" or "D. D.," but unlike some other scientific husbands, Gilbert did not allow all the house cares to fall on his wife so that he might be uninterrupted in his professional work. He took a good share of responsibilities, as is indicated by many entries in his diary; for example, one made shortly after the removal to Le Droit Park, which reads: "To-day we begin 3 pts. of cream and 3½ qts. of milk." It may therefore be fairly inferred that milk bills and all other bills were closely scrutinized. He was devotedly fond of the little daughter, Bessie; the affection that he felt is recalled by items in the journal. January 14, 1876: "Bessie 13 lbs.," and a few days afterwards: "Rattle, 0.25." The following autumn when he was in the arid and empty plateau country, a notebook bears the entry: "Bessie a year old today." These little touches are sad reading in view of the heart-breaking grief that the father felt over the daughter's death six years later.

During the winter of 1875–76, Gilbert was caused much anxiety by the fatal illness of his friend and brother-in-law, Archibald Marvine, "a conscientious, able, and vigorous geologist," who had spent the previous summer as a member of the Hayden survey in the mountains of Colorado; there, as Gilbert wrote in an obituary notice, a long season of "toil and privation in that wilderness of cañons, crags, and peaks undermined his health." After his death, on March 2, Gilbert accompanied the widowed wife to the interment at Marvine's home in Auburn, N. Y.; and thereafter extended to her and her affairs in Washington a most brotherly care. Gilbert's first son, born December 6 of the same year, was given the name of this much esteemed associate.

With the increase of scientific opportunity that followed Gilbert's transfer from the Wheeler to the Powell survey, as told in the following section, he took a more active part in the intellectual life of Washington, and largely through the meetings of the Philosophical Society of that city contributed a good share of information concerning his western field of work to his fellow members. Among the topics that he treated are "Wind-drift erosion" in 1875; "Landslips and lake basins" in 1876; "Drainage system of the Black Hills," "Recent history of Great Salt Lake," and the "Wasatch a growing mountain" in 1878; the "Kanab base line and a proposed new system of base measurement," and "Air currents on mountains slopes" in 1879; and "Relations of Permian beds to the Aubrey limestone" in 1880. Unfortunately the printed record of these communications is very brief, but some of their subjects will be recognized as being fully set forth in his official reports.

CHAPTER VIII

FIVE YEARS ON POWELL'S SURVEY

ACQUAINTANCE WITH POWELL

Gilbert's association with Powell was the greatest determining factor in his mature life. It lead him to broad opportunities which he greatly enjoyed and it imposed upon him heavy sacrifices which he loyally made. The two men first met in Washington in the late winter or spring of 1872, probably at a meeting of the Philosophical Society, and acquaintance was already well entered upon before they went west for the summer field work of that year. At the close of the season, when Gilbert on returning from the Plateau province was in Salt Lake City, he invited one of his nongeological associates on the Wheeler survey to call with him on the "Major," then in charge of the "Geographical and Geological Survey of the Rocky Mountain region," and on the way spoke of him in terms of high praise as a man whom it was a privilege to know. The visitors were cordially received and the conversation naturally turned to geological problems, especially those of the region north of the Colorado Canyon, where both Powell and Gilbert had spent several previous months in field work. The third member of the party afterwards remarked to his companion upon Powell's extraordinary frankness in telling of his new results, which he imparted quite regardless of the fact that his visitors were members of what might be considered a rival survey; but it may be well believed that the frankness on Powell's part was in no small measure a response to his recognition of a fine sense of scientific honor in Gilbert.

The cordial relation early established between the two men is well illustrated by a letter that Gilbert wrote to Powell from Fort Wingate, N. Mex., under date of July 17, 1873, soon after arrival there at the opening of his third western campaign on the Wheeler survey:

> I reached here by buckboard four days ago and have been skirmishing in the neighborhood for a geological start. The lithological series is well exposed but I cannot find a fossil—except the Shinavav wood—between the Permo-carboniferous and the Cretaceous. . . . One day I spent on the dislocation that Newberry described. I think he and you, too, must have crossed it at Stinking spring on the Puerco of the Colorado Chiquito. A few miles further north the structure is perfectly exhibited—a regular flexed fault without the slightest fracture, and with a throw of 2500 feet to the west. From a high point I could see it for twenty-five miles at least.

In closing he expressed the wish that Powell rather than himself should see the Indian village of Zuñi. "You would make the visit profitable while I shall merely gratify idle curiosity." Clearly the two geologists were on familiar terms with each other at this time, and the acquaintance so well begun rapidly grew into a close intellectual comradeship; witness the interchange of ideas about the Colorado Canyon and its problems, as noted above. In a few years the relation of the two men became like that of older and younger brothers who had complete trust in each other.

The contrast between Powell and Wheeler must have had, on Gilbert's side, much to do with the intimate association that sprang up between the director of one survey and a subordinate on another; for while Gilbert's relations with his own chief were, so far as known, always friendly and pleasant, they could not, in view of Wheeler's ignorance of geology, have been a source of scientific inspiration, such as intercourse with Powell must have been from the first. Moreover, Gilbert was not only hampered in his geological work under Wheeler by its subordination to the topographical objects of the survey, as has already been told; he chafed under its military cast and was annoyed by the many restrictions imposed upon its conduct by reason of rulings from military officials "higher up." Indeed, its chief himself was not free from embarrassment caused by these strict requirements. A quotation made above from one of Gilbert's letters explains how his wish for the early publication of his results was negatived by the Chief of Engineers—doubtless with entire propriety from the standpoint of an engineer in chief—and how little ambition he felt to write reports the publication of which would be post-

poned to uncertain dates. Hence, after three field seasons under Wheeler and a fourth season of report writing in Washington, he gladly accepted the opportunity of taking up work under Powell. Wheeler, who had himself spoken freely of his admiration for Gilbert as a man and for his ability as a geologist, generously approved of the transfer and recognized that opportunity for fruitful results was likely to be thereby greatly augmented. This was very clearly the case.

Gilbert's connection with the Wheeler survey closed on September 30, 1874. While he was in Rochester at the end of his wedding journey he wrote, on November 27 of the same year:

> My Dear Major: . . . I am getting to be a little anxious to be at work—partly because it has come to be more natural to me than play, and partly because I ought to be earning something. So I am going to Washington in a few days with the intention—if you have not changed your mind—to begin work with you at once.

On the very day, December 2, of his arrival at the National Capital, he formally joined the Powell survey, and remained a member of it until, five years later, the various separate surveys were merged in the United States Geological Survey.

These two master minds were, from the first, admirably suited to work together, for where they were not alike one was in large measure complementary to the other. Powell had, in addition to his proved ability as an explorer, an extraordinary capacity for organization and administration, and after the establishment of the National survey he most successfully used this capacity in providing means for the prosecution of special studies by men like Gilbert—or as nearly like him as could be found; while Gilbert, caring little for directing the work of others but excelling in field work of his own, was still more excellent in the theoretical discussions to which his field observations led. Moreover, Gilbert became more and more deliberate as he gave an increasing share of attention and thought to research, seeking out all the possibilities of every theoretical explanation for observed phenomena, and thus testing the worth of every explanation that came to him from whatever source; and at the same time Powell, while turning more and more from research to administration, nevertheless still enjoyed the occasional exercise of his unusually speculative intellect in the solution of problems that his associates encountered; but he was well content to leave the closer definition of his suggested solutions to men of Gilbert's analytical power. More important still, these two men had entire confidence in each other; they cared little for personal priority or individual credit; in their generous devotion to the search for scientific truth each gave the other free use of his every thought, asking no greater reward than that the thought might be of service. What Gilbert said of Powell at a memorial meeting in 1904 was equally true of himself: "Phenomenally fertile in ideas, he was absolutely free in their communication, with the result that many of his suggestions—the number of which can never be known—were unconsciously appropriated by his associates and incorporated in their published results."

Gilbert must have been greatly refreshed and invigorated by the favorable conditions afforded on Powell's survey. From his former chief he could have had no helpful counsel on geological problems; the study of the basin ranges was surely not advanced toward its solution by any suggestions or criticisms from Wheeler; but stimulating and helpful counsel was received from his new chief at every turn. During the investigation of the Henry Mountains, the first problem upon which Gilbert reported for the "Survey of the Rocky Mountain region," each step in the discussion was submitted to Powell for criticism, and many of the criticisms thus received were accepted by Gilbert and embodied in his text. Little wonder that the scientific companionship of the two men endured, or that when, a few years later, the unified National survey came under Powell's direction, Gilbert as one of its leading geologists more than replaced the right hand which the director had lost in the War of the Rebellion 20 years before.

FIELD WORK FOR THE POWELL SURVEY

Gilbert's first field season on the Powell survey was spent on the high plateaus of southern Utah and in the vast amphitheater of excavation to the east of them. Salt Lake City was reached on the outward journey on June 16, 1875, and on the homeward journey on September 12. The huge, east-reaching plateau spurs, known as the Aquarius and Kaiparowits, as well

as the great Water-pocket flexure in the lower land east of the plateau base, were examined in some detail; and as these features are among the finest of their kind in the known world, the season must have been of great profit; but no report was made upon it. That appears to have been reserved by Powell for Dutton, who spent the summers of 1875, 1876, and 1877 in the high plateau region, and whose excellent account of it was published in 1880. Gilbert's published results concern only the Henry Mountains, which, lying east of the high plateaus, were briefly examined in 1875, and more closely the following year, as will be told on a later page.

During both these summers the change from the irksome restrictions under which Gilbert had been working on the Wheeler survey to the favoring conditions afforded on the Powell survey was greatly enjoyed. Under Wheeler, Gilbert's movements had been largely subordinated to those of an expedition, the chief object of which was topographical mapping; he had been hampered, not to say harassed, in his field work by military regulations, one of which is illustrated by the incident of the lost carbine in the Colorado Canyon in 1871, already told. Under Powell, he was given the freest possible opportunity to move, with a small party of which he was the head, over whatever route he selected; and his own geological studies were the main object in view. He could thus himself turn the pages of the great book he was reading, and ponder on each page as long as he wished. Evidently his transfer from a survey conducted by an Army officer under military regulations to a survey conducted by a civilian scientist under very free conditions was as salutary as the Army officer had predicted and as the civilian scientist had hoped.

It is, however, not clear why Dutton and Gilbert were sent by Powell in the summer of 1875 to examine the same field, thus introducing into a single survey the duplication of work that was complained of when committed by rival surveys. It may be that Gilbert, having already spent a good part of the summer of 1872 in the high plateaus, recognized their blocked structure to be in a measure intermediate between the broadly extended masses of essentially horizontal structure in the Colorado Plateau—the plateau trenched by the Colorado River in northern Arizona—and the linear masses of deformed structures in the basin ranges; and that he wished to see the plateau blocks again in the hope that they would throw light on the basin-range problem. In any case his visit of 1875 taught him much about the region, although a good share of his time was spent on the denuded area east of the lava-capped highlands. It is to be regretted that his results remain unpublished; but instead of writing a report on what he had learned in the summer, he spent at least a part of the following winter in working up illustrations for the Uinta Mountain report by Powell, who noted in its preface: "To Mr. Gilbert, I am indebted for great assistance in the preparation of the graphic representation employed." In the atlas accompanying this report, Gilbert is credited with Plate IV, a bird's-eye view or block diagram of a part of the Uinta uplift, showing the actual topography of the district in the foreground, and a stereogram of the imagined uplift, unworn, in the background; and this is believed to be one of the earliest published examples of a compound block diagram of such design. It is, therefore, one of the many novelties which geology and physiography owe to the ingenuity and good sense of this self-trained investigator.

While Gilbert's notebooks, five in number, of the summer of 1875 on the high plateaus, show that he enjoyed much greater freedom in making records than had been possible under Wheeler, generalized descriptions are nevertheless rare. The pages are filled with detailed geological sections, explained by an abbreviated notation that is not always easily interpreted; but intercalated among these are many items of special interest. An Inoceramus was found that measured 43 inches across; a worthy rival of the giant Tridacna in the South Seas of to-day. "Witch pinnacles," or slender columns of weak gravels capped by large bowlders, were figured as occurring on the retreating eastern slope of the Kaiparowits Plateau, and a curious note follows a near-by sketch of several of them:

Opposite the point we climb is a red pinnacle 5 m. out in the valley as slender & as inclined & probably on as large a scale as the tower of Pisa.

No other account of this natural leaning tower is known. Time was repeatedly taken for records of mountain and valley breezes, descending at night and ascending by day, and Gilbert later spoke on this subject before the Philosophical Society of Washington. A remarkable discussion of the relation between the diurnal range of air temperature and the "barometric horary curve" was made on September 6; this evidently foreshadows a graphic discussion of the same problem in an address 10 years later, as will be told in due season. East of the plateaus the greatly denuded Water-pocket flexure—recorded in the notes under the name of "Escalante"—is described as of unsymmetrical anticlinal form, with steep dips on the east and very gentle dips on the west; its axis is slightly arched, so that the Vermilion sandstone, removed from the highest part of the flexure, overrides it in strong cliffs at points 30 miles apart north and south and these two crosswise cliffs are "connected by continuous (except for cañons) escarpments in the two slopes of the fold, the NE standing under the steep fold slope & the SW standing from 5 to 10 miles down the gentle fold slope. So there is a continuous cliff of erosion returning in itself & facing inward—the reverse of a mesa. Inside the Vermilion the Shinarump circles in the same way. I will call the upper of the cliffs 'The Circle Cliffs'." The phrase, "reverse of a mesa," is certainly felicitous.

The faults and flexures between the various members of the high plateaus were examined with care, and the relative values of displacement and erosion in producing the existing relief was especially studied. One of the most deliberate records, written "on a crag overlooking the lake," July 8, 1875, relates to this problem:

What was the origin of Fish Lake Valley? 1. Its walls are of trachyte. Are they massive eruptions over dikes—hills of eruption? No, for they exhibit in their escarpments a bedded structure & their slopes are too steep & too definitely angled at top . . . 2. Is it the result of aqueous erosion? No, for it is too broad to have been cut from the trachyte since the age of the trachyte, and its side cañons are disproportionally small . . . 3. It is not the result of glacial erosion, for the glaciation of this region is but slight—entirely inadequate to the making or shaping of a great valley. 4th. It remains to suppose that since the trachyte epoch the floor of the valley has sunk (or the adjacent hills have risen) an amount equal to the present depth of the valley, plus the depth of its detrital filling, plus the loss of the uplands by waste—an amount somewhere between 2000 & 3000 ft.

There was no "jumping at conclusions" in a study thus conducted.

TWO VISITS TO THE HENRY MOUNTAINS

The most important observations made by Gilbert during his first season of field work under Powell concerned the Henry Mountains, which rise in an arid and relatively inaccessible region east of the Water-pocket flexure and north of the Colorado River. They were examined in the fortnight beginning August 19, 1875, and the structures then discovered proved to be of so great interest that the mountains were made the subject of a special investigation in the campaign of 1876, when two months of a field season that extended from early August to late November were devoted to them. Thus originated one of Gilbert's most famous studies. The mountains are conspicuous objects in the eastward prospect from the rim of the high plateaus, from which they are 30 miles or more distant; and it is probable that their striking forms rising in the distance excited Gilbert's wish to look at them more closely. On the way across the intermediate barren country he had a distant sight of Navajo Mountain, south of the Colorado Canyon, and discovered that, like the Henry Mountains, it also has upturned strata around its base:

On all its visible flanks, from NE around by N & W to SW, [it] is built of Trias rocks dipping away from its center. It is truly a volcanic cone of elevation. . . . I measure 11° at the NE & 15° at the SW on the flanks—dips that would carry the Trias almost to its crest, but its crest is dark with lava.

The novel structure of the Henry Mountains appears to have been recognized in the fortnight of their first examination, and the problem of their origin was then in part formulated; for on September 5, a question is recorded:

By the way, how do the flexures of Ellsworth and Henry V [members of the group] & Navajo consist with the violeuce belonging to irruption & eruption. Can deeply buried strata be bent without the slowness of action necessary at the surface ?

It will appear in the sequel that the answer to this question was most ingeniously worked out in the summer of 1876, when the facts of form and structure were carefully ascertained. In both years Gilbert had the services of W. H. Graves as topographer. The five notebooks of that second summer contain many sketches of the mountains, and it is evident from the numeration of prominent points and from comments concerning them that Gilbert also contributed largely to the work of topographic mapping. Indeed from Powell's report on the work of his survey for 1876, one might suppose that Gilbert was not accompanied by a topographer, but did all the work himself:

A topographic survey of the Henry mountains was made [by Gilbert's party] in 1875, and a map constructed on a scale of 4 miles to the inch; but this being thought too small a scale to admit of correct representation of the details of the geology, Mr. Gilbert in addition to his geological work made [in 1876] a more detailed survey of the topography, carrying on a complete system of secondary triangulation and a connected plane table sketch over more than 1,000 square miles. The data collected are sufficient to make a topographic map of the Henry mountains on a scale of 2 miles to an inch.

In any case Gilbert was much interested in the topographic work. On October 12, a rainy day spent in camp near the northernmost member of the mountain group, two pages were given to a calculation of "spherical excess," in order to determine whether allowance should be made for it in local triangulation; the result showed that "the excess of the whole polygon of triangulation is not greater than 5″ and is quite inconsiderable as compared with the probable error of measurement of one angle, 1 minute." The many-sided competence which such an entry illustrates was characteristic of all Gilbert's work.

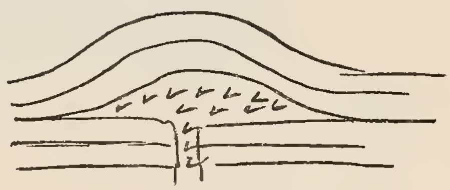


Fig. 6.—Ideal section of a laccolith; from Gilbert's notebook, August, 1875

The novelty of the structural features discovered in the Henry Mountains was so great as to compel close scrutiny before they were accepted as definitely determined even by their observer. It was indeed so great as to awaken doubts in the minds of distant readers as to the competence of a little-known geologist, as Gilbert then was, who should declare that rising lavas could bend upward the strata into which they rose. Such an explanation recalled the discredited theory of "craters of elevation," which after strong advocacy by eminent geologists of an earlier generation had been gradually disproved and abandoned by the leading geologists of Gilbert's time. It was like a backward step to return to any such idea. But there was this difference: "Craters of elevation" were superficial phenomena; and the upturned strata of the Henry Mountains, although now visible at the surface, had become so only as the result of an enormous denudation; the upturning was a deep-seated phenomenon. Whatever uncertainty others may have felt on this point, Gilbert really proved his case beyond all possible doubt. Yet in spite of the large theoretical importance of the structures thus made known, their region is so remote that, since Gilbert's visit, only one other geologist has given close study to this singular mountain group, and his report upon it is not yet published. So remote indeed is the district of the Henry Mountains from lines of ordinary travel that it is known in the surrounding region as a safety zone for those who wish to "avoid doing business with the sheriff." During the Great War, certain persons who were under suspicion of the Military Intelligence Division of the Army chose this very district as a refuge because of its inaccessibility and lonesomeness. After they had reached its seclusion they were visited by an officer disguised in his usual occupation of geologist, and so completely did their isolation render them harmless that they were let alone in their voluntary internment.

THE HENRY MOUNTAINS REPORT

The problem of the Henry Mountains must have been truly of an inspiring nature, for the report upon it was completed March 1, 1877, four months after Gilbert's return from the second season in the field. Its date of publication on the title-page is the same year, although by reason of delay in preparing certain plates, the report was not completed and distributed till 1879; a second edition was issued in 1880. Short as was the time given to its preparation, it is one

of the most instructive reports ever issued. Gilbert's thoughts must have frequently turned, while he was still in the West, upon the form in which the report should be cast; and he was doubtless aided in such thoughts by an indifference to, not to say a dislike for the companionship of the usual run of camp helpers, with whom he had little or nothing in common. It appears to be in part at least for this reason that he formed the habit of setting out from camp early and returning late, thus securing an interval of 14 or 16 hours when he might be alone with his problems. But this does not mean that he was indifferent to camp work; he was well informed as to the duties of every man and beast in his outfit, and in this respect the apprenticeship in the Wheeler survey served him well. He made definite arrangements at the outset of a field trip as to the distribution of work, and held every member of his party to a high standard of performance. He very properly left the work of the camp to his men, not so that he should have no work to do himself, but because he had plenty of work of his own which his men could not possibly perform.

Moreover, his field work was not by any means limited to observation. He constantly carried on a mental inquiry as to the meaning and interpretation of observed facts, and in certain problems much of the inquiry was written down in the field, as extracts to be quoted below from his notebooks will testify. His report is, however, not chiefly a transcript of his field records, cast in narrative form. Its first 50 pages contain an able-minded generalization of the facts of the Henry Mountains district; and the second 50 are devoted to a keenly analytical

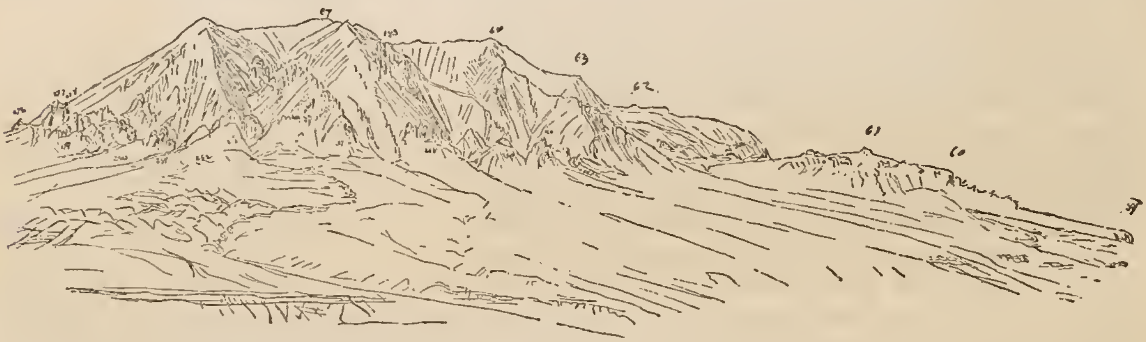


Fig. 9.—Mount Hillers, Henry Mountains; from Gilbert's notebook, 1876.

inquiry as to the processes by which the facts of the district are best explained. When this is understood one must marvel at the rapidity with which the report was put into shape, all the more since the shape into which it was put represents the very best form of investigational procedure. Surely whatever were the difficulties its author had previously experienced in expressing his ideas in writing, he had now overcome them.

As to the 50-page discussion of the processes of erosion which follows the 100-page account of the Henry Mountains, that is expanded from a more condensed statement of earlier preparation which makes part of the illuminating and masterly article, already analyzed, entitled "The Colorado Plateau province as a field for geological study;" an article which appears to have been largely prompted by earlier observations in the plateau province as well as by the field work of 1876 on the high plateaus and their eastern margin, where the phenomena of erosion are displayed on a magnificent scale; but the expanded discussion contains many lessons from the Henry Mountains also.

The illustrations of the report call for mention. They include, in addition to a good number of smaller figures, 13 full-page views, chiefly of the laccolithic mountains, based on Gilbert's own pencil sketches in the field. The originals, while not possessing the exceptional artistic quality of Holmes's landscapes, give a good idea of the barren mountain forms; and they also show that, had he been given time enough in earlier years, Gilbert might have prepared many helpful outlines of the basin ranges for his Wheeler survey report, which as a matter of fact is almost without such illustration, most of its figures being sections. Some of the Henry

FIG. 7.—MOUNT HOLMES, HENRY MOUNTAINS, LOOKING SOUTHEAST

Photograph by H. E. Gregory, U. S. Geological Survey

FIG. 8.—MOUNT JUKES, HENRY MOUNTAINS, LOOKING WEST

Photograph by G. R. Longwell, U. S. Geological Survey

Mountains sketches, Figures 34, 39, 42, 48, 49, were redrawn in ink from Gilbert's originals by "F. S. D." (Dellenbaugh), who had been with Powell on the Colorado in 1871 and with Thompson in the Henry Mountains in 1872; a special red ink had to be used and the lines kept very thin for reproduction by a "photographic process" new at that time. Other sketches, Figures 16, 27, 36, 43, 44, were more conventionally worked up as woodcuts in artisanlike fashion, with little appreciation of western scenery. Both styles of "copies" depart undesirably far from the originals; the woodcuts in particular, overloaded with monotonously uniform lines, give the impression of dark landscapes under a cloud-covered sky, altogether inappropriate for the arid plateau country. A few halftones, here reproduced from Gilbert's originals, show that very little "working up" was needed, for rough as his drawings were, every touch by which the original is "improved" by some one else than the observer himself is likely to introduce departures from nature. Special mention should be made of the frontispiece to the report, a block diagram in two sections, like the one prepared by Gilbert for Powell's Uinta Mountain report above noted; it shows Mount Ellsworth in the foreground, and a stereogram of its restored dome in the background, with indication of the strata involved on the right side of the block; this was evidently Gilbert's design, although it was drawn by some one else.

Besides these pictorial views, the report includes two full-page photoplates of models and two of stereograms, which are of great value to the reader in aiding him to visualize the region treated. Their value in this respect was appreciated by able scholars in France, as Gilbert had the pleasure of knowing when he visited Paris in 1888. One of the models, reproduced on a scale of about 9 miles to an inch, represents an area of 76 by 80 miles, extending from the high plateaus on the west to the Colorado on the east, with the long Water-pocket flexure through the middle and the Henry Mountains between it and the river; this is based on "Topography by W. H. Graves," but as already noted Gilbert himself did much topographic field work in the way of plane-table surveys and barometer readings. The maker of this model is not named, but it is believed that Gilbert had much to do with it. The other model, reproduced on a scale of a little more than 4 miles to an inch, represents the Henry Mountains alone, and was made "by G. K. Gilbert"; this is repeated in another page plate with geological colors added. The stereograms, which appear to have been built up from the models, represent the deformed surface of a single geological stratum for the same two areas. Gilbert undoubtedly had help in the laborious construction of the originals, but even so, it is difficult to understand how they could have been designed and completed in the same four months that were occupied with the writing of the report.

The rapidity with which this famous report was prepared and the keen analysis of the problems that it treated point to a fundamental change of conditions in Gilbert's scientific life, caused by his transfer from the Wheeler to the Powell survey. He rose immediately to the occasion; and it is characteristic of his conscientious nature that the satisfaction he felt in the great opportunity opened to him by Powell took the form of gratefully recognizing its advantages and loyally accepting full responsibility for making the most of them. The preface to his report reads:

> If these pages fail to give a correct account of the structure of the Henry Mountains the fault is mine and I have no excuse. In all the earlier exploration of the Rocky Mountain Region, as well as in much of the more recent surveys, the geologist has merely accompanied the geographer and has had no voice in the determination of either the route or the rate of travel. When the structure of a mountain was in doubt he was rarely able to visit the points which should resolve the doubt, but was compelled to turn regretfully away. Not so in the survey of the Henry Mountains. Geological exploration had shown that they were well disposed for examination, and that they promised to give the key to a type of structure which was at best obscurely known; and I was sent by Professor Powell to make a study of them, without restriction as to my order or method. I was limited only in time, the snow stopping my work two months after it was begun. Two months would be far too short a period in which to survey a thousand square miles in Pennsylvania or Illinois, but among the Colorado Plateaus it proved sufficient. A few comprehensive views from mountain tops gave the general distribution of the formations, and the remainder of the time was spent in the examination of the localities which best displayed the peculiar features of the structure. So thorough was the display and so satisfactory the examination, that in preparing my report I have felt less than ever before the desire to revisit the field and prove my conclusions by more extended observation.

Rapidly as the report was prepared, it still ranks to-day as a masterpiece of logical and geological analysis. Geologists of the younger generation who have encountered laccoliths chiefly on the pages of a textbook along with faults and unconformities, as parts of the standard material of their science, will do well to make acquaintance with the original monograph in which the existence of these peculiar igneous structures was first demonstrated a little over 40 years ago. Geologists of an older generation will also profit by turning again to the report which they must have first read with immature eyes, for a rereading of it will probably discover many ideas which their memory has not retained, and possibly not a few lessons which they did not clearly apprehend before. But quite apart from the content of the report, its method and its manner deserve attentive examination by anyone who is searching for a good example of scientific investigation, for it is a model of penetrating interpretation and candid exposition; and it may be particularly recommended to students of philosophy who, already familiar with the principles of logic in the abstract, wish to study a worthy example of their application to a concrete scientific problem. If such students are partisans of the absolute school, they will of course condemn Gilbert's adoption of one set of speculations as the basis for another set, and they may even be amused by the credulity of geologists in general who so willingly accept even the later set of speculations as "conclusions"; but if they are of the pragmatic school, they will rejoice over the ingenuity with which Gilbert succeeded in revealing so many conditions and processes of the past on the basis of so short a study of the present.

OBSERVED AND INFERRED STRUCTURES

Gilbert learned the general structure of the Henry Mountains by "a few comprehensive views from the mountain tops," the highest of which rise some 5,000 feet above the surrounding surface, thus attaining altitudes of over 11,000 feet. This method of geological investigation may seem hasty if not superficial to an observer bred in a plant-covered region, but it will be accepted as satisfactory and convincing by an observer familiar with the frank confession made by the rocks of "a naked desert, soilless and almost plantless," as to their succession and attitude. The mountain group as a whole occupies a space measuring about 30 miles north-south by 10 miles east-west; and each of its 36 members represent a laccolith [1] or cisternlike mass of igneous rock, still more or less covered by the lower beds of the heavy series of strata beneath which it was originally intruded. The stratified rocks of the district are nearly horizontal, except where blistered up by the intrusions. Their total thickness from the upper Carboniferous into the mid-Cretaceous is about 7,000 feet; several thousand feet of Tertiary strata, still seen in the high plateaus on the west, are here wanting by reason of widespread erosion. The laccoliths consist of "trachyte" according to the terminology adopted by Gilbert; a rock that might now be called a "holocrystalline quartz monzonite porphyry." They occur chiefly at two levels or zones in a lower and a higher series of shales, each series between being 1,000 feet or more in thickness, and the two being some 3,000 feet apart vertically. The mean diameter of 8 well-defined laccoliths in the lower zone is 2.6 miles; and of 10 in the upper zone, 1.2 miles; their thickness is usually from a sixth to a quarter of their diameter; but one of them, Howell, has a diameter of 2,000 feet and a thickness of only 50 feet. The volume of the largest, Hillers, is estimated at 10 cubic miles. The arrangement of the different laccoliths is without discernible system, and they would "prove intractable in the hands of those geologists who draw parallel lines through groups of volcanic vents by way of showing their trend."

At about the time of Gilbert's first work on these singular mountains, certain geologists, members of the Hayden survey, were studying other examples of a somewhat similar structure in the Rocky Mountains of Colorado; but they will not be especially referred to here, as the object of this memoir is biographical rather than geological. Even in the Henry Mountains, the outward or quaquaversal dips of the strata by which the laccoliths are now more or less enveloped had been recognized by two earlier explorers of that secluded region; one was Steward, a geologist of Powell's boat party down the Colorado in 1871; the other was Thompson, Powell's topographer, in 1872. But Gilbert was the first to detect the existence of hori-

[1] This form of the word, suggested by Dana, has generally replaced Gilbert's original spelling, laccolite, and is here adopted except in quotations

zontal strata beneath the heavy igneous masses, whereby to his astonishment their blisterlike structures were demonstrated. On this point he wrote:

If the structure of the mountains be as novel to the reader as it was to the writer, and if it be as strongly opposed to his preconception of the manner in which igneous mountains are constituted, he may well question the conclusions in regard to it while they are unsustained by proof. I can only beg him to suspend his judgment until the whole case shall have been presented (p. 18).

This presentation he then proceeds to make, not by narrating his personal experiences in the field, a plan which serves well enough in popular exposition, but, as he was addressing professional geologists, by at once setting forth the general conclusion that he finally reached as to the structure of laccoliths, and then marshaling the facts in systematic order as a means of showing how fully his general conclusion would take account of them, great as their variety appeared, and how necessary the adoption of the conclusion therefore was; or in his own words:

The preliminary explanation of the type structure furnishes a complement of categories and terms by the aid of which the description of the details of observation, essentially tedious, is greatly abbreviated.

The facts were marshaled in a very convincing procession: First, the up-domed strata, not yet sufficiently worn away to reveal the inferred cistern of igneous rock beneath; then a series of more and more unroofed igneous masses; and finally those masses, which are not only completely stripped of their former cover, but are so far undermined around their borders as to give clear view of the undisturbed strata underlying them.

The erosion of the mountains has given the utmost variety of exposure to the laccolites. In one place are seen only arching strata; in another, arching strata crossed by a few dikes; in another, arching strata filled by a net-work of dikes and sheets. Elsewhere a portion of the laccolite itself is bared, or one side is removed so as to exhibit a natural section. Here the sedimentary cover has all been removed, and the laccolite stands free, with its original form; there the hard trachyte itself has been attacked by the elements and its form is changed. Somewhere, perhaps, the laccolite has been destroyed and only a dike remains to mark the fissure through which it was injected (21).

It is thus evident that, although the general structure of the mountains had been quickly discovered by a few comprehensive views from mountain tops, their detailed structure had been learned by patient and close-range observation.

Following the general statement, nine examples of domed strata are described in detail as more or less eroded, but not sufficiently so to reveal any igneous rocks; five, of domed strata intruded by dikes and sheets; eight, of partly revealed igneous cores; five, of cores well revealed on one side at least and there showing undisturbed strata beneath; two, of fully exposed laccolithic cores; and seven of partly demolished laccoliths; the last nine, like the preceding five, presumably exhibiting the underlying horizontal strata, although this significant item is not specified in the summarized statement. No example, however, was found in which the demolition of the cistern mass was so far advanced as to reveal the expected feeding dike beneath it. It is then held that the type structure "accords with all the facts that have been observed and unites them into a consistent whole" (54). The existence of laccoliths was thus established, and at the same time the mountains that they now form were shown to result from intrusion and doming or up-arching of various dimensions, followed by erosion of various degrees.

There can, therefore, be no question that Gilbert had abundant evidence for his conclusions as to the structure of laccoliths; and, structurally considered, they might be defined as masses of instrusive igneous rock, supplied through unseen chimneys beneath, roughly circular in plan, and having a domelike form, with gently curved top, steep-sloping sides, and flat floors, between undisturbed strata below and up-arched strata above. Even Mount Ellsworth, a large domed structure, one of the southernmost and least denuded of the group, in which overhead dikes are seen but no central laccolith is revealed, illustrates the immensity of the erosion involved in the production of the existing forms; for it is believed to have been originally covered with domed strata more than a mile in thickness. Regarding this fine example, Gilbert reports with characteristic frankness that although the laccolith itself is not exposed to view, he is convinced that it exists;

that the visible arching strata envelop it, that the visible forest of dikes join it, and that the visible faulted blocks of the upper mountain achieved their displacement while floated by the still liquid lava. The proof, how-

ever, is not in the mountain itself, but depends on the association of the phenomena of curvature and dike and sheet with laccolites, in other mountains of the same group (27).

The importance of progressive erosion in developing the mountain is alluded to again on a later page, where after explaining that the laccoliths of the higher zone are already well exposed in strong relief by the degradation of the surrounding strata, while those of the lower zone are not yet so prominent as they will be later, Gilbert gracefully added:

In attaching to the least of the peaks the name of my friend Mr. Holmes, I am confident that I commemorate his attainment by a monument which will be more conspicuous to future generations and races than it is to the present (150).

RECOGNITION OF LACCOLITHS

It is interesting to learn from the field notebooks of Gilbert's first season in the Henry Mountains that their true interpretation was very early forced upon his attention. He approached their southern members from the west, and on August 18, 1875, was on the great Water-pocket flexure near the point where Hoxie Creek, about 12 miles north of its junction with the Colorado, departs from the weaker strata of the flexure and cuts a singular horseshoe canyon in the harder beds of the western rise. He then noted, concerning the southernmost of the mountains, distant nearly 20 miles: "Ellsworth shows no volcanic colors but looks as though built of the valley rocks. In the region *a* [the summit part of an adjoined sketch] I can make out no dip but in the regions *b* & *c* [the southern and northern flanks] I measure dips of about 25°." The next day while still on the same great flexure he wrote: "I see Ellsworth better. On *this* [western] flank the dip is this way unmistakably. I can see the successive outcrops circling around it—red at base, then white—& the white probably caps the summit." Thus the doming of the strata over the mountain top was detected.

Gilbert and three of his party "slept out" that night, as explained the morning after: "Darkness overtook us & we barely made a water pocket in the descent when we were forced by the uncertainty of the way and by weariness to stop. At 4:15 this morning we started again & reached breakfast at 6:30." It was then briefly noted regarding two mountains north of Ellsworth: "Pennell and Hillers still look very volcanic"; this probably meant that their igneous rocks were better revealed than those of Ellsworth, not that they were of eruptive origin. Later in the day the basal features of Ellsworth were seen to be repeated in Hillers: "The hogbacks seem to trend in a curve around the mountain flank as far as they extend"; and the following day it was added: "The bending or swinging of the hogbacks about the mountain base is unmistakable." A part of the mountain next north of Ellsworth, called "Henry V" in the notes and later named Mount Holmes, was in view the next day, August 22: "It was just a tumor, cracked in the middle," the cracks being filled with dikes; and to this was added: "I am impressed with the idea that the dikes are radial, diminishing outwards." The next day the same mountain was described as of "bubble form . . . the strata being nearly level on top & the crests controlled by dikes, which are radial." Later on the same day the same mass was compared with others: "It is a low-angled bubble. Hillers is high-angled & Ellsworth strikes a mean." A significant generalization had already been recorded the day before regarding Hillers: "Only the Trias was lifted & the Carboniferous either lay below the seat of action or below a distributing reservoir"; thus the idea of a reservoir or cistern of igneous rocks on an undisturbed foundation is first announced. The idea was at that time presumably based more on the manifest arching of the Triassic strata, which were lifted but not intruded by the igneous mass, than on any observed occurrence of Carboniferous beds beneath the mountain here examined. Indeed, the horizontal underlying strata were imperfectly noted during the brief visit to the mountains in the summer of 1875, yet a small figure, here copied, is added in which the undisturbed underlying strata are clearly represented. Not until the following summer were the trachyte reservoirs given a special name, "Lacune"; and this was modified in the published report to "Laccolite," in which the root has the Greek rather than the Latin form.

The mental processes of description, comparison, and generalization can find no better illustration than is offered in the first 50 pages of the Henry Mountains report; but it is well to recognize at the same time that these 50 pages also afford an admirable example of the

essentially speculative nature of geological science; for various matters which are set forth as "the principle facts in regard to the laccolites" are in reality speculative inferences that far transcend the reach of observation; indeed, they are nothing more than the thriving outgrowths of certain venturesome speculations of a century or more ago, which have been since then accredited and accepted as the veritable counterparts of invisible, unobservable facts in the vanished past. The production of the laccolithic rock masses by cooling from a state of fusion; their ascent, while fused, from a deep source by unseen conduits; the upheaval of the covering strata as a result of the ascent; and the later removal of more or less of the covering strata by erosion—all these and various similar matters are evidently not visible facts of observation but only accepted concepts of speculation, yet so successfully fortified that they are properly taken, along with visible facts, as realities to be explained by other speculations.

Only one of these speculative conclusions was regarded as in need of special inquiry for its justification; namely, that the laccolithic masses are really subterranean intrusions which blistered up the strata above them, and not ancient volcanic masses erupted upon the earlier-deposited, underlying strata and buried under the later-deposited overlying strata. To this inquiry, "the answer is not difficult." The laccoliths are subterranean intrusions; for this conclusion, which after it is reached is treated with entire geological propriety as a "fact," is supported to the point of demonstration by the standard scientific method of first deducing

Fig. 10—The Western face of the Morvine lacolith

contrasted groups of consequences from the two suggested possibilities of intrusion and extrusion, and then impartially confronting the deduced consequences with appropriate items of observation; a method of proof that is truly simple enough; so simple that some readers may think it hardly worth mentioning here; yet it is doubtful whether the question at issue—the explanation of buried igneous masses as subsequent intrusions or contemporaneous extrusions—had previously been treated in this way as fully by anyone else. Indeed, the failure of some of Gilbert's predecessors to apply this simple method in the investigation of buried igneous masses elsewhere delayed them in the attainment of safe interpretations.

THE BASE OF THE LACCOLITHS

A metaphysically minded reader may object to the use above of such words as demonstration and proof in connection with a method of inquiry that leads, as geologists very well know, to nothing more than a high order of probability, and even to that only on the pragmatic assumption that the present order of nature has endured all through the geological past; but with this sort of objection to the interpretation of laccoliths and other geological phenomena we are not here concerned. More pertinent are the doubts that were expressed by certain distant scientists 30 or 40 years ago as to the geological validity of Gilbert's conclusions. For example, Reyer, of Vienna, apparently more guided by his own prepossessions than by Gilbert's evidence, contended that the Henry Mountains laccoliths must really be buried volcanoes and not subterranean intrusions.[2] Neumayr, deservedly regarded as one of the leading geologists of his time, inclined to the same view, but in deference to Gilbert's explicit statement of the reasons

[2] Theoretische Geologie, 1888, 135, 136.

for assigning the Henry Mountains masses an intrusive instead of an eruptive origin, constrained himself, though still incredulous and unconvinced, to announce that interpretation with the reservation that Gilbert's observations needed verification.[3]

More critically pertinent were the doubts expressed by the English geologist, Green, in Nature; for this reviewer, apparently familiar with large igneous intrusions that had broken and upheaved the strata through which they had risen and that had been explained as huge columnar, or blunt conelike masses extending with full or increasing diameter indefinitely downward, was skeptical as to the sufficiency of the reasons offered for the existence of a limiting undersurface at the base of the laccoliths. He wrote, "in no captious spirit," yet in some incredulity, questioning the "evidence by which the existence of these peculiarly shaped bodies of intrusive rock is supported. Mr. Gilbert has evidently seen enough to satisfy himself on this point, and we are quite willing to put every confidence in the statements of so accurate and skilful an observer; at the same time we cannot help feeling some regret that he has not been a little more explicit in his description of the sections which lay open the characteristic form of the laccolite. The horizontal base and the undisturbed state of the underlying strata are the first points on which we wish to be thoroughly assured. . . . The views of the Marvine laccolite in Figs. 43 and 44, if we understand them aright, do not seem to be conclusive on the point of the horizontal base; but the evidence would have been more convincing if these plates had been explained at greater length in the text." [4]

It is true that Gilbert did not take pains to distinguish the igneous masses of the Henry Mountains from deep columnar intrusions, although, as is shown above, he distinguished them carefully enough from buried volcanoes. The evident reason for disregarding columnar intrusions is that, while his published statements concerning the occurrence of horizontal strata underlying the laccoliths are as a rule not so emphatic or so conspicuous as to arrest the attention of a hurried reader, his field studies had so clearly discovered the presence of such strata, that the possibility of the downward extension of the igneous masses, except in inferred feeding dikes, was for him completely excluded. The field notebooks for the season of 1876 leave no doubt upon this point. Some 25 pages of notes witten on the rainy October 12 were filled with a structural summary which included the following explicit statements:

In the NE Butte [Jukes ?] none of the curved [overlying] strata are visible, the whole summit is trap. But the level beds below are visible on nearly every side. . . . In 249 [Marvine?] every element of the type, except the supply dike or chimney, is clearly visible. At one end the upper strata complete the arch; at the other they have been completely removed and the trap can be seen resting on level strata.

Three other examples of visible underlying beds are also instanced. A week earlier the general relation of the intrusions had been clearly generalized in the case of two mountains of complex structure:

The general structure of Ellen and Pennell [in the northern and central parts of the group] is a system of bulge intrusions of trap compiled as irregularly as the secondary cones of a volcano. Successive jets of trap finding passage at diverse points ceased their upward movement at points equally diverse & spread in lenticular form, lifting the superior strata.

And here the occurrence of undisturbed underlying strata is only implied instead of being explicitly stated.

Explicit statements on this point in the published report are brief, as follows: As to the Steward laccolith, east of Hillers, "at one end it is bared quite to the base, and the sandstone floor on which it rests is brought to light" (33). Beneath the neighboring Howell laccolith, "the underlying strata, locally hardened to sandstone, lie level; the overlying curve downward to join them" (34, 35). The Sentinel Butte laccolith, north of Pennell, is "sapped by the yielding of its soft foundation" (38). Laccolith E "rests upon the Tununk sandstone" (41). The Shoulder laccolith overlies the Geikie, and a bed of "conglomerate runs under the one and over the other and separates them" (42). Under the Jukes laccolith, the northeasternmost of the group, "are five hundred feet of softer rock which constitutes its pedestal" (46). These state-

[3] Erdgeschichte, 1887, i, 180.
[4] A. H. G[reen]. Nature, xxi, 1879, 177-179.

ments when thus brought together are clear enough, even though they do not always mention the attitude of the underlying strata; but in the original text they are scattered and almost lost among other matters. Moreover, in the published account of two other well-exposed laccoliths the underlying strata are not mentioned: The Marvine, the northernmost of all, "stands forth on a pedestal, devoid of talus, naked and alone" (42); and as to the Scrope, northeast of Pennell, "the erosion of its matrix has left it a conspicuous crag" (47). Even in the generalized structural summary above cited, the underlying strata are not given prominence; for besides the 20 instances in which erosion is not yet deep enough to reveal the base of the laccolith, and the five instances in which one side of a laccolith has been eroded, "exposing the core of trachyte to its base and showing undisturbed strata beneath it," there are seven other instances which are next noted as having suffered a still greater erosion, but in connection with which nothing is said of the underlying strata. Silence here can not mean that the fundamental strata were not seen, but that they were so plainly seen as to have been taken as a matter of course.

There can therefore be no question that Gilbert determined the general facts of laccolithic structure correctly. There may, however, be question whether certain igneous masses which have been in later years explained as laccoliths, after the laccolith idea had been generally accepted, are truly of that nature. For example, Hauthal describes two bold mountains, Payne and Fitzroy, in the southern Andes as laccoliths, and explicitly states that they have a partly eroded cover of Cretaceous strata; but he does not give explicit account of any underlying undisturbed strata.[5] It is quite possible that the conception of laccoliths has become so popular that masses of other nature have been called laccoliths without sufficient assurance that they have a level base as well as a domed top.

[5] Mitteilungen über den heutigen Stand der geologischen Erforschung Argentiniens. 9th Internat. Geol. Congress, Vienna, (1904), 1905, 649–656.

CHAPTER IX

THE CONDITIONS AND PROCESSES OF LACCOLITHIC INTRUSION

THE HENRY MOUNTAINS AS TYPICAL LACCOLITHS

It is as true to-day as it was when Gilbert wrote in 1877 that, while larger bodies of intrusive rocks occur elsewhere, no other examples so typical of simple laccolithic structure as the Henry Mountains have been described. Had his study not been carried further than the account of observed and inferred structures presented in the first 50 pages of the published report, summarized above, it would have been a valuable contribution to American geology; indeed to the geology of the world. But instead of stopping with existing structures, he undertook in the second 50 pages a penetrating discussion of the conditions under which the laccolithic intrusions of the Henry Mountains had taken place. His treatment of this problem is delightfully ingenious and open; the reader feels as if the author were inviting his company on a speculative excursion, in which all pertinent facts and suppositions are to be candidly examined. The method of presentation is the very reverse of that obscurantist style, supposedly appropriate for popular scientific story-telling, in which the writer, as if assuming that the public can not understand geological reasoning, announces conclusions without giving grounds for them; or, still worse, puts the cart of conclusion before the horse of inference and even presents the cart as the pulling force, witness such a statement as:

> Geologists have discovered that the climate of the earth was, in a past stage of its history, much warmer and moister than now; therefore plants then grew luxuriantly and coal beds were formed of their remains.

Gilbert's method was precisely the opposite of this, in that he always presented facts and inferences in proper order, left no points in obscurity, never glossed over a difficulty or stretched an argument, and never attempted to impose his views as if by authority. Indeed for purity and candor of reasoning, the chapter on the conditions of laccolithic intrusion as exhibited in the Henry Mountains has few parallels in geological literature. It is not to be expected that the chapter was complete or that its conclusions were so final that they should apply to laccoliths in regions of disordered structure; but the discussion unquestionably deserves high praise for its ingenuity in utilizing all pertinent and available knowledge. It will be here presented in outline.

The discussion opens with a review of the results reached concerning the structural features upon which the hypothesis that the Henry Mountain laccoliths are intrusions is based, and then adopts the hypothesis because it "accords with all the facts that have been observed and unites them into a consistent whole" (54). Next, the intrusive rocks of the laccoliths are compared with the effusive volcanic lavas of the plateau province; the first are without exception acidic, while the others are basic, Dutton being here called in as a petrographic expert. It is not possible to combine the two phenomena of intrusion and extrusion as the result of the rise of one magma—Gilbert of course used the old term, lava—for "the acidic type if extruded at the surface would be an ordinary [not a porphyritic] trachyte; the basic type if crystallized under pressure would be classed with the greenstones. The basis for the generalization is exceedingly broad. . . . In the Uinkaret Mountains [on the plateau north of the western part of the Grand canyon] Professor Powell has distinguished no less than one hundred and eighteen eruptive cones [all of them resting on horizontal strata which give no indication of being arched by underground laccoliths], and in the Henry Mountains I have enumerated thirty-six individual laccolites. In one locality basic lava has one hundred and eighteen times risen to the surface by channels more or less distinct, instead of opening chambers for itself below. In the other locality porphyritic trachyte has thirty-six times built laccolites instead of rising to the surface" (71).

THE HYDROSTATIC LAW AND ROCK COHESION

Now disregarding the source of the magma and its propelling force, the question arises: "Why is it that in some cases igneous rocks form volcanoes and in other cases laccolites?" Here successive suppositions are made with a view to simplifying the problem. It is first supposed that the solid rocks of the crust have no cohesion to impede either the vertical rise or the lateral spread of the fluid rocks. "The lava will then obey strictly the general law of hydrostatics, and assume the station which will give the lowest possible position to the center of gravity of the strata and the lava combined" (72). This conclusion appears to have been reached in the field, for a notebook record of September 20, 1876, concerning two contiguous intrusive sheets near Mount Pennell implies hydrostatic action in the brief statement: "I call the upper the newer because it was lighter in weight molten than the other cold." A part of the long summary written three weeks later on the rainy 12th of October gives a more general treatment of the same principle. It is evident that careful theoretical reflection recorded in view of the facts must have greatly expedited the preparation of the report in Washington.

Inquiry is next made into the validity of the hydrostatic law when cohesion is considered. Gilbert recognizes that the strength of the crustal strata must of course modify the behavior of the rising magmas, and then frankly confesses, in a way that immediately begets confidence in his fairness: "I am at a loss to tell in what way it influences the selection by a lava flood of a subaerial or a subterranean bourne" (73). However, he concludes that the hydrostatic law is not wholly abrogated; it is only modified by cohesion: "Light lavas will still *tend* to rise higher than heavy, however much the rising of all lavas may be hindered or favored" (74). The problem therefore really turns on whether the relative density of lavas and crust or the penetrability of the crust is the determining factor. "When resistance to penetration is the same in all directions, the relation of densities determines the stopping place of the rising lavas; but when the vertical and lateral resistances are unequal, *their* relation may be the determining condition." Hence, if penetrability were the controlling factor during an epoch of igneous activity, all kinds of magmas would rise to the surface where the crust is vertically penetrable and would be found together in volcanoes; while where the crust is not thus penetrable but is liftable, all kinds of magmas would fail to reach the surface and would be found associated in laccoliths. But, on the other hand, if density be in control, then certain lighter magmas would usually form volcanic masses and other heavier magmas would form laccoliths.

In the actual case of the Henry Mountains the difficulty of penetrating and the ease of lifting the strata may have guided the intrusions into two shale horizons, as above noted, but these factors may not alone have been in control. When the constitution of the intrusive and effusive rocks of the plateau province is examined, "we find the entire weight of the evidence in favor of the assumption that conditions of density determine the structure. The coincidence of the laccolitic structure with a certain type of igneous rock is so persistent that we can not doubt that the rock contained in itself the condition which determined its behavior. We are then led to conclude that . . . the fulfillment of the general law of hydrostatics was not materially modified by the rigidity and cohesion of the strata" (75).

A test for this conclusion is next sought for in the densities of the rocks concerned, and thereupon an apparent contradiction is met; for in surface volcanoes, where the density of the rocks should be low, it is about 3; while in the subsurface laccoliths, where the density should be high, it is only from 2.6 to 2.9. Here, however, Gilbert added: "But in order that the laccolitic structure should have been determined by density, the acidic rock of the laccolites must have been heavier in its *molten* condition than the more basic rocks of the neighboring volcanoes" (75). Search was therefore made for information as to change of density when a rock passes from the molten to the solid condition, and citation was made of the results of experiments by Bischof, Delesse, and Mallet—much to the dissatisfaction of the last named;[1] but the data thus gained were not considered pertinent, because all the experiments involve dry fusion, while "it is generally conceded that the fusion of lavas is hydrothermal." Sub-

[1] Nature, xxii, 1880, 265.

sequent studies have confirmed this latter opinion, and given some support to the view that even if the solidified basic rocks of surface volcanoes are denser than the solidified acidic rocks of laccoliths, the molten basic rocks may, before they lost their included gases and vapors during eruption, have been less dense than the molten acidic rocks in their subterranean cisterns. Hence the relation of densities now obtaining may be due in part at least to the loss of a good share of initial gases in the surface rocks. In other words, the contraction and density increase of the surface basic rocks in cooling at the time of their eruption may have been greater than that of the buried acidic rocks; but it is generally felt that Gilbert carried this principle too far.

However, he cited "a fact of observation which tends to sustain the view that the laccolitic rocks of the Henry Mountains contracted less in cooling than the volcanic." Prismatic structure is produced by contraction in cooling; and as it is absent in the Henry Mountain trachytes, their contraction must have been small; conversely, its presence in many basic volcanic rocks indicates that their contraction was relatively great. But if the hydrostatic law holds good, the acidic rocks of the subterranean laccoliths may have contracted by a considerable amount; for if those rocks really were, when molten, only slightly denser than the strata above them (2.3), then their volume when molten must have been about one-tenth greater than when solid; and to this it may be added that, under the same hypothesis, the increase in density of surface volcanic rocks on passing from the molten to the solid state must have been greater still. Thus it is possible that if Gilbert's hydrostatic theory of laccolithic intrusion proves to be measureably true, it may, by means of a comparison of the densities of stratified rocks, laccoliths, and volcanoes, lead to a better determination of the density of both intrusive and extrusive igneous rocks when they were in a molten state and of their increase of density on solidifying, than is obtainable by experiment; for artificial conditions in the laboratory can hardly imitate natural conditions in a volcanic vent, much less those in an underground laccolithic reservoir. But it must be added at once that laccoliths later found in other parts of the Cordilleran region have varied compositions and are thought by their investigators to discredit Gilbert's view that density of magma was an important factor in the formation of laccoliths. Yet none of these investigators has published so critical an analysis of the factors of penetrability and density as Gilbert did.

MECHANICS OF LACCOLITHIC INTRUSION

The physical conditions of intrusion being thus outlined, the mechanical changes that accompanied the intrusions are examined. It should be here recalled that the average form of the Henry Mountains laccoliths is that of a low arched dome with a rapid marginal descent; and that the covering strata therefore rise in a steep-sided, flat-topped quaquaversal arch; also that, although the flat tops and the flexed sides of the arched beds are broken by radial fissures occupied by dikes, the flexed sides are not fractured on circumferential lines; hence the central covering strata must have been increased in radial measure by stretching during deformation. The stretching is explained as due to an extension or outward squeezing of the immediately overlying strata between the upward pressure of the intruding trachyte and the downward weight of the higher strata. An interesting corollary follows: Any fractures that were made in the arch must have been instantly filled with intrusive dikes, and no empty fissures were left to be slowly filled afterwards with mineral veins; and it is for this reason that the Henry Mountains have no attraction to the miner (83).

The cause of the steep dips around the margin of the flat-topped laccolithic domes is next sought for; and it is particularly here that Gilbert's analysis of the problem has seldom been quoted, perhaps seldom understood, so fully as it deserves. It is probable that some of the masses are more flat-topped than others; if so, it is the typical flat-topped domes that are especially considered. It was conceived that when lateral movement of the rising magma began, a comparatively thin horizontal sheet was formed, which would be nearly circular if the ease of lifting the strata were about the same in all directions outward from the supplying chimney or neck. Consideration was then given to the distribution of pressure exerted by the intruding magma, the thickness of the overlying strata being constant. The total upward

pressure exerted by a circular sheet will increase with its area, or with the square of its radius. Part of the upward pressure may be given to lifting the weight of the cover, and in a cover of constant thickness the weight will increase with the area of the sheet or the square of its radius, and hence with the upward pressure; the remainder of the upward pressure may be given to deforming the cover. Deformation by the formation of a vertical cylindrical fracture around the margin of the sheet is first considered, and allowance is afterwards made for deformation by flexure. The resistance to the making of a cylindrical fracture of the same diameter as the intrusive sheet will increase with the area of the fracture surface; that is, with the circumferential area of the cylinder; and this, in a cover of constant thickness, would increase only with the first power of the cylinder or the sheet radius. Hence, while the resistance to cylindrical fracture will probably be greater than the available upward pressure of a small circular sheet in the early stage of horizontal intrusion, equality of the two forces will be reached later as the sheet increases in radius; and thereafter the direction of least resistance will be upward, and the reservoir will increase in vertical thickness instead of in horizontal diameter.

An important generalization follows: "The laccolite in its formation is constantly solving the problem of 'least force,' and its form is the result" (91). "That is to say, at a given depth beneath the surface a laccolite of a certain circumference will be able to force upward the superjacent cylinder of rock, while a laccolite of a certain smaller circumference will be unable to lift its cover. Or, in other words, there is a limit in size beneath which a laccolite cannot be formed" (88). Therefore "when a lava forced upward through the strata reaches the level at which under the law of hydrostatic equilibrium it must stop, we may conceive that it expands along some plane of bedding in a thin sheet, until its horizontal extent becomes so great that it overcomes the resistance offered by the rigidity of its cover, and it begins to uplift it. The direction of least resistance is now upward, and the reservoir of lava increases in depth [thickness] instead of in width. The area of a laccolite thus tends to remain at its minimum limit" (88, 89). But as the thickness of the laccolith increases, its growing weight "is progressively subtracted from the pressure [exerted by the rising magma] against its top, and this proceeds until the upward and lateral pressures become proportional to the resistances which severally oppose them. Further expansion is then both upward and outward" until, "when the sum of the weights of the cover and laccolite equals the total pressure of the intrusive lava, uplift ceases, and the maximum depth or thickness is attained" (90, 91).

RELATION OF DIAMETER AND DEPTH

A further consequence of this theoretical discussion is that the diameter of a Henry Mountains laccolite "is proportioned to its depth beneath the surface"; and this is susceptible of test by comparing the diameters of laccolites in the lower and upper zones of the Henry Mountains strata. The test gives remarkable confirmation to the theory; for not only is the mean diameter of 8 laccoliths in the lower zone (2.6 miles) double the mean of 10 in the upper zone (1.2 miles), but in the upper zone, where their stratigraphic position is best determined, the highest laccolites are the smallest, and their size increases downward with some approach to regularity. "There is no laccolith of the upper zone so large as the smallest in the lower zone; and the mean diameter of those on the lower zone is double the mean of those on the upper. . . . The confirmation of the conclusion is as nearly perfect as could have been anticipated. There is no room to doubt that a relation exists between the diameters of laccolites and the depth of their intrusion" (92, 93).

A most ingenious extension of the theory is finally made in estimating the original thickness of the covering strata:

> Having determined by observation the mean size of the laccolites in the upper and lower zones, as well as the interval [3,300 feet] which separates the two zones, and knowing approximately the law which binds the size of the laccolite to its depth of intrusion, we can compute the depth of intrusion of each zone. Our result will doubtless have a large probable error, but it will not be entirely without value (94).

The result gained varies from 10,300 to 14,500 feet, according to the relation that is assumed to exist between the thickness of the cover and its resistance to flexure; and as the present cover of the upper zone averages only 3,500 feet in the thickness, the original cover, presumably Tertiary for the most part, must have lost from 7,000 to 11,000 feet of its thickness by post-intrusional erosion. As the lost strata are still present in the high plateaus to the west, and as independent geological evidence suggests their original extension over the Henry Mountains area, the calculated thickness of the original laccolith cover seems reasonable.

Yet in spite of the plausibility of his argumentation, Gilbert did not insist upon the acceptance of the results that it reached. He wrote:

> I am far from attaching great weight to this speculation in regard to the original depths of the laccolite covers. It is always hazardous to attempt the quantitative discussion of geological problems, for the reason that the conditions are apt to be both complex and imperfectly known; and in this case an uncertainty attaches to the law of relation, as well as to the quantities to which it is applied. Nevertheless after making every allowance there remains a presumption that the cover of the laccolites included some thousands of feet of Tertiary sediments (94).

This is putting it mildly, to say the least. It would not be overstating the case to say that a Henry Mountains laccolith is not, as usually understood, simply an irresponsible mass of intrusive rock, but in view of Gilbert's well-conducted analysis it is a comparatively orderly and reasonable structure, the general position and dimensions of which appear to be explicable on mechanical principles.

GENETIC DEFINITION OF A LACCOLITH

The largely inductive or empirical definition of a laccolith given at the end of an earlier section may now be replaced by an expanded genetic definition: A Henry Mountains laccolith is a mass of intrusive igneous rock which, supplied through an inferred chimney from a source of unknown depth, begins its expansion by spreading laterally in a horizontal fissure-plane, apparently in accordance with the hydrostatic law, at a level where, while still molten, its density is less than the average density of the underlying and greater than that of the overlying strata. In spreading, the molten magma first assumes the form of a roughly circular sheet or disk of small thickness, but as the radius of the disk increases it attains a dimension at which the intrusive force of the magma is more economically applied to the thickening of its mass in domelike form by abruptly flexing up the strata around its margin and lifting up the strata above it, than to spreading farther and lifting up a larger total area of overlying strata without so much flexing. As marginal flexing takes place, the flexed strata are stretched by being squeezed between the upward pressure of the intruding magma and the downward weight of the higher strata.

The limiting diameter at which the abrupt up-flexing of the marginal strata takes place increases with the depth at which the magma outspreads; the lower-lying laccoliths of the Henry Mountains have an average diameter of 2.6 miles under an estimated original cover nearly 3 miles thick; and the same figures for the higher-lying laccoliths are 1.2 and somewhat more than 2 miles. If the upward pressure of the magma is maintained, the laccolithic dome will gain in thickness by continuous or by successive intrusions until the total weight of the laccolith and its cover is greater than the upward pressure of the magma; when this stage is reached, the magma will seek another place of ascent and intrusion. The thickened form of the completed laccolith is not an indication that the magma was imperfectly fused at the time of its intrusion, but that it was constantly "solving the problem of 'least work'"; for its effective fusion is proved by the thinness of certain sheetlike laccoliths which did not thicken, as well as by the narrowness of the many dikes which appear to have been instantly shot into any fissures opened in the overlying beds. The formation of laccoliths, as thus explained, leads to the conclusion that they are not associated with volcanoes, beneath which, according to the hydrostatic law, the rising magma should be in its molten state less dense than the rocks through which it rises, whatever its density may become after its gases have escaped and it has been cooled and solidified.

GILBERT'S THEORY NOT GENERALLY UNDERSTOOD

If the second 50 pages of the Henry Mountains report mean anything, they mean that Gilbert attached about as much importance to the highly theoretical inferences which they contain concerning the conditions and processes of laccolithic intrusion as he did to the simpler inductions presented in the first 50 pages concerning the actual structure of laccoliths. It therefore occasions some surprise to discover that many geologists, while accepting his inductions as to the structure of laccoliths and his inferences as to their subterranean intrusion, have given little heed to his speculations as to the processes of their origin, apart from the elementary item that intrusion actually took place. A further statement of this aspect of the problem is here undertaken, not so much with the purpose of advancing the understanding of a special geological problem, as with the desire to promote a fuller appreciation of Gilbert's thought upon it.

We are not here concerned with the doubts frankly expressed by Green and Neumayr as to the verity of laccolithic intrusion, or with Reyer's erroneous preconception that the igneous masses of the Henry Mountains are extrusions; sufficient references to these authors have already been made. We are still less concerned with the unappreciative comment made by De Lapparent, who after spending several pages in combating the obsolete theory of craters of elevation, briefly discredits Gilbert's novel and valuable interpretation of laccoliths, because it involves intumescence, "pour l'explication de laquelle on propose une hypothèse dépourvue jusqu'ici de toute vérification directe";[2] or with the careless misrepresentation made by Haug, who takes to himself the explanation by intrusion and intimates that Gilbert had advocated an eruptive origin: "On a supposé tout d'abord que le magma fluide s'était précipité dans des creux préexistants, résultant du décollement des strates supérieures, d'où lenom de laccolithes que leur a donné Gilbert. Mais il est bien plus probable que le magma s'était introduit entre deux couches en soulevant la couche supérieure."[3] Misunderstandings of these kinds do not need special comment, for they are not likely to endure; but attention must be given to certain incomplete statements by other geologists who, while accepting the most manifest parts of Gilbert's theory, fail to do justice to its more delicate and ingenious elements.

It is possible that some or all of these finer elements are debatable, for Gilbert himself recognized that his method of analysis included several assumptions and approximations; and 20 years later, when he described a small laccolith in the plains of eastern Colorado, he referred to some of his earlier conclusions as only "tentative"; but they were nevertheless framed with good judgment and they were well grounded. Perhaps some of them are erroneous, although they have not yet been shown to be so; surely, until they are shown to be without value, they ought to be announced as inherent parts of Gilbert's views. There is no novelty in this principle of impartial geological exposition; our more important textbooks traditionally present both sides of other debatable questions; for example, the peculiar views of Semper, Rein, and Murray, although they were observers of no particular geological competence, are still frequently cited in paragraphs on the geological aspects of the coral-reef problem in standard textbooks. Surely Gilbert's views on the laccolithic problem merit at least as fair a consideration, for he was an exceptionally competent geological observer and thinker.

Yet an admirable textbook, which deserves high rank by very reason of giving its readers the opinions of different observers on debated problems—for example, the opinions of the three observers just named in connection with the coral-reef problem—does scanty justice to Gilbert's theory of the origin of laccoliths, concerning which it is merely said: "Large bosses of trachytic lava have risen from beneath, but instead of finding their way to the surface, have spread out laterally and pushed up the overlying strata into a dome-shaped elevation." The student here finds nothing about the hydrostatic law in connection with the level of lateral intrusion, and nothing about the advantage that a large intruded sheet has over a small one in the production of a dome; but he does find in the next sentence a direct contradiction of Gilbert's views that laccoliths and volcanoes do not occur in close association, for the further

[2] Traité de Géologie, 5th Ed. 1906, i, 469.

[3] Traité de Géologie, 1907, i, 276.

statement is made that laccoliths should be "probably not uncommon in denuded volcanic districts." But no reasons are adduced for thus summarily setting aside Gilbert's well-supported induction to the contrary. In another excellent modern work, which usually gives alternative explanations a sufficiently full consideration, laccoliths are treated only in a brief statement under a general account of intrusions: "Sometimes the lava appears to have forced its way into the rocks, and sometimes to have lifted the upper beds and formed great subterranean layers or tumorlike aggregates, called bathyliths and laccoliths." Again, a responsible volume of still later date says nothing of Gilbert's views as to the condition of laccolithic intrusion, but propounds a new explanation for laccolithic domes dependent on the rate of intrusion: "If the supply of material in the formation of an intrusive sheet is more rapid from below than can easily spread laterally, the strata above will be up-arched, as if by a hydrostatic press, and a thick lens of liquid rock will be produced, giving rise on solidification to a laccolith."

If, instead of examining general textbooks of geology, attention is given to special reports and works on igneous structures, fuller statements concerning laccoliths are found, but none of them do justice to Gilbert's explanation of the intrusions in the Henry Mountains. A report on another laccolithic mountain group, in which many features of the Henry Mountains type are repeated, gives very little attention to the mechanical factors involved in the doming of the covering strata, yet concludes that "the controlling factors are shown to be the viscosity of the magma, the rate of injection, and the load of sediments and their plasticity, when in mass, under stresses"; but as Gilbert's analysis of the behavior of an essentially fluid magma is not explicitly discussed and shown to be wrong, it may be seriously questioned whether viscosity and rate of intrusion are really proved to have the leading values here ascribed to them. Again, a monographic volume on igneous rocks announces: "Among the special students of laccoliths the hypothesis prevails that great viscosity is an essential prerequisite in this mechanism"; yet none of these students is shown to have published an analysis of the problem that in completeness at all approaches Gilbert's, nor have they especially considered the evidence that is found, as will be stated below, in the typical Henry Mountains laccoliths for the high effective fluidity of their magma at the time of its intrusion. Still another volume of monographic quality concludes that the magma of laccoliths is driven upward until it reaches a horizon of easy fissuring, where it spreads laterally; thus incoherence of crustal rocks is tacitly postulated to be of greater importance than hydrostatic pressure, which is not considered; yet if this tacit postulate were true, no volcanoes could be built where the land surface, as in the high plateaus of Utah, consists of a heavy series of horizontal strata in which easily fissured members lie at a considerable depth. It is then added that the form which the spreading magma assumes is primarily a function of its viscosity; thus again the evidence for fluidity and the mechanical factors involved in the doming of overlying strata are left out of the discussion.

These special reports therefore give little if any better understanding of Gilbert's views than is to be gained from the terser statements of standard textbooks. It is as if, with the recent discovery of a great variety of structures assumed by intrusive rocks, and still more with the recognition of the great variety of rocks found in intrusive structures, the attention of geologists had been directed chiefly to the existing structural complications of laccolithlike masses and to the manifold problems of their magnetic differentiation, and thus diverted from Gilbert's discussion of the conditions under which the simple and typical laccoliths of the Henry Mountains had been formed; and it is furthermore as if, at the same time, a wave of very imperfectly argued opinion in favor of viscosity as a determining factor in the origin of laccoliths has been permitted to obscure the evidence against viscosity provided by the Henry Mountains as described in Gilbert's eport. Possibly other laccolithic magmas were more viscous than those of his type examples; possibly a decrease of temperature in the rising magma has made it more viscous in the later than in the earlier stages of a laccolithic intrusion; but these possibilities should not be allowed to blur the evidence which the type examples give for fluidity.

In any case it would certainly appear from the foregoing citations that no general agreement has yet been reached as to the origin of laccoliths, and that the conditions of their forma-

tion are therefore still debatable. So long as the debate continues Gilbert's theory, which is much more penetratingly argued than any other yet published, merits fuller recognition than it has received. Three elements of his theory in particular deserve emphasis; the first concerns the value of the hydrostatic law as controlling the level at which a rising magma ceases to continue its ascent and begins to spread laterally; the second concerns the mechanical controls by which the laterally spreading magma is limited in diameter and led to assume a domelike form; the third concerns the influence that the depth of a laccolithic intrusion exerts upon its diameter. As two of these elements of the theory received a certain measure of consideration and yet were rejected in a critical review of Gilbert's report by a leading American geologist, attention may be called to the arguments there adduced.

DANA'S ALTERNATIVE THEORY

Shortly after the publication of Gilbert's Henry Mountains report, Dana reviewed it and proposed an alternative explanation for laccoliths—an alternative that was favorably quoted by another writer on Rocky Mountain laccoliths 14 years later—as follows:

> With so powerful a forced movement in the lavas as the facts, if they are rightly interpreted, show to have existed, no other cause could be needed for a flow to the surface in the case of an open channel, or for a flow to any level in the strata at which a [vertical] fissure might terminate, and this is true whether the lava be light or heavy.[4]

This is unfortunately only a rough and ready method of settling the problem at issue, as compared with the deliberate analysis that characterizes Gilbert's discussion. It leaves the main question unsettled; namely, if an ascending magma has the force to act as it chooses, how will it choose to act? Indeed, Dana's solution is based on the tacit assumption that rock cohesion overcomes the hydrostatic law, and Gilbert's reasons for adopting a contrary opinion are not adequately presented either in the review cited or in the later editions of the reviewer's Manual of Geology. Yet if Gilbert's reasons for his opinion are valid, it would follow that even if a column of somewhat dense laccolithic magma rose in an open fissure almost to the surface of the earth's crust, it would still be easier for the magma to wedge its way laterally between slightly coherent strata at a hydrostatically determined depth than to overflow at the surface.

It is, of course, possible that Gilbert may have underrated the value of rock cohesion; and that a heavy magma ascending through a vertical fissure in a less dense but very solid granite might have to rise all the way to the surface instead of spreading laterally at a hydrostatically determined depth; or, on the other hand, that a very light magma might, instead of rising to the surface, spread laterally into a subterranean domelike cavity that was opened for it in very dense rocks by diastrophic forces which did not at the same time open a higher fissure; but until actual examples are found that appear to exemplify one or the other of these possibilities, it seems reasonable at least to announce the well analyzed reasons which led Gilbert to place a higher value on the hydrostatic law than on rock cohesion in his explanation of the actual laccoliths and volcanoes of the Colorado plateaus.

In the meantime, it should be remembered that Gilbert himself did not by any means overlook the possibilities just suggested regarding the dominance of cohesion; for after briefly examining the relative values of densities and of penetrability, he concluded: "Since the condition of penetrability resides in the solid rock only, and the condition of density pertains to both solid and fluid, either condition might determine laccolites at certain stratigraphic horizons, while the latter only could discriminate certain lavas as intrusive and others as extrusive" (74). But inasmuch as he found that the lavas of the plateau region are, as a matter of fact, sharply discriminated between intrusions and extrusions, he was "led to conclude that the conditions which determined the results of igneous activity [for that region] were the relative densities of the intruding lavas and of the invaded strata; and that the fulfillment of the general law of hydrostatics was not materially modified by the rigidity and cohesion of the strata" (74, 75). Evidently enough, if he had found examples elsewhere in which relatively light intrusive rocks formed laccolithic masses in relatively dense strata, he would have placed a

[4] Amer. Journ. Sci., xix, 1880, 24.

higher value on cohesion than on density in the explanation of such occurrences, but even then he might still have very reasonably held to his original explanation for the Henry Mountains laccoliths.

True, it has been proved that the erupted lavas in a certain district of the Yellowstone Park were supplied by the same magma that fed a number of underlying dikes and sheets, now laid bare by later erosion; but the sheets are not especially laccolithic in form, and moreover the disturbed structure of that region makes it possible that diastrophic forces as well as the pressure of the rising magma had to do with the opening of underground fissures for the deep-seated intrusions; and in such case these phenomena would fall into a different category from that which includes the typical laccoliths of the undisturbed Henry Mountains district. It is true also that Gilbert's explanation of his laccoliths in terms of the hydrostatic law requires, as he candidly stated, the assumption that the present relation of densities in laccolithic and volcanic rocks should have been reversed when those rocks were molten and before the volcanic rocks in particular had lost a large share of their originally included gases; and this assumption has not yet been verified. But if a thinker like Gilbert found value in the assumption, that is a good and sufficient reason for presenting it as a part of his theory of laccoliths; yet, such presentation is rarely if ever found.

EFFECT OF MAGMATIC VISCOSITY

As to the second point—the control of the diameter and domelike form of laccoliths—Dana's explanation in his review is that, as the lighter magmas are the least fusible, viscosity becomes dominant in determining the form that they assume; they are "easily chilled and thicken greatly in the upper part of narrow fissures or of volcanic conduits, and it is for this reason that they have so often made steep sided *domes* over subaerial vents." This point is somewhat more fully treated in the latest edition of his Manual:

> The thickness [of intrusions] depends somewhat on the fusibility of the rock, the more fusible kinds making extended masses or sheets, and the less fusible producing thicker and more bulging forms. . . . The breadth of the [laccolithic] mass is consequently only three to seven times greater than the height.[5]

Here the implication is clearly made that the magma of a laccolith begins to thicken vertically as soon as it turns to spread laterally, as if its viscosity were so great that its upward pressure could not be readily turned into a lateral or horizontal pressure. But if a domelike form is thus early assumed, the sharp bending of the domed strata could be accomplished only at a great mechanical disadvantage, for in such case the strata above the dome would have to be strongly up-arched while the laccolith is still small; moreover, the strata over the center of a completed laccolith, which has a nearly flat-topped dome, are very little arched. Hence, on the assumption of a viscous magma which begins to thicken as it begins to spread, the strata over its center must be first up-arched and afterwards flattened again. Such a doing and undoing is conceivable, but it does not seem probable.

Furthermore, the assumption of a viscous magma overlooks or sets aside the evidence that convinced Gilbert of the effective fluidity of the magma; more serious still, this explanation neglects the observed facts of the Henry Mountains which indicate that full-sized thin sheets were formed before their thickening into domelike masses was begun; and most unfortunately of all, it takes no adequate account of the mechanics of doming, and nowhere recognizes the pertinent principles that "the rigidity or strength of a body is measured by the square of its linear dimensions, while its weight is measured by the cube," and that "a small laccolite [of fluid lava] can not lift its small cover, but a large laccolite can lift its correspondingly large cover" (97). Gilbert alone has considered these essential elements of the laccolith problem.

Fortunately, however, Dana made a nearer approach to Gilbert's view on the next page of his Manual: "As Mr. Gilbert states, the intrusion of the lava laterally into a chamber widened the area of pressure, and thus enabled it, on the principle of the hydrostatic press, to accomplish the lift by very slow steps of progress"; but even this does not recognize the mechanical advantage that a sheet of large diameter has over a sheet of small diameter beneath a given thickness of cover in raising the overlying strata and thickening itself into a domelike mass; nor is recognition given to the remarkable confirmation for Gilbert's theory that is found in the increase of the diameters of laccoliths with depth.

[5] Manual of Geology, 4th ed., 1895, 301.

EVIDENCE FOR MAGMATIC FLUIDITY

The validity of Dana's assumption that viscosity is the cause of laccolithic doming may be next examined. As above noted, this assumption takes no account of the narrow dikes above the laccoliths and of the thin sheets around them, although both these classes of intrusions result from the immediate entrance of the laccolithic magma into cracks and splits that appear to have been produced by strains in the overlying strata, not at the first stage of sheetlike spreading but later, when the domelike form of the central mass was eventually assumed. Hence the magmas of the Henry Mountains laccoliths must have preserved their fluidity all through the period of their intrusion in the presence of the great superincumbent pressure under which they were intruded, even though the same magma might be somewhat viscous in surface eruptions. Moreover, serious mechanical difficulties are involved in the assumption that laccolithic intumescence went on *pari passu* with lateral spreading; for in that case it must be believed that the up-arching of a small area of overlying strata by the direct upward push of the fluid magma was easier than the radial extension of the magma in a thin sheet, as a result of a hydrostatic change in the direction of the push from vertical through the conduit to lateral between the inclosing strata. A magma must have been extremely viscous to act in this way. When it is recognized that the production of a small arched dome by upward magmatic pressure in a series of horizontal strata 2 miles thick is accomplished at a great mechanical disadvantage as compared to the production of a flat-topped dome of much larger horizontal radius in the same thickness of strata, and when it is remembered that the laccolithic magma behaved essentially like a fluid in filling all available cracks and splits, it ought to be understood that this same magma, when "solving the problem of least work," would have avoided the difficult task of producing a small arched dome and would have chosen the easier task of extending itself laterally in a thin sheet before beginning to thicken in a flat-topped dome. In other words, a magma which was fluid enough to form narrow dikes and thin sheets at a late stage of its intrusion, could not have been viscous enough at an early stage to persist in upward pressure when a lateral escape was opened; it must have then been mobile enough to change its upward pressure into a horizontal thrust at the level of laccolith formation.

VALIDITY OF GILBERT'S VIEW

Finally, the facts recorded by Gilbert prove that in certain cases the laccolithic magma did actually begin its lateral movement by spreading horizontally between the strata in comparatively thin sheets which caused small uplift, and that the formation of flat-topped domes was not undertaken until the sheets had acquired a considerable area. Of prime importance here is the evidence given by the Howell laccolith, which has a depth of only 50 feet in a breadth of over 2,000 feet; and yet even this sheetlike laccolith is critically distinguished from normal intrusive sheets by a sudden thinning at the margin:

> In place of the tapering wedge which usually terminates intrusive sheets, there is a blunt, rounded margin, and the lava scarcely diminishes in depth in approaching it. The underlying strata, locally hardened to sandstones, lie level; the overlying curve downward to join them, and between the curved strata is interleaved [exterior to the laccolith] a curved lava-sheet. In all these characters the intrusive body is affiliated with the typical laccolites, and distinguished from the typical sheets (34, 35).

Surely if a "curved lava sheet" could have been intruded exterior to the margin of a laccolith, where the cooling of the magma must have been greatest, and yet could even there have had tapering edges and a much less maximum thickness than 50 feet, the intrusion of such a curved sheet contradicts the assumption that the magma of a laccolith, even at the center of its supply where it must have been hottest, was so stiffly viscous as to bend up the heavy overlying strata almost as much as it bent its own motion.

Indeed, the possibility that the magma was viscous is hardly referred to in Gilbert's analysis; it is always described as a fluid which obeyed the hydrostatic law, and this conception was clearly not a gratuitous assumption but a well-reasoned conclusion, based on such facts and inferences as have just been presented. For example, in the final summary, entitled "History of the Laccolite," the explicit statement is made:

The station of the laccolite being decided [namely, when lavas . . . reach the zone in which there is the least hydrostatic resistance to their accumulation], the first step in its formation is the intrusion along a parting of strata, of a thin sheet of lava, which spreads until it has an area adequate, on the principle of the hydrostatic press, to the deformation of the covering strata. . . . So soon as the lava can up-arch the strata it does so, and the sheet becomes a laccolite (95).

Twenty years later, Gilbert quoted another writer's view that viscosity is "an essential condition in the production of thick intrusive lenses," but added that "this theory encounters serious difficulty."[6] This appears to be the most explicit reference that he made to viscosity. In his judgment it did not seem to deserve consideration.

It would, indeed, be extremely difficult to imagine that the upward flexing of the strata which cover the thin Howell laccolith began, as the assumption of a viscous magma demands, when the diameter of the spreading igneous mass was small, and that the flexing was then continued radially outward as the diameter increased; for that process of deformation would involve a progressive unflexing of the first-flexed central strata, in order that they should now lie flat on the upper surface of the intruded mass. Hence, thin as this laccolith is, its initial intrusion must have been thinner still; and the thickening to the present modest measure of 50 feet, by which the upflexing of the marginal strata was caused, could not have been begun until the present diameter of the laccolith was reached. But it is not this exceptionally thin laccolith alone that furnishes evidence for the initial intrusion of thin sheets. Additional evidence is given by certain other laccoliths which "appear to be built up of distinct [igneous] layers." Even the thin Howell laccolith shows two such layers.

The Peale exhibits three layers with uneven partings of shale. The Sentinel shows two without visible interval. . . . The Pennell has a banded appearance but was not closely examined. . . . It is probable that all the larger laccolites are composite, having been built up by the accession of a number of distinct intrusions (55).

Hence, as far as the Henry Mountains laccoliths are concerned, even if the magma were imperfectly fluid, it was fluid enough to spread in comparatively thin sheets of a considerable diameter before it began to thicken into domelike forms, either by its own intumescence or by the addition of new sheets. Viscosity may play a part in determining the form of other intrusive masses, especially when it is of a much higher degree than it can have been in the Henry Mountains laccoliths; and extreme fluidity as well as a relatively high density may have had to do with the great horizontal extension of certain thin intrusive sheets, such as the whin sill of northern England; but viscosity does not appear to have determined the doming of typical laccoliths. Gilbert evidently had good reasons for his conclusions in this problem. It must therefore be urged that whatever explanations may be offered for intrusions of various irregular forms in divers crustal structures, the explanation that Gilbert presented in the middle chapters of his report for the intrusion of the typical laccoliths of the Henry Mountains into the undisturbed strata of southeastern Utah takes fuller account of all attendant conditions and is more critically and convincingly analyzed than any of its successors. The explanation may not be right in every particular, but it has not yet been shown to be wrong for the laccoliths that it treated. It may be suggested that rate of intrusion, a factor to which Gilbert gave little attention, may nevertheless have been of importance in determining the form assumed by the intruded magma. A very slow intrusion might gain a horizontal dimension much greater than the limited diameter of Gilbert's theory, and a very rapid intrusion might break the covering strata and enter therein irregularly.

If the foregoing pages appear at first reading to be devoted more largely to the discussion of a peculiar geological problem than is appropriate in a biographical memoir, let it be understood that they have really been devoted to an exposition of the care with which the analysis of a new problem was conducted by an eminent geologist. Whatever value his analytical explanation of laccoliths may have in making known the true nature of the Henry Mountains, an understanding of the explanation is indispensable if one would know the true nature of its inventor. Indeed, the middle chapter of the Henry Mountains report possesses, to a degree that is almost or quite without rival, that clearness of treatment which became a pronounced characteristic of all Gilbert's later papers, and which gave him the rank of a leader among

[6] Journal of Geology, IV, 1896, 821.

American geologists. Even if that chapter has not been fully appreciated by later writers, it not only taught many early readers a most instructive lesson in physical geology but gave them their first introduction to a personality by which their own work was to be profoundly influenced.

It may perhaps be asked why Gilbert did not himself continue the discussion of the laccolith problem as new examples of such structures were reported in various parts of the United States and elsewhere by observers who appear to have given very scanty attention to his ideas. His indifference may have been due in part to the increasingly petrographic aspect assumed by the problem as others treated it, for petrography does not appear to have attracted him; but it was probably due in larger part to a feeling that, as he had had his say, he preferred to turn his attention to other subjects and therefore left his laccoliths to work out their own destiny. In any case, when he was in England in 1888 and was approached by a young geologist there who was working on some British examples of laccolithic structures and who was anxious to talk them over with the original discoverer of laccoliths, the problem seemed to have lost its interest for him; for "he professed to have been so much occupied with other things since his Henry Mountains work that he had forgotten all about it," much to the disappointment of the young inquirer. This recalls his forgetfulness about his new interpretation of Adirondack history, as told above.

CHAPTER X

THE PRINCIPLES OF LAND SCULPTURE

A PHYSIOGRAPHIC CLASSIC

It is safe to say that the chapter on "Land sculpture," occupying the last 50 pages of the Henry Mountains report, has been more generally read and apprehended than the preceding chapter of similar length on the conditions of laccolithic intrusion. The discussion of the laccolithic problem possessed, in view of the frequent use that it made of algebraic equations, a mathematical turn which was unattractive to many readers, and it moreover had, in view of the rarity of well-defined laccoliths, little relation to the experience of most geologists. On the other hand, the discussion of land sculpture, although penetratingly analytical, was simpler in its manner of presentation and was also of universal application. More important still, it was a timely chapter, for, although published less than a half century ago, it appeared at an epoch when only a good beginning had been made toward recognizing the operation of slow-acting erosional processes, not simply in furnishing detritus for marine deposition, as Lyell and other uniformitarian stratigraphers had taught, but also in the shaping of the surface of the land from which the detritus was taken, as American observers were teaching. Gilbert was still several years later of the opinion that the rational treatment of land forms should be brought to the attention of geological readers and observers, for he wrote in a preliminary report on Lake Bonneville: "We are unaccustomed to think of the ordinary forms of land as a work of sculpture, but that is none the less their origin."[1] The sculpturing work of erosion therefore needed at that time precisely the kind of deliberative elaboration that it received in the Henry Mountains report.

Powell's Exploration of the Colorado River of the West and his Geology of the Eastern Part of the Uinta Mountains, both of which were issued shortly before the Henry Mountains report, had contributed valiantly toward opening the way for Gilbert's elaborate discussion of land sculpture, for they emphasize the conviction previously announced by Newberry that even the greatest of chasms was wholly the work of ordinary erosional processes. They also explained that the downward erosion of an "antecedent" river may be faster than the upward heave of a mountain range athwart its path, and they introduced, along with the excellent term "base level," the general principle that the long-continued action of weather and streams must eventually reduce any still-standing land mass, however extensive, elevated, and resistant it may be, to a lowland sloping very gently to the shore of its adjoining sea. Powell summarized this geological philosophy in two striking sentences: "Mountains cannot long remain mountains; they are ephemeral topographic forms. Geologically all existing mountains are recent; the ancient mountains are gone." (Uinta Mountains, 196.) He also gave some consideration to the processes by which the waves of the earth's surface, raised at a time of crustal storm, are slowly flattened down during a long period of crustal calm, but his analysis of this problem was in several respects less detailed than Gilbert's. However, Powell's explanatory discussions directed attention to the problems of degradation and sculpture and placed the scientific public in the attitude of asking for "more." Gilbert's, chapter on "Land sculpture" therefore found ready attention from a large circle of readers who were eagerly waiting for further instruction on the subject that it treated.

It is nearly always the case that an essay such as this one of Gilbert's, clear as may be the treatment of its problems, gives no indication of the circumstances which prompted its composition. They are revealed, however, in a letter that Gilbert wrote to a distant correspondent nearly 30 years later, in September, 1905. He then explained:

> My youth was passed and my early geologic studies were made [he would have called them "geological" at the time of their making] in a glaciated region. When I afterward studied the mountains of the Great Basin

[1] U. S. Geol. Survey, 2d Ann. Rpt., 1881, 83.

where evidence of glaciation is not ordinarily seen, I was impressed with the topographic types because they differed from those that I was familiar with, and I was led to study the causes of their development and write an analysis of land sculpture by weathering and streams.

The increased understanding that he reached of glacial sculpture in his home district after he had gained an understanding of the normal processes and products of land sculpture in the West is alluded to in a later section.

The famous chapter on "Land sculpture," which is expanded from a briefer statement of the same problem in the essay on the "Colorado plateau province as a field for geological study," above analyzed, offers an illustration of Gilbert's manner of thought that is both pleasing and edifying; and it is particularly with reference to the revelation of his inner nature thus afforded that the chapter is here reviewed. It exemplifies a principle which he announced later, that in the teaching of a science more attention should be given to its philosophy than to its material content; for having recognized that an understanding of sculpturing processes would lead to the understanding of sculptured forms, he expounded the philosophy of the elementary processes of erosion as well as philosophy of the evolution of land forms, and set forth both doctrines in the most genial and competent manner, thus elevating them to the grade of serious studies. For example, as to processes one may learn, under the heading of "Transportation and comminution," just why it is that a river which is carrying a maximum load of detritus of varied texture will, when its velocity is diminished, not decrease its load by laying down a fraction of detritus of all textures, but by laying down only the coarsest detritus; or, in more general terms, why it is that when a river is accelerated it will successively select coarser and coarser detritus to take up; while when it is retarded, it will select first the coarsest and then successively the less and less coarse detritus to be laid down (107). Similarly, clear explanation is offered on sufficient physical grounds for the fact that "in any river system which is fully supplied with material for transportation [as a river system usually is at time of flood] and which has attained a condition of equal action [at present often denoted by the term, grade, which Gilbert himself later introduced], the declivity of the smaller streams is greater than that of the larger" (114). These facts were, 50 years ago, already familiar enough as matters of observation, yet few students of earth science then—and perhaps now also, for that matter—concerned themselves with a critical analysis of the causes of the facts. Hence, to find their causes logically analyzed was a gratification to the mind of many a reader; and the gratification thus excited was soon transmuted into gratitude toward the author of the analyses.

It was the same with the discussion of land forms as the result of erosional or sculpturing processes upon crustal masses. Many matters that were familiar enough as facts of occurrence were systematically formulated, greatly to the advantage of the study of the physical geography—or the "physiography," as it was later called—of the lands. Following Powell's lead in his Colorado River report, it was shown in the first place that the rock structure of a land mass is a fundamental factor in the determination of its sculptured form; and here was given the warrant, if indeed warrant were needed, for the introduction of "structure" as the first term in the explanatory description of land forms by those physiographers to whom a rational treatment of their subject is preferred over an empirical treatment. It was next shown that, supplementary to the "law of structure" is the "law of divides," according to which a homogeneous mass will come to have its steepest slopes at the stream headwaters and its gentlest slopes at their mouths. Under these two laws, "the features of the earth are carved. . . . . The distribution of hard and soft rocks or the geological structure, and the distribution of drainage lines and water-sheds, are coefficient conditions on which depend the sculpture of the lands. . . . The relative importance of the two conditions is especially affected by climate, and the influence of this factor is so great that it may claim rank as a third condition of sculpture" (116, 117). In this way Gilbert made approach to certain matters of detail, the explanation of which not only exemplified the unexpected interaction of several additional factors, but also illustrated the importance which comparatively small topographic features had for this master of observation and analysis. No wonder that his discussion, clear, logical, judicious, timely, "soon became the great classic among students of geomorphology."

LAND SCULPTURE AND CLIMATE

The detailed features to be discussed under "Sculpture and climate" were the sharp crests of certain summits as contrasted with the rounded crests of other summits of similar structure in the Henry Mountains; and the contrast was shown to depend initially on altitude; for altitude increases rainfall, rainfall increases vegetation, and vegetation increases the retention of weathered detritus. Hence the lower crests, where rainfall is small and vegetation is lacking, are sharp; while the higher crests, on which rainfall is somewhat more plentiful and vegetation is encouraged, are soil cloaked and round. This was one of the topics which was well thought out in the field; for it was noted on October 27, 1876, under the heading, "Mountain sculpture & climate":

> The mts which lift the largest masses highest get most moisture. This stimulates vegetation & thereby aids disintegration (weathering) & hinders transportation. The result is a smoothness of slope & contour as compared with lower, smaller mts. In general, the other conditions of erosion being the same, aridity tends to ruggedness, to cañons, to pinnacles.

The report next takes up a discussion of bad-land forms carved in homogenous soft rocks, where the law of divides leads to the expectation that the minutely subdivided crests should have angular cross profiles; but it was noted that as a matter of fact the profiles just over the crests are round.

> Evidently some factor has been overlooked in the analysis,—a factor which in the main is less important than the flow of water, but which asserts its existence at those points where the flow of water is exceedingly small, and is there supreme (123).

It is curious to read here that "some factor has been overlooked," in view of an explanatory entry in a field notebook on the same date with the extract last quoted. The entry concerns "any process which penetrates from the surface of a solid," and opens with the statement: "I think I have solved the problem of the rounding of the crests of the bad-land ridges"; then after a page of general considerations it reads: "Where the surface is convex there is greater penetration than where it is plane; and where it is concave, these is less. . . . It is under this action that angular blocks of crystalline rocks (& others) become round by simple weathering, without attrition." A significant application of this principle follows:

> Now what is the action which penetrates in the case of bad-lands? It is weathering. It is the action of water in dissolving, in decomposing, in expanding (either with or without frost) the shale & thus disintegrating it and preparing it for transportation. Thus in badlands, transportation and corrasion are tending to produce angular forms & weathering opposes their production. The action of weathering is most potent where the deviations from a plane is greatest [that is, along the convex crest of a divide]. Angularity is greatest where erosion is most rapid [that is, along the sharply incised channel of a rain-water rill].

This explanation would seem sufficient to most students of land forms; but perhaps Gilbert's failure explicitly to recognize "soil creep" as an effect of weathering—a process which Lesley had recognized and explained 20 years before—left him dissatisfied with it. In any case he certainly showed great conscientiousness if, after getting so "warm," he still felt that the rounding on the crests of bad-land divides needed further examination. He returned to this subject 30 years later in an article on "The convexity of hilltops,"[2] and explained it fully on the basis of observations made on the uplands of the Sierra Nevada. He might have at the same time adduced examples of extremely sharp crests in the plant-covered ridges of certain volcanic islands in the tropical seas, like Oahu and Tahiti, where the rainfall is so heavy that its action is far in excess of soil creep; as a result the slopes on the two sides of a ridge increase in steepness upward until they meet in an extraordinarily acute edge, and thus warrant the "law of divides" fully. But Gilbert's experience even in the wettest parts of the Henry Mountains and the high plateaus was with only a moderate rainfall; so moderate that instead of producing acute-edged ridges like those of the tropical islands just mentioned, it favored the production of rounded crests and summits in the manner stated in the first paragraph of this section.

[2] Journ. Geol., xvii, 1909, 344–350.

WANDERING STREAMS ON PLANATION SURFACES

Several other topics are treated so effectively and withal so pleasingly that they deserve statement in brief form. "Planation" is explained as the result of lateral shifting by a stream which has reduced its slope to so low a declivity that its capacity for transporting load is balanced by the load it receives to be transported; it then swings from side to side, beveling off the underlying strata whatever their attitude in a gently inclined plain; plains of this kind, veneered with laccolithic gravels, being frequently seen around the Henry Mountains. An explanation of the structure of flood plains is added which merits attention: In consequence of the lateral shifting of a balanced stream, "every part of the valley which it has crossed in its shiftings comes to be covered by a deposit which does not rise above the highest level of the water. . . . The deposit is of nearly uniform depth, descending no lower than the bottom of the water channel, and it rests upon a tolerably even surface of the rock or other material which is corraded by the stream" (127). A corollary of this explanation gives correction to an error earlier made by an eminent American geologist, who supposed that "river terraces in general are the records of sedimentation, when in fact they record the stages of a progressive corrasion. . . . There is a kindred error . . . involved in the assumption that the streams which occupied the upper and broader flood-plains of a [terraced] valley were greater than those which succeeded them. . . . Of the same order is the mistake, occasionally made, of ignoring the excavation which a stream has performed, and assuming that when the upper terraces were made the valley was as open as at present, and the volume of flowing water was great enough to fill it." A diagram is introduced showing Gilbert's own idea of valley-side flights of terraces, each step being carpeted with a flood-plain deposit lying on an eroded surface of the mass in which the cutting and depositing river excavated its terraced valley; and here the comment is added: "The pre-existent material in the region of the Henry mountains is always rock in situ, but in the Northern States it often includes glacial drift, modified or unmodified" (132, 133). It surely required an unusual capacity for seeing and thinking to develop ideas so novel and so just as these.

In connection with the wandering of streams down the radial slopes of their alluvial cones, reference is made to Blake's early observations regarding the effect of the Colorado delta in cutting off the upper end of the Gulf of California, the severed end of the gulf having been later evaporated to dryness so that its depressed floor now forms the Colorado desert.

Its bottom, which is lower than the surface of the ocean, is strewn with the remains of the life its waters sustained, and its beaches are patiently awaiting the cycle of change which is slowly but surely preparing to restore to them their parent waters (135).

This passage is quoted here in order to introduce an item of 15 years later date, predestined by the intimacy that was formed between Gilbert and Powell in the Henry Mountains epoch. In 1905 when the misbehaving Colorado River was turning part of its flood waters into the depressed desert, as if striving to verify Gilbert's prediction, he confessed to a friend his share in the authorship of an article, written in Washington in 1891 and published over Powell's name, on a part of the desert known as the Salton Sink. The confession explains the circumstances of attending the composition of the article as follows:

Powell sent for me one afternoon and said he had been asked to write on the Salton Sea; he had already written twice and could not write again without repeating; he could not turn the invitation over to me because the publishers wanted his name; if I would write he would share the spoils with me. I had my stenographer come to my house and dictated the article that evening. [Let the reader note that survey time was not infringed upon]. Powell added two paragraphs next morning and it went off before night. My share was fifty dollars, which made me happy. I have just reread the article . . . and it seems to me to check up by modern history remarkably well. Of course it is Powell's article, so far as any references in print are concerned—and I think I wrote it with exceptional freedom because he instead of I was to be responsible.

If any punctilious readers feel a bit scandalized by this performance of a man so irreproachable as Gilbert, let them remember the pious fraud perpetrated by the equally impeccable William James. He was one evening attending to the illustrations of a lecture at Harvard on a physiological subject by a visiting professor from Johns Hopkins; a lever touching the pulse

of some small animal was to be magnified by projection upon the lantern screen so as to exhibit heart beats to the audience. Unfortunately, something interfered with the mechanism at the critical moment and the lever shadow remained stationary, until James himself gave it a series of gentle periodic touches, whereupon the words of the lecturer were admirably verified by the pulsating shadow; and as James remarked afterwards, "The audience was entirely satisfied." So, it may be assumed, was the reading public in the other case.

INTERDEPENDENCE OF DRAINAGE LINES

Of much greater importance than these small matters is the law of the interdependence of drainage lines, which is certainly one of the most beautiful of Gilbert's generalizations. Given "the tendency to equality of action, or to the establishment of a dynamic equilibrium" in the streams of a well-dissected land surface, it must come to pass that every valley-side slope will be—

> a member of a series, receiving the water and the waste of the slope above it, and discharging its own water and waste upon the slope below. If one member of the series is eroded with exceptional rapidity, two things immediately result: first, the member above has its level of discharge lowered, and its rate of erosion is thereby increased; and second, the member below, being clogged by an exceptional load of detritus, has its rate of erosion diminished. The acceleration above and the retardation below, diminish the declivity of the member in which the disturbance originated; and as the declivity is reduced the rate of erosion is likewise reduced. But the effect does not stop here. The disturbance which has been transferred from one member of the series to the two which adjoin it, is by them transmitted to others, and does not cease until it has reached the confines of the drainage basin. For in each basin all lines of drainage unite in a main line, and a disturbance upon any line is communicated through it to the main line and thence to every tributary. And as any member of the system may influence all the others, so each member is influenced by every other. There is an interdependence throughout the system (124).

How alive, how organic a landscape becomes when the truth of this extraordinary principle is realized. How infinite the patience of the processes by which the orderly interdependence of slope lines is established. And when once established, how persistent is the interdependence in the further progress of degradation, as long as no crustal or climatic disturbance intervenes.

As if to illustrate the manner in which the interdependence of drainage lines is evolved, several pages are devoted to classifying streams according to their origin and to explaining the changes that they and their divides afterwards suffer by reason of reactions between external erosion and internal structure. Many examples of such changes are taken from the Henry Mountains district. It was indeed from these pages that most American students of land sculpture gained their first introduction to the fascinating problem of river "abstraction" or capture, a subdivision of the rational treatment of rivers that has been much advanced in later years, largely because of the great impulse that Gilbert gave to it. It should therefore be understood that his analysis of the conditions by which river courses are established in their initial stage and by which they are diverted or shifted in their later stages of development, although incomplete, shows that he apprehended even more fully than Powell the responsibility of the physiographer to give a reasonable account of drainage systems, as well as of land forms. Physiographers were soon united in feeling their great indebtedness to him and in striving to learn and to apply the principles that he taught.

TERMINOLOGY OF DRAINAGE LINES

It is instructive to trace Gilbert's progress in the matter of stream terminology. Attention has already been called in the review of his early Maumee Valley study to the fact that he considered the origin of stream courses as a problem for investigation; he there explicitly noted that "the smaller streams follow and indicate the slopes," and that the larger ones are guided by the morainic ridges; but he proposed no genetic name for streams of this kind. Again, it has been noted in the review of his two reports for the Wheeler survey, that he there, while using such a term as monoclinal valley appropriately enough, employed no systematic series of descriptive or genetic terms to indicate the origin of river and valley courses. The same is

true of the first 50 pages of the Henry Mountains report, in which the manifestly consequent radial drainage of certain laccolithic domes is described without any genetic term by which to name it:

The drainage of Mount Ellsworth is from the center of the dome outward. A half dozen drainage-lines originate in the high crests and pass outward through the zone of upturned strata. Lower down the interspaces are divided by others, and when they reach the circling escarpment of the Vermilion sandstone their number is fifteen (25, 26).

It might perhaps be inferred from Gilbert's failure to introduce here a systematic terminology for stream courses of different kinds, that terminology did not greatly interest him; yet some years later he spoke with much satisfaction of the advantage that had come from his invention of the term "laccolite." Indeed, even for streams, he used a systematic terminology when it came fully to his attention; for in the final chapter on "Land sculpture," Powell's terms, consequent, antecedent, and superimposed are adopted, and it is then concisely said:

The drainage of the Henry Mountains is consequent on the laccolitic displacements. . . . The drainage system of Mount Ellsworth is more purely consequent than any other with which I am acquainted (144, 145).

Again, the drainage of the greater arch of Mount Holmes, which still preserves its covering strata, is described as "consequent upon the structure"; that is, on the dip of the up-arched strata; and the successive stages of its development are pleasingly outlined:

When it was first lifted it became a drainage center because it was an eminence; and afterwards it remained an eminence because it was a drainage center. When in the progress of denudation its dikes were exposed, their hardness checked the wear of the summit and its eminence became more pronounced (147).

Gilbert's final adoption of "consequent" and its associates of Latin origin in the sense above indicated was evidently the result of conference with Powell, by whom they had been announced two years before in his Colorado River report. It is of interest to note in this connection that, of the two sets of terms which Powell introduced, one of Greek derivation, embodying an empirical relation of streams to structures, and the other of Latin derivation, indicating a genetic relation of streams to structures, Gilbert, familiar with both, preferred the latter. His familiarity with the first set is shown in a notebook entry of September 20, 1876, a year after the Colorado River report was published. During an ascent of one of the laccolithic mountains on that date it was recorded that the horses were left at a certain point, beyond which the party "traveled all the way cataclinal, the dip being 7° by guess & our course inclining 20°"; but excepting "monoclinal," the other terms of this set were not elsewhere employed. As to the terms of the other set, they appear to have been familiar at least a year earlier, for a note under date of July 12, 1875, uses both "antecedent" and "superimposed." In view of this it is surprising that "consequent" was used the same year in a sense altogether different from that which Powell had given to it, as the following narrative account will show.

THE WATERPOCKET CANYON

It will be remembered that Gilbert first approached the Henry Mountains in the summer of 1875 from the west, and that on the way he crossed the greatly degraded east-dipping monocline of the long Waterpocket flexure, which trends about north and south. In the northern part of its length the flexure is traversed by the east-flowing headwaters of Dirty Devil River, which in its farther course flows around the mountains on the north and east to the Colorado; but the southernmost 30 miles of the flexure are followed by a continuous longitudinal valley, excavated along a belt of weak shales between the rising roll of underlying sandstones on the west and the escarpment of overlying sandstones on the east. This valley which Gilbert describes in his report as the longest monoclinal drainage line with which he is acquainted—

here bears the name of Waterpocket cañon . . . The upper part of the cañon is dry except in time of rain, but the lower carries a perpetual stream known as Hoxie Creek [which flows directly to the Colorado]. Whatever may have been the original meanderings of the latter they are now restrained, and it is limited to the narrow belt in which the shale outcrops. As the cañon is worn deeper the channel steadily shifts its position down the slope of the underlying . . . sandstone, and carves away the shale. But there is one exceptional point where it has not

done this. When the bottom of the cañon was a thousand feet higher the creek failed, at a place where the dip of the strata was comparatively small, to shift its channel as it deepened it, and began to cut its way into the massive [underlying] sandstone [on the west]. Having once entered the hard rock it could not retreat but sank deeper and deeper, carving a narrow gorge through which it still runs making a [3-mile] detour from the main valley (137, 138).

The main monoclinal valley has been weathered and washed out along the streamless intervening space to almost the same depth as farther north and south where the creek runs. Such is the origin of the little Horseshoe Canyon, figured in an upstream or northward view on page 138 of the report.

Now if the drainage of this flexure were stated in accordance with the present terminology of stream and valley lines, the headwater branches of the Dirty Devil where they cross the flexure would, like the radial streams of Mount Ellsworth, be called consequent; and Hoxie Creek, manifestly developed by the headward erosion of a branch of the Colorado along the belt of weak shales in the great flexure, would be called subsequent, except that its detour through Horseshoe Canyon might be described as having been locally superposed by planation during a time of wandering in a late stage of a former cycle of erosion—probably the cycle that preceded the current cycle of canyon incision. But Gilbert did not use these terms. The terminology that he employed is best understood by reviewing the earlier stages in his treatment of stream and valley lines.

His first stage is found in the Wheeler reports, where certain valleys that follow the strike of weak strata were in some cases at least given the empirical or structural name of monoclinal valleys, as has been already told. A second stage was reached after he joined the Powell survey, when, curiously enough, he used "consequent" in a peculiar sense in a notebook, under date of August 18, 1875; after giving a brief account of Horseshoe Canyon and another but smaller detour of Hoxie Creek he wrote:

> It is a strange watercourse, consequent (following a mono) for most of its course, but inconsequent in two places.

That is, he regarded the main course of the creek as "consequent" not upon the initial slope of the great flexure but upon the monoclinal belt of weak shale laid bare between the overlying and underlying sandstones; and the two detours were regarded as "inconsequent" because they departed from the monocline. Such a terminology was perfectly consistent within itself; but it was peculiar in leaving its first term without explanation, although an explanation for its second term—superposition by planation—was suggested in the final report.

A third stage of treatment in terminology exhibits progession in certain respects, and retrogression in another. It is found in the Henry Mountains report, where Powell's terms are adopted as already noted. Here "consequent" is employed, as quotations given above clearly show, for streams that still follow courses originally determined by the initial slope of a deformed land surface, like that of a laccolithic arch or a monoclinal flexure, and this was a forward step; but singularly enough no term is introduced to express the other relation between stream and structure previously attached to "consequent" in its notebook meaning, and this was a backward step. Thus while Gilbert did not commit the positive error made by Powell earlier and by Dutton later of naming a monoclinal stream like Hoxie Creek "antecedent," he unfortunately overlooked the descriptive use of "subsequent" in an account of the erosional origin of monoclinal streams by Jukes 15 years before, and he committed the negative error of not inventing any other genetic name for streams and valleys of this class. The reason for this omission appears to be that subsequent streams like Hoxie Creek had not run across his path frequently enough to hold his attention upon them. It is true that the subsequent monoclinal ridges and divides, the converse of subsequent monoclinal valleys and streams, which encircle a number of the Henry Mountains domes, were seen, described, explained, and named, but the little subsequent valleys between the ridges and the domes were not especially considered, as will appear from the following passages in the report.

SUBSEQUENT VALLEYS IN THE HENRY MOUNTAINS

In association with the above citation concerning the radial streams of Mount Ellsworth, Gilbert wrote:

> It is usually the case, where the strata which incline against the flank of a mountain are eroded, that the softer are excavated the more rapidly, while the harder are left standing in ridges; and an alternation of beds suitable for the formation of a ridge occurs here. One of the upturned beds is the massive Vermilion Cliff sandstone, and beneath it are the shales of the Shinarump group. By the yielding of the shales the sandstone is left prominent, and it circles the mountain in a monoclinal ridge. But the ridge is of a peculiar character, and really has no title to the name except in the homology of its structure with that of the typical monoclinal ridge. It lacks the continuity which is implied by the term "ridge."

Then follows the passage already quoted regarding the 15 radial streams, after which it is added:

> Each of these cuts the ridge to its base, and the effect of the whole is to reduce it to a row of sandstone points circling about the mountain. Each point of the sandstone lies against the foot of a mountain spur, as though it had been built for a retaining wall to resist the outthrust of the spur. Borrowing a name from the analogy, I shall call these elementary ridges *revet-crags*, and speak of the spurs which bear them as being revetted (25, 26).

Evidently, the little subsequent valleys which head against each other in short pairs behind the prominent revet crags were not conspicuous enough to be worth naming.

The same conditions obtained around Mount Pennell, although the ridge is there more continuous:

> The Gate sandstone has been worn away nearly to the foot of the slope, and forms a monoclinal ridge circling about the base. The ridge is interrupted by a number of waterways, and it sends salients well up upon the flank, but it is too continuous to be regarded as a mere line of revetments (36, 37).

Yet nothing is here said of the monoclinal valleys that the successive arcs of the encircling ridge inclose. Although all such monoclinal valleys were understood to be excavated along belts of inclined shales, it was not explicitly stated that their excavation was accomplished by the headward erosion of wet-weather branches of the radial streams; nor was it noted that each radial stream therefore has a pair of such branches mouthing opposite each other, and that each monoclinal depression back of a revet crag has a pair of such branches heading against each other.

It thus seems that the failure to give more explicit consideration to the short monoclinal subsequent valleys around the Henry Mountains domes led also to leaving Hoxie Creek and its 30-mile monoclinal valley unexplained; both when it was earlier called "consequent" and when it was later bereft of a genetic name. And yet its explanation was unconsciously prepared for in two well-considered statements; one regarding the tendency of waterways to follow weak belts, and the other regarding the processes by which this tendency is realized. As to the first it is said:

> In a region of inclined strata there is a tendency on the part of streams which traverse soft beds to continue therein, and there is a tendency to eliminate drainage lines from hard beds.

However—

> The tendency of waterways to escape from hard strata and to abide in soft, and their tendency to follow the strike of soft strata and to cross hard at right angles, are tendencies only and do not always prevail (135, 137).

The second statement concerns the process by which streams are developed along monoclinal belts of weak strata, and for which full explanation is given under the discussion of the instability of divides, where it is shown that:

> In homogeneous material, and with equal quantities of water, the rate of erosion of two slopes depends on their declivities. The steeper is degraded the faster. It is evident that when the two slopes are upon opposite sides of a divide the more rapid wearing of the steeper carries the divide toward the side of the gentler. The action ceases and the divide becomes stationary only when the profile of the divide has been rendered symmetric. . . . It results also that if one of the waterways is corraded more rapidly than the other the divide moves steadily toward the latter, and eventually, if the process continues, reaches it. When this occurs, the stream with the higher valley abandons the lower part of its course and joins its water to that of the lower

stream. . . . There is one case which is specially noteworthy on account of its relation to the principles of sculpture. . . . If in the course of time one of the [two parallel] streams encounters a peculiarly hard mass of rock while the other does not . . . the unobstructed stream will outstrip it [the obstructed stream], will encroach upon its valley, and will at last abstract it. . . . Thus by abstraction as well as by monoclinal shifting, streams are eliminated from hard rocks (140, 141).

THE SUBSEQUENT ORIGIN OF WATERPOCKET CANYON

Yet although both tendency and process were thus explicitly treated in general terms, they appear to have reference chiefly to the case of equal streams in horizontal structures, especially in bad-land areas. Nevertheless the principles involved apply very closely to the case of parallel streams of unequal size that flow square across an inclined series of hard and soft strata, such as is exposed in the great Waterpocket flexure; for here the Dirty Devil headwaters represent the obstructed stream and the Colorado the unobstructed stream in Gilbert's supposed example; the divide between the two will be locally homogeneous in each belt of rocks and therefore subject to competitive erosion and shifting; but the shifting of the divide between the two streams will be most rapid and therefore most conspicuous in the weakest belts. Hence the streams of the Dirty Devil group and the Colorado may be regarded as constituting an actual case of competitive parallel streams, in so far as the Waterpocket flexure is concerned; and the fact that they cross it in opposite directions, the first with and the second against the dip of the flexure, only gives the greater advantage to the latter.

When this great flexure was upwarped with a maximum displacement of 7,000 feet, a host of small consequent streams must have run eastward down its slope; but the Colorado was perhaps strong enough to run against it then as it does now. In due time a part of the divide between the deeply incised west-flowing Colorado and the next adjoining small east-flowing consequent stream on the north, much less deeply incised, must have lain across the belt of weak shales along which the present Waterpocket Canyon is excavated; and as the divide was there in homogeneous material but much steeper on the southern or Colorado side, it must have been rapidly carried northward far enough to "abstract" or divert the headwaters of the small consequent. Thereupon the divide between the first and second consequent streams, which had previously been essentially symmetrical, would become strongly unsymmetrical, by reason of the steep descent of the diverted first consequent in its southward subsequent course along the weak shale belt to the Colorado; hence this divide would in its turn be carried northward until the second consequent was diverted; and so on, as far as the asymmetry of the divide continued. The Colorado would continually gain, and the streams of the Dirty Devil group would as continually lose. The shale valley is longitudinal in relation to the trends of the neighboring ridges, monoclinal in relation to the great flexure that determines it, and subsequent in relation to its origin as compared with that of the transverse consequent valleys. Preparatory to every capture this valley would slowly increase in length by headward erosion. At the time of each capture, the area of the shale-valley drainage would be suddenly enlarged by a lassolike looping of its divide around the headwaters of the captured consequent, and the volume of the capturing stream would be at the same time suddenly increased. Thus the Waterpocket Canyon, as the actual shale valley of the flexure is called, and its stream, Hoxie Creek, must have been developed in true subsequent fashion. It may be added, however, that substantially the whole of their present length would appear to have been gained in an earlier cycle of erosion, preceding the present cycle of deep canyon incision; for Hoxie Creek must have been well established before its horseshoe detour could have been cut; yet the process of headward erosion is presumably still continued, for the waters of the next-to-be-captured consequent stream must now make a circuit of about 100 miles, around the northern and eastern side of the Henry Mountains by the Dirty Devil and along their southern side by the Colorado, before reaching the mouth of the direct 30-mile subsequent valley which Hoxie Creek follows.

When the problem is thus envisaged with the aid of hindsight, it is difficult to understand why Gilbert did not have the foresight to see through it. Its essential features, as far as they involve matters of fact, were all well known to him, for they are beautifully illustrated on the relief map of the region, Plate 1 of his report. Over a score of small consequent headwaters are shown to be

still gathered in by the roundabout Dirty Devil and therefore yet to be diverted to the more direct subsequent course of Hoxie Creek. Gilbert must surely have been familiar with these details, for he had not only been much concerned in making the model from which the relief map was photographed, but he had previously crossed the Waterpocket flexure four times in different parts of its length. His first sight of it was when going eastward in August, 1875, when he learned to know Hoxie Creek and its horseshoe detour, as detailed in the report and described in abstract here. He next saw the flexure when he was returning westward some 40 miles farther north; his notes of September 1 record that the larger branches of the Dirty Devil there cross the monoclinal valley of the shale belt and cut square through the sandstone escarpment on the east; and that the shale valley, although continuous as a depression for a good number of miles, "is not a line of drainage except for short washes"; these short washes evidently being pairs of embryonic subsequent streams as now understood. The two crossings of 1876 did not yield new facts, but they must have made the facts previously noted more familiar. Nevertheless, in spite of the knowledge thus gained, in spite of the clear statement of the principle that streams tend to avoid hard strata and to abide in weak strata, announced just before the description of Hoxie Creek above quoted, and in spite of the clear explanation on a later page of the processes by which divides are shifted, no application of the principle and the processes to the facts was made in this particular case; Hoxie Creek, a typical example of a subsequent stream, was therefore left without physiographic explanation and without genetic name.

And yet it can not be doubted that Gilbert carried the problem of land sculpture as far forward as he could. His analysis of the processes concerned was carefully conducted; his treatment of the resulting land forms was deliberate and critical. If he did not immediately reach an understanding of subsequent streams and valleys, it must be that that subdivision of the natural history of rivers could not be successfully treated even by his penetrating intellect at the time of his writing. It is of course to be regretted that neither he nor Powell nor any other of his contemporaries was aware that, as has already been intimated, the problem had been successfully solved by Jukes 15 years before in his account of certain streams in southern Ireland, and it may be believed that if Gilbert had had what are called "the advantages" of more thorough collegiate instruction in the erosional chapters of dynamical geology, as that very physiographic subject was understood in his time, his reports would have had a greater number of footnote references to Jukes and other European authors than are now to be found in them. It is, however, also possible that such instruction in dynamical geology as was available in the sixties might have clogged his inquiring mind with conservative prepossessions, and that he might have been thus impeded rather than aided when he had to interpret new phenomena in a new field. But instead of vainly speculating on this might-have-been aspect of the matter, it is more profitable to learn a lesson from the actual facts; namely, the absence of a competent explanation and of a genetic name for subsequent valleys. The chief lesson here is that progress in physiographic terminology, which goes with progress in physiographic interpretation, is, like progress in general, not accomplished all at once even by a Gilbert, for it is an evolutionary process; and on the other hand, that such progress is not made by minute and imperceptible increments but per saltum, for the advance that Gilbert made over his predecessors was notably great; and finally, that successive leaps of progress are separated by pauses of no progress; for the advance made in the Henry Mountains report was not immediately continued either by its author or by anyone else. This lesson is reenforced by the next.

BASELEVEL AND TIME

Inasmuch as Gilbert's adoption of Powell's river-course terminology had manifestly been furthered by intercourse between the two men in Washington, it is truly remarkable that Powell's invaluable term, "baselevel"—now preferably written as a single word, "baselevel"—was not also taken over, for it was conspicuously introduced and employed in Powell's Colorado River report; yet it occurs nowhere in Gilbert's Henry Mountains volume. The phrase "base plane of erosion" is used in a field note under date of July 20, 1875, but not in the report: One may search for the term, baselevel, in vain all through the discussion of erosional processes,

under such headings as declivity, transportation, and balanced action. Indeed, even the idea that underlies the term is mentioned only in connection with river mouths, and curiously enough it is usually the mouths of branch rivers in trunk rivers that are referred to, and not the mouths of trunk rivers in the sea. The latter relation is stated once and empirically in connection with the general problem of the upstream increase of river grade.

If we follow a stream from its mouth upward and pass successively the mouths of its tributaries, we find its volume gradually less and less, and its grade steeper and steeper, until finally at its head we reach the steepest grade of all (116).

Farther on the problem is more locally treated in connection with the drainage of the Henry Mountains:

The streams which flow down them are limited in their rate of degradation at both ends. At their sources, erosion is opposed by the hardness of the rocks. . . . At their mouths, they discharge into the Colorado and the Dirty Devil, and cannot sink their channels more rapidly than do those rivers (175).

Gilbert's whole discussion of land sculpture is a "fresh-water" discussion, and Powell's discussion was very much the same.

The reason for this would appear to be that both Powell and Gilbert lived and worked within a broad continental area, and not along its ocean borders. Although both of them greatly advanced the physiographic problem of land sculpture in their early reports, neither of them then discussed the action of the waves on shore lines. Gilbert later gave truly enough a most illuminating account of lake shores, as will be told below; but he never gave much attention to continental coasts; and Powell's only concern with oceanic shore lines was of a perfunctory kind in one of the short-lived "National Geographic Monographs." The progress of physiography in continental America is therefore just the reverse of its progress in insular Great Britain; for there, on a land fragment where, as one may say, it is hardly possible to escape the sound of the surf, the work of rivers was relatively neglected and the sculpture of inland escarpments as well as the excavation of interior lowlands was long attributed to marine action; while here, in a continent so broad that the very existence of an ocean may be forgotten while the problems of the vast interior are considered, the importance of river action was more fully recognized, and naturally enough the action of ocean waves on continental coasts at the same time was relatively overlooked.

The absence of the important physiographic factor, time, from Gilbert's reports is more perplexing. He must have known perfectly well that the existing conditions of drainage systems as well as the existing forms of the land surface are the product of erosional processes acting upon structural masses through longer or shorter periods of time; yet his account of streams and of land forms is much more concerned with their existing status than with their evolutionary development from an earlier or initial status into the present status. It is only by reading between the lines that the idea of systematic change with the passage time is to be gathered, and even then but incompletely. The passage about stream volumes and grades quoted in the second preceding paragraph concerns only a maturely developed river system; nevertheless the law of increasing steepness upstream is, without qualification, said to apply "to every tributary and even to the slopes over which the freshly fallen rain flows in a sheet before it is gathered into rills. The nearer the watershed or divide the steeper the slope; the farther away the less the slope" (116). Yet this evidently holds good only for ready-made, full-grown drainage systems, neither young nor old. It is true that the need of much erosional work in the production of a systematic increase of river grade from mouth to source is intimated a few lines later, when it is said that such an arrangement "is purely a matter of sculpture, the uplifts from which mountains are carved rarely if ever assuming this form"; but the idea of development here intimated is not fully carried out. Consideration of the time factor is exceptional all through the chapter on "Land sculpture." Even the possibility that rivers may grow old and that mountains may be worn down is presented only as an unrealizable tendency:

It is evident that if steep slopes are worn more rapidly than gentle, the tendency is to abolish all differences of slope and produce uniformity. The law of uniform slope thus opposes diversity of topography, and if not complemented by other laws, would reduce all drainage basins to plains.

20154°—26——14

But the possibility of such reduction is at once denied: The law of uniform slope "is never free to work out its full results; for it demands a uniformity of conditions which nowhere exists" (115).

It is chiefly in connection with the balanced action of maturely graded rivers, as they would to-day be described, that the evolution of drainage systems and of the associated land forms is most directly intimated. One there finds statements like the following:

> As the soft rocks are worn away the hard are left prominent. The differentiation continues until an equilibrium is reached through the law of declivities. When the ratio of erosive action as dependent on declivities becomes equal to the ratio of resistances as dependent on rock character, there is equality of action (115, 116).

Yet the time element is not specifically treated here. Even the beautiful generalization concerning the interdependence of slopes is introduced abruptly, although it is a condition which is only reached after an immense amount of preparatory work has been accomplished; for it involves not only the degradation of every cascade in all the streams, but also the obliteration of every outcropping ledge on all the interstream slopes. The introduction is:

> The tendency to equality of action, or to the establishment of a dynamic equilibrium, has already been pointed out in the discussion of the principles of erosion and of sculpture, but one of its most important results has not been noticed (123).

Interdependence of slopes being thus presented only as the result of a tendency, the reader gains little understanding of the many changes through which the tendency has been realized.

The failure to give fuller consideration to the time element and to the changes of action and of form that go with it occasionally leads to minor errors. Thus it is announced under the law of structure:

> Erosion is most rapid where the resistance is least, and hence as the soft rocks are worn away the hard are left prominent (115, 116).

Yet this statement evidently applies only in the early stages of erosional degradation, while the weaker structures are yet to be worn down. It is reversed in the later stages, after the weaker structures have been worn down; for then, during the remainder of an uninterrupted cycle of erosion, the prominences composed of the harder strata will be worn down faster than the surface of the depressions already degraded on the weak strata; hence in these later stages erosion is not most rapid where rock resistance is least. True, the eventual degradation of hard-rock prominences will be much slower in the later stages of a cycle than the preliminary degradation of the belts of weak strata was in the early stages, but it will be faster than the contemporary degradation of the weak strata. Again, under "planation" it is stated that the "downward wear [of streams] ceases when the load equals the capacity for transportation" (126); and this overlooks the long continuation of a very slow "downward wear" through the late maturity and old age of a drainage system, as the result of an equally long continued and correspondingly slow decrease of "load" from its maximum value at the time of full maturity. Revival of erosion by renewal of uplift is also omitted from this discussion as it was from the Wheeler reports

PROGRESS IN PHYSIOGRAPHY

But the reader may ask, if not exclaim: Why point out these shortcomings in a study that had so many excellencies? The answer is: Partly to reinforce the lesson of the preceding section that progress is not made all at once; but even more to spur on those discouraged physiographers of to-day who seem to fear that their science is now completely developed, and that no new progress is to be expected. The same fear might have been expressed with as much justice when the Henry Mountains report came out, for Gilbert surely carried the formulation of physiographic laws as far as he could in the final chapter of that memorable volume; and his readers of that time might have regarded his contributions as a final, beyond which no further advance was to be expected. Yet as a matter of fact even his lesser followers have gradually built higher on the firm foundations that he then laid; and if their superstructure seems to any of them a new finality, that is only because no new Gilbert has yet arisen to show them where to build next.

In the meantime, the progress made in the half century past should surely encourage our physiographers as they march into the future. Let them glory in the remembrance that, under the leadership of our western explorers, and especially under the inspiration of the philosophical observer who pointed out the way in which the slopes of the land must gradually organize themselves in a relation of interdependence under the laws of structure and of divides, an American school of physiography and hence of geography came to be developed. This school was a natural outgrowth of the impulse toward new methods of thought given by the exploration of a region in which underground structure and surface form were manifestly related. If the more recent progress of this school be less rapid than its earlier progress, that is only because the young geographers who entered the subject from the physiographic side have so seldom carried their work far forward into the human and economic aspects of the science; just as those who later entered it from the historic or economic sides have as a rule failed to apprehend the full meaning of its physiographic basis. The future development of scientific geography in America, the earlier stages of which profited so greatly from Gilbert's physiographic leadership, will depend largely on the completeness with which the whole breadth of the science is cultivated from whichever side it is entered.

CHAPTER XI

DIVERS DUTIES ON THE POWELL SURVEY: 1877–1879

LAND CLASSIFICATION

Gilbert's report on the Henry Mountains is one of the smallest volumes issued by any of the governmental surveys of its period, but its contents sufficed to give him a leading position among his associates. His future career as a geologist was assured. Yet for two years after the Henry Mountains report was issued, while he was still a member of the Powell survey, Gilbert's attention was largely turned from the analysis of geological problems, in which his native capacity delighted as much as it excelled, and directed to matters of an altogether different nature. The season of 1877, from July to October, inclusive, was spent in northern Utah with an outfit of two men, four horses, and a wagon studying the classification of lands and the possibilities of irrigation. His estimates were based, as Powell very properly wrote, "on the experience of the [chiefly Mormon] farmers of the district, who have practised irrigation for 30 years, and have given it a greater development than can be found elsewhere in the United States."

At the beginning of the season's field work on this new subject Gilbert wrote to his chief:

I will make the circle of the Jordan valley and in it develop the alphabet of my inquiry.

Various topics are reported upon in other letters; thus, shortly after reaching Salt Lake City in July, mention is made of a personal call, which was presumably suggested by Powell as an aid to work in the Mormon settlements:

I have just seen Brigham Young who has not been in the city until today. It was just after his dinner and he felt happy. We had a nice talk of half or three-quarters of an hour and Cannon was instructed to write me the letter I want—to be signed by Young. There were a dozen present, half of whom were Church Dignitaries, and I found in a few minutes that I was talking only nominally to Brigham, but really to his advisers. They talked to the point and appreciated what I was at, but he strayed as badly as Dr. Hayden. He told me to "tell Major Powell that if he thinks I have done anything which should prevent his calling on me, he must come and see me and I will prove to him that I have not." He has had a great desire to see you ever since he learned that a piece of bacon which Fremont threw awav and which the wolves failed to find has been recently discovered in a petrified condition.

Young died later in the summer and Gilbert attended his funeral.

Other letters bore more directly upon irrigation:

There is a new agricultural development which may modify the classification of land. Lucerne, if once rooted, will grow, it is claimed, without irrigation and give each year one crop of one or two tons to the acre. If that is the case then a small stream carried in successive years to different tracts may establish meadows over a much larger area than it can permanently irrigate, and it will be proper to count as agricultural land all the arable land with suitable climate to which water may be carried with little reference to the quantity of water available. I will look into it.

Later experience did not support this hopeful expectation. A letter dated July 18 in Tooele Valley, southwest of Salt Lake City, tells of good progress and adds a personal item:

The gauging of streams, the study of beaches, and the study of recent faults go well together and make the greater part of my field work. I have finished Jordan, Cedar, Rush, and Tooele valleys, and for these valleys came at about the right time for the measurement of the water. The greatest demand for water is now or last week, when small grains want their last watering and corn its first or second. . . . I have a good outfit and everything goes well. Have read King's Catastrophism address [1] and planned half a dozen controversial papers while I was reading. Perhaps Uniformitarianism is overdone in geology, but King is certainly "out" in the opposite direction at several points. I wonder if "massive eruptions" are not laccoliths distorted by pre-existent faults.

[1] Clarence King. Catastrophism and Evolution (an address at the Sheffield Scientific School of Yale University), Amer. Nat , xi, 1877, 449–470.

It would appear from the last sentence that the possibility of the guidance of unsymmetrical laccolithic intrusions by faults, which has been recently suggested as an explanation for all such bodies, had by no means been overlooked by Gilbert, but that its application to the typical laccoliths of the Henry Mountains, which was included in the recent suggestion would not have been acceptable to him. Later in the same season he wrote to his chief from Kanab, in southern Utah: "I am anxious to hear all about the Nat. Acad. and the consolidation of surveys," thus showing that he was apprised of the movement made by Powell toward abolishing the separate departmental surveys and uniting all geological work of the Government in one organization, as was done two years later.

The results of the season's field work in 1877 are embodied in two chapters that Gilbert prepared for Powell's Lands of the Arid Region (1879). One gives a methodical account of the "irrigable lands of the Salt Lake drainage system"; the other discusses the water supply of the same area in an analytical manner thoroughly characteristic of its author. It is to be doubted whether any other discussion of this subject in more recent years, extended as it may now be by longer records of rainfall and of stream flow, is so complete as Gilbert's in the way of examining every item of cause, every step in argument. His discussion is based on recorded variations in lake level and area for the 30 years following the time of Mormon settlement in 1847, on measurements of certain streams, and on various general considerations, among which the human agencies of farming, grazing, and tree cutting are prominent. A rise of lake surface as the result of an upheaving deformation of the lake bottom is definitely excluded, because any resulting tendency to increase of area would be counteracted by increased evaporation, unless a change toward moister climate occurred at the same time. Human agencies are not accepted as affecting precipitation, in spite of the widespread popular opinion to that effect; but they are thought to diminish evaporation and to increase snow melting and run-off by small amounts, and thus possibly to increase stream inflow into the lake more than it had been decreased by evaporation. The chief cause of lake increase was provisionally held to be climatic. The increase of lake area between the surveys by Stansbury at a seasonal low-water stage in 1850 and by King at a seasonal high-water stage in 1869, when the lake covered 1,750 and 2,166 square miles, respectively, is interpreted to represent an increase of 17 per cent from a mean stage of 1,820 square miles at the earlier date to one of 2,125 square miles at the later date; and the areal increase of 17 per cent is estimated to represent a 10 per cent increase of rainfall. It is then concluded that while "the hypothesis which ascribes the rise of the lake to a change of climate should be regarded as tenable . . . it can claim no more than a provisional acceptance." A test of this hypothesis to-day is made difficult if not impossible, because the great shrinkage of lake area in recent years, usually ascribed to an increase in irrigation, has rendered the lake next to useless as a gauge of climatic change.

TRIANGULATION IN UTAH AND ARIZONA

The field season of 1878, lasting from July to December, was given to a task that few geologists would have felt themselves capable of executing: Nothing less than designing and executing a system of triangulation as the basis for a topographic survey of the plateau province, covering a part of the high plateaus in the north, the Colorado plateaus on both sides of the Grand Canyon farther south, and some of the adjacent basin ranges. The field party included, besides Gilbert in charge, Renshawe, Bodfish, Hillers, and a number of others. The geodetic work involved the connection of two base lines previously measured, one at Gunnison near the western base of the Wasatch Mountains, about 120 miles south of Salt Lake City, the other at Kanab, as much farther south still and close to the southern boundary of Utah; also the remeasurement of the latter base by an ingenious apparatus of Gilbert's own design, constructed under his personal supervision. A letter to the "Major" written before the party was subdivided, told of "having all the new boys drilled in barometric work," running levels to connect barometric stations, and setting up "aparejoes" or pack saddles and half converting a skeptical member of the party to their utility; then of sending off one division of the party with wagon and pack train southward to cross the Colorado below the mouth of the Grand Canyon and

there to turn eastward over the southern plateau to Moencopie on the line of the Echo Cliffs monocline; another with a 4-mule supply wagon to Kanab; and a third, of which Gilbert was a member, with pack train to set up signals on selected mountain tops, "the geodetic points of the main chain of triangles," and thence to Kanab, Moencopie, San Francisco Mountain, and back to Salt Lake City. Gilbert probably enjoyed this work, for it led him again over a region which he knew fairly well from previous campaigns, and which richly repays many visits; and moreover it called for an ingenious application of his mathematical capacity to field problems. But it led to no published report.

Lest some readers should fail to apprehend the manner in which the practical execution of a geodetic task departs from the theory of geodesy as presented in a textbook, an extract may be made from one of Gilbert's later letters to Powell, written at Kanab, October 18, 1878, regarding W———, who was in charge of a side party

> I am afraid he is in trouble. He let go one of the men I sent with him and hired in his place a man whom we know to be a horse thief just escaped from the sheriff. I have sent an Oraibe Indian to warn him, but fear he will be too late. I start myself in the morning. . . . Shall write you from Moencopie.

The promised letter from the last-named point was sent two weeks later:

> I have not yet seen W———. He is not very far away and I have sent a scout after him. He lost no horses by the thief in his employ, but on the contrary, liked him exceedingly and was much annoyed at my letter of warning.

The saying, "You never can tell," might have originated with this incident.

BAROMETRIC HYPSOMETRY

One of Gilbert's most aberrant interests concerned the principles involved in the determination of altitudes by means of barometric observations, a method which was much more extensively used in the earlier years of governmental topographic surveys than it has been in later years after the running of many lines of accurate levels and the geodetic determination of many stations by the Coast and Geodetic Survey as well as by the Geological Survey in nearly all parts of the country. Barometric measurement of altitudes was, however, an important element in Gilbert's scheme of triangulation in the summer of 1878, as is shown by his mention of drill in barometric observation in a letter above quoted. Indeed, the subject of barometric hypsometry as a problem of atmospheric physics must have occupied his attention for several years, judging from various entries in his notebooks. One of the most significant was made during the first season on the Powell survey. He wrote, September 6, 1875, while in the high plateaus:

> A barometric station on Musinia Plateau & another in Castle Valley would make a fine couple for an hourly scientific set. At the same time a set of vertical reciprocal angles would test refraction & check the determination of altitude.

Three days later a supposed relation of the double daily curve of atmospheric pressure to the expansion and compression of the lower air associated with the single daily curve of temperature was outlined in a series of tersely stated interrogative propositions:

> While the temperature curve is convex upward, the horary [barometric] curve must be descending; while it is concave, ascending (?). The horary curve is highest when the increase of rate of expansion is most rapid; hence when the temperature curve is concave upward & has the smallest radius of curvature (?). Which of these two propositions is correct? Is the horary curve a first, or a second differential of the temperature curve? If the second proposition be true, then the radii of curvature of the two curves change signs through ∞ at the same time. In any case the horary curve of the barometer is the true datum from which to determine the mean temperature curve of the air column used in hypsometry. . . . If one curve is a differential of another, then the integral curve will have a max. or min. for every point in which the dif. crosses the axis & for no other points. . . . It is probable that the hypsometric (as distinguished from the superficial) maximum of temperature occurs only 2 or 3 hours before sunset—or later than that directly indicated by thermometer.

It is not likely that a similar series of propositions is to be found in the notebooks of any other geologist on or off the survey staffs of those days or of any other days

Gilbert himself recorded many barometer readings in connection with his notes on structure and topography in the Henry Mountains and elsewhere, and it is to be presumed that they were utilized in calculating the thickness of stratified formations as well as in determining altitudes and constructing topographic maps. But he appears, judging from a passage on "Office work" in Powell's report for 1876–77, to have been troubled by errors in readings recorded at base stations by his assistants. The report states that—

Mr. Gilbert has made a critical examination and discussion of the barometric observations extending through the previous years of the work, for the purpose of determining the range of error, and of detecting as far as possible the source thereof. The result of this examination tended to show that one of the principal sources of error was inaccuracy in reading and recording, and for the purpose of eliminating these, [he] suggested a number of checks, of which the most important was the reading and recording of the two verniers of the Green barometer instead of a single one. The interval between the two verniers is of such length that their fractional readings are always different, and it is practically impossible to repeat the same error.

The chief outcome of Gilbert's attention to this subject is a remarkable essay, "A new method of measuring heights by means of the barometer," the preparation of which must have demanded much thought and labor during the later years of his connection with the Powell survey; it is published in the first annual report of the national survey (the second of the series) after Powell became its director. It made what musicians might call a prompt "attack" by the immediate announcement of the essence of the plan in the very first paragraph:

The change proposed in this paper is of a radical nature. Since the time of Laplace the formula he developed has formed the groundwork of all investigation and practice. . . . It is here proposed to abandon it entirely for the greater part of hypsometric work and to substitute a new formula involving none of his constants and having but a single element in common. The new element abandons both psychrometer and thermometer and employs the barometer alone (405).

The novelty of the method consisted, first, in the use of three barometers, two at stations of known altitude, as near to each other horizontally and as far separated vertically as possible, and the third at a point the altitude of which is to be determined; and, second, in the omission, as above noted, of all factors dependent on air temperature and moisture.

The essay as a whole is very carefully prepared. After the many complicating factors of the general problem are set forth, the new solution is first briefly stated with gratifying directness and then demonstrated with mathematical elaboration. The solution is in essence as follows: Given two stations, A and B, the altitudes of which are known, and a third station, C, the altitude of which is to be determined. Assume the air to be dry and of uniform temperature at 32°, and compute the altitudes B–A and C–A by the usual formula, thus gaining approximate results. Then solve the proportion in which the first three terms are known:

Approximate B–A : true B–A :: approximate C–A : true C–A

The value of the new solution was tested by comparing its results with those obtained by other methods at various places where the necessary observations were available; a series of tables is added to facilitate the calculations necessary in practical work.

The whole essay shows an immense amount of conscientious labor. It closes with a beautiful "graphic table" for the ready determination of a small correction dependent on two "arguments," which serve as abscissas and ordinates for the correction values represented by a series of curves. Gilbert's comment on this handy device shows the exceptional breadth of his treatment:

The graphic table is in some sense an experiment. The idea, indeed, is not novel, but it has not been widely applied. It appears to the writer that a similar plan might advantageously be adopted for the tabulation of factors dependent on two arguments, whenever the arguments are large as compared with the tabulated factor; or rather, whenever the number of digits used to express each argument is large as compared with the number of digits used to express the dependent factor.

Two comments of a rather mournful turn must be made on this exceptionally ingenious essay. First, it appears from a chapter on the work of other hypsometricians near the close of the essay, in which Gilbert refers to himself only in the third person, as if to make the statement more impersonal, that his method was not so novel as he had at first supposed. The new solution "was first advanced by the writer at a meeting of the Philosophical Society of Wash-

ington in May, 1877,[2] at which time he supposed it to be novel. He has since learned that he was antedated in publication by no less than two hypsometers, while it is probable that a third also anticipated him in the conception of the idea. Nevertheless the announcement of his method was not devoid of novelty, for he differed radically from his predecessors in his manner of developing and applying the idea" (548). Second, the expense attendant upon the maintenance of two base stations and the rapid extension of lines of level and chains of triangles shortly after Gilbert's essay was published worked against the adoption of his method. The labor that he put into it has been hardly repaid by the few measurements of altitude in which it has been employed. Twenty years later he wrote of this work:

The discussion . . . has been complimented, but the "new method" never got itself used.

DIURNAL VARIATION OF THE BAROMETER

Reference may be made at once to a return to one phase of these barometric studies in Gilbert's second address as president of the Society of American Naturalists in 1886, when he gave a closely argued discussion on "Special processes of research," with particular reference to graphic devices. Among various other problems, that of the diurnal variation of the barometer was taken up, and Espy, the early American meteorologist who gained after death the renown that he should have had while living, is credited with the idea which, judging by the notes quoted above, Gilbert himself appears to have reached independently while he was in the high plateaus of Utah in 1875; namely, that the double diurnal curve of total atmospheric pressure is a reaction from the single diurnal curve of lower atmospheric temperature. The idea is then tested with negative results, for while it appears satisfactory for certain regions, it fails in other regions. Thus Gilbert was led to revise the hypothesis and to conclude that "the relation of the temperature oscillation to the pressure oscillation is less simple than the one suggested by Espy."[3] In case other investigators take up this elusive problem, it should be noted that Gilbert's assumption of a constant rate of terrestrial radiation to outer space may probably be modified to advantage; for such radiation should vary between the wide limits of equality to indirect and weak insolation at the time of the early morning temperature minimum and equality to direct and strong insolation at the time of the early afternoon temperature maximum. It may be added that Gilbert's "special processes of research" differed about as greatly from the research processes usually employed by the body of naturalists whom he addressed, as his hypsometric method differed from the methods of geological research usually employed by the many readers who resorted for information and inspiration to the remarkable series of essays, published along with his barometric essay, in the annual reports of the national survey; but if the geologists and naturalists who naturally have their own methods of investigation wish really to know the Gilbert whom they all delighted to honor, they must study his methods of investigation in all their variety and in all their breadth and depth.

GEOLOGY OF THE BLACK HILLS

A report on the "Geology and resources of the Black Hills of Dakota," published by the Powell survey in 1880 under the apparent authorship of Henry Newton and W. P. Jenney, and including several supplements by experts on various subjects, deserves mention here because it exhibits two of Gilbert's well-marked, one might say most strongly marked, characteristics—generosity and acumen—for although a bibliographer, guided by the title-page only, would not give him credit as even part author of the volume, a biographer guided by the preface and the text must not fail to recognize his large share in the preparation of its first 220 pages, or nearly half of the whole. The story of the report is one of tragic interest. During the decades following the middle of the nineteenth century the Black Hills served for some 50,000 Sioux Indians as a cherished resort; indeed, some of them thought of it as a "final refuge" after their removal from farther east; but as gold was discovered in the hills in 1874 a fuller knowledge of their geology and resources was called for than several hasty reconnaissances of earlier years

[2] Bull. Phil. Soc. Washington, ii, 1877, 131–132.
[3] Amer. Journ. Sci., xxxiii, 1887, 452–473; see 472.

had supplied. An exploring expedition was therefore planned in 1875; and as its object was related "specially to the right and interests of the Sioux Indians," it was placed under the Bureau of Indian Affairs of the Interior Department—thus adding still another independent survey to a list of such organizations that was already too long. Jenney was appointed geologist in charge and one of his assistants was Newton, whose name has already been mentioned as a member of the Ohio Geological Survey during Gilbert's association with it. The expedition set out over the plains from Cheyenne, Wyo., in May, 1875, with two companies of Infantry and six of Cavalry, 400 men in all, and 75 wagons, and returned there in October.

Jenney promptly prepared a report on mineral resources, which was published in 1876 and republished in the volume here considered. Newton also prepared a report at once, and spent a considerable sum of his own money on it, but when presented to Congress for publication it encountered "a selfish and heartless opposition . . . springing from the fear that it would betray the inaccuracy of previously published descriptions of the geology of the region"; such was Newberry's view, expressed in a biographical sketch of his former student and associate at the beginning of the volume, of the rivalry and jealousy felt at that time by one governmental survey for another. By reason of the delay thus caused Newton returned to the Black Hills in 1877 to revise and extend his previous work, and especially to settle certain questions that had arisen while he was engaged upon his report; and while there he died of typhoid fever at the age of 32 years. His manuscript, chiefly in the form of a first draft, was left incomplete, with much erasure and interlineation. Its revision for publication was intrusted to Gilbert, who "therefore felt called upon to put himself, in a certain sense, in the place of the author, and make such emendation of form as seemed necessary to harmonize the whole." He "freely modified the language" in such ways as he conceived the author might have done had he survived; but except for a correction regarding the source of placer gold, the substance was carefully preserved.

There was, however, a section of the report on the "Structure and age of the Black Hills," indicated by title in Newton's table of contents but unwritten; and the whole of this section was therefore composed by the editor. "All the data are Newton's, and so are all the principal deductions except those derived from the drainage system"; these are Gilbert's and they are truly Gilbertian in their directness and lucidity. The main body of the report is a straightforward account of the geological formations involved, beginning with the Archean and ending with the Tertiary and volcanic rocks; and it is worth noting that Gilbert, who in his own reports had followed the rule in text and in tabular statement of beginning with the younger or higher formations and ending with the older or lower, here appears to have followed Newton's preference of beginning with the oldest formations and proceeding in time sequence, even to the point of tabulating some of the detailed sections upside down.

PHYSIOGRAPHY OF THE BLACK HILLS

The 20-page section that Gilbert wrote is essentially a discussion of physiographic geology. Although it still bears Newton's title, "Structure and Age of the Black Hills," its first sentence reads:

> It remains to consider the origin of the topographic forms which constitute the Hills. How and when did the plateaus, and peaks, and cliffs, and cañons, and valleys that make up its topography come into existence? The answer to the question is, in general terms, that the rocks were uplifted, and that being uplifted they were by the processes of erosion worn away and carved into the forms we have seen (203).

A restoration of the original uplift, as if unworn, was first attempted; thus reconstructed, it would have been a flat-topped dome, like the Uinta Mountains or the Kaibab Plateau, of oval outline, 70 by 40 miles in diameter, 6,600 feet above the surrounding plains, with lateral slopes having declivities of from 15° to 90°, and with a total volume of 4,200 cubic miles. The ideal form was, however, not realized, because much erosion must have been accomplished during its progress, and because "upon no plausible theory of mountains can it be supposed that their birth-labor is other than exceedingly slow. The earthquake is but the passing pang that records a unit of progress; it is only by the combination of many such units, separated

often by wide intervals of time, that the great result is accomplished" (210). If Gilbert thought it desirable in 1880 to write in a manner that seems to-day so elementary, it must be that physiographic geology has made great progress between then and now.

In consequence of long-continued erosion, partly during, partly after, the time of uplift, the ideal flat-topped dome has been greatly denuded and so far unroofed over an excentric area of 60 by 25 miles in its eastern half as to expose beneath the cover of the bedded rocks, originally nearly half a mile in thickness, the nearly vertical quartzites and schists of the Archean, trending northward or northwestward and including lenticular masses of intrusive granite. In this unroofed area the resistant quartzites form "abrupt, dike-like ridges . . . bristling suddenly in peaks" (52, 58), the granites form ridges, peaks, and pinnacles; and slates are excavated in valleys, some of which are wide enough to constitute "beautiful expanses of treeless and well grassed parks" (56). It may be noted in passing that these ridges and valleys afford a fine illustration of what has later come to be called topographic discordance with respect to the scarped edges of resistant overlying limestones. A further consequence of the long-continued erosion is that a series of weak "Red beds," 540 feet thick, lying in an inclined position around the margin of the dome, are now excavated in a "Red valley," known to the Indians as the "Race-course," from 1 to 3 miles wide, which runs conformably between the stripped slope of the underlying limestones, 650 feet thick, and an encircling monoclinal ridge of resistant sandstones, 300 feet thick.

The first of the two chief factors which determine mountain form, namely, the rock structure of the mass, having been sufficiently treated, attention is turned to the second factor, namely, the arrangement of drainage lines. Here Gilbert's treatment of stream development is much the same as it was in the Henry Mountains report. Distinction is made between the creeks which flow radially outward from the hills on the one hand, and, on the other hand, the two chief rivers of the district, both branches of the Cheyenne, which flow eastward across the southern and northern ends of the domed strata. The creeks are first described empirically, with relation to the dip of the domed strata; they are called cataclinal, following Powell's terminology. They are then described genetically with relation to their origin, and called consequent. Taken together, they are held to "afford a rare example of consequent drainage"; the encircling monoclinal ridge is cut by them 27 times. The two rivers, running somewhat irregularly into and out from the encircling monoclinal ridge at the two ends of the dome, are not classed as antecedent, as they might at first appear to be, but as superimposed, because the White River beds, the uppermost of the plains strata, are found to lie unconformably on the next lower strata, and to contain a bed of quartz pebbles at their base, derived from the Archean rocks of the hills; hence it is inferred that all the doming and much of the erosion were accomplished before the deposition of the White River beds; that those beds were spread unconformably over the worn-down margin of the domed strata; and that the two rivers took consequent courses on the unconformable cover; and later, as their valleys were incised, found themselves by superimposition more or less astride of the monoclinal sandstone ridge as it came into relief. The creeks of the unroofed Archean area might also have been classed as superimposed, as they transect the quartzite ridges in sharp-cut canyons with precipitous walls (59). It is interesting to note that, in connection with the unconformable deposition of the White River beds, the phrase, "base level of erosion" appears to have been used by Gilbert for the first time. His chapter closes with a large generalization regarding the work of the consequent streams:

> The cañons they have cut, and which appeal to our eyes as marvellous monuments of their industry, are the least of their results. Since their labor began they have demolished and removed one half of the entire mass of the uplift. Modest and feeble as they seem, it is their ambition patiently to toil on until no vestige of the Hills remains (222).

In two respects, Gilbert's analysis of the drainage of the Black Hills calls for extension. First, as with the little pairs of short subsequent valleys back of the "revet crags" of the Henry Mountains, so with the somewhat better developed pairs of subsequent valleys that, as branches from the radial consequents, are extended along the weak "Red Beds" around the

Black Hills, they are given no special name, although they are manifestly neither consequent, antecedent, nor superimposed. The same is true of the longitudinal and presumably subsequent valleys that follow the weak schists between the quartzite ridges of the unroofed Archean area. Perhaps it is because most of these manifestly subsequent valleys have no streams that they were given no systematic names. Second, mention is made of the disagreement between the crest of the original dome, which as structurally restored lay near its eastern side, and the present divide between the east-flowing and west-flowing consequent creeks, which lies 15 or 20 miles farther west. This is explained as the result of a displacement of the line of maximum uplift from its original position, as if the creeks were consequent upon the first uplift and, thus being located antecedent to the later and more eastward uplift, held their courses across it.

When the displacement began its summit was necessarily a divide or water-shed, from which the water flowed in all directions. In the absence of any disturbing cause this water-shed would remain as steadfast as the drainage lines all through the period of denudation. Knowing of no disturbing cause, we may assume that the existing water-shed in the Hills holds the same position as the original water-shed, and marks, therefore, the position of the summit of the uplift at the time of its inception. . . . It would appear that the portion of the uplift which took the lead at the start was afterward overtaken and exceeded by another portion, so that the present summit or axis of upheaval is not the original axis of upheaval (217, 218).

This conclusion appears to overlook the principle, announced in the Henry Mountains report, that steep slopes are eroded more rapidly than gentle slopes; and that an unsymmetrical divide between two streams must therefore be slowly pushed from its steeper slope toward its gentler slope. In a word, the shifting of divides by headwater erosion is not here considered; it is not mentioned even in connection with the statement that "in a few cases, [radial consequent] streams of considerable size have so far shifted their courses as to form unions with each other before escaping from the foot-hills, but such instances are rare"; nor is any special attention given to the longitudinal stream courses in the Red Valley by which such unions appear to have been effected. Here, as before, the moral to be drawn is plainly that, great as were the contributions which Gilbert made to the rational treatment of land sculpture, the subject was so little developed in his earlier years that even he did not develop it fully.

PERSONAL ITEMS

During the two-year period of these divers occupations, Powell's survey would appear to have been conducted in a very personal manner, if one may judge by certain entries in Gilbert's diaries. One for February 2, 1878, reads: "Powell still owes, unpaid salary, $217.50"; and three weeks later: "Major Powell requests me to wait until May 1st before receiving more of my pay. I have now received all of my salary for 1877 except $5.00 and none for 1878." The survey must indeed have been in a peculiar financial condition if it had to leave unpaid a balance of $5 due to a valued member, who surely thereafter deserved to be called a trusting as well as a trusted friend of the director. It is possible that these irregularities, not to say uncertainties, of payment prompted Gilbert at least to consider a change to university work, for in his diary for 1879 a summary for the year previous, during which he had reached his 35th year of age, includes the following record: "Visited Providence, R. I., to talk about Professorship, Mar. 25–27." In view of the diversion of a large share of his attention a few years later from research to administration, it is open to question whether geology did not suffer more by his retention of a place in Government service than by his nonacceptance of a university position. He would have made a delightfully inspiring teacher, for his happy manner when speaking at geological meetings in Washington was already exerting a strong influence over younger men, who were quick to recognize his mastery of geological philosophy; and he had moreover an extraordinary capacity for making difficult subjects easy by analyzing the successive steps and stating each one separately and clearly, so that it could be surmounted with facility. Surely no professor in the land would have presented his science more objectively, none would have attacked his problems in a more calm and dispassionate spirit, none would have presented conclusions more free from personal bias. But had he taken up university work he ought to have had a research professorship; and it does not appear that such a position was opened to him.

CHAPTER XII

THE UNITED STATES GEOLOGICAL SURVEY

THE CONSOLIDATION OF THE EARLIER SURVEYS

The rivalry of several national geological surveys in the seventies was not creditable to the departmental organization of our Government. King's Fortieth Parallel survey was for a time one of the competitors, but as its field work was completed in 1873, it did not thereafter duplicate the work of other surveys. There remained the Hayden, the Powell, and the Wheeler surveys, which fought it out to a finish. By good fortune, a brief statement by Gilbert describing the climax reached in 1877 is to be found in an account of the later organized National Survey which he contributed to Appleton's Annual Cyclopedia for 1885:

> The duplication of plans, and the rivalries associated with it, were recognized by Congress as seriously prejudicial to a work which all desired to see carried forward. Various proposals were entertained from time to time to abolish all but one of the organizations, and to give that one exclusive possession of the field, and, though none of these prevailed, the corps were greatly embarrassed by the uncertainty of their tenure. Each was seriously tempted to make haste in publication, at the expense of thoroughness, so as to enable Congress and the public to appreciate that work was actually being performed by it; and each was restrained by similar considerations from the formation of far-sighted, economic plans for its future work.

It was at this stage of the game that the National Academy of Sciences was called upon to suggest a way out: "Congress doubting its own ability to select from the three [surveys] the one best qualified to conduct the entire work finally appealed to the National Academy of Sciences, the official adviser of the Government in scientific matters"; the academy recommended the abolition of the then existing surveys and the creation of a single United States Geological Survey under the Interior Department. The recommendation was adopted in 1879, and since then the national development of geological science has gone on apace.

Gilbert was one of six members of the earlier surveys who were appointed to the new one with the title of "geologist" at a salary of $4,000 a year. It may be added that his salary always remained at that figure, except during the three years, 1889–1892, of his service as "chief geologist," when it was raised to $4,250, and except also during certain later years after his health had failed, when he worked intermittently on a $13 per diem basis. For the first two years of the organization, 1879–1881, when Clarence King, previously in charge of the Fortieth Parallel survey, was director, and when the field of work was the "public domain" which lay for the most part beyond the Mississippi, only an administrative office was maintained in Washington, while four field divisions were established elsewhere in charge of Emmons, with headquarters at Denver; Dutton and Gilbert, both at Salt Lake City; and Hague, at San Francisco. Gilbert's division was known as the division of the Great Basin, and Lake Bonneville, the abandoned shore lines of which he had come to know in the course of his work in Utah under Wheeler and Powell, was his first subject of special investigation; his work upon it is described in the next chapter. His own explanation of this assignment, made a few years later, was as follows: "At the time of the organization of the survey, it chanced that there was in possession of the writer a considerable body of unpublished material bearing upon Lake Bonneville, and that lake was therefore selected as the first individual subject of study" in the Great Basin province, with the expectation that other extinct lakes would be taken up later. But in the spring of 1881, Powell, succeeding King, abolished the several district divisions and created divisions by subjects, namely, the divisions of topography, general geology and economic geology, coordinate with the divisions of paleontology, physics, and chemistry; thereafter geographical divisions were used only as subordinate to subject divisions.

This change in organization is also referred to in Gilbert's account of the National Survey mentioned above. After describing the district subdivisions that King introduced, he goes on:

It was soon found, however, that although the districts were exceedingly large, each of them was too small for the satisfactory conduct of the most important investigations instituted within it. The demands of the work led to a practical abandonment of the lines of demarcation. It was also found that the attention of the geologist in charge of each division was so distracted by the variety of work he was called upon to supervise that his personal studies were greatly hampered. The junior assistants, exempt from the cares of administration, were able to push their special investigations far in advance of the complementary work undertaken by their chiefs and senior colleagues. The talents and acquirements that rendered the work of an individual most desirable, led, by qualifying him to direct the work of others, to a great diminution of personal accomplishment. The original subdivision of the work by geographic areas has therefore been in large part abandoned, and for it there has gradually been substituted a system in which the primary basis of subdivision is the nature of the work to be performed, and in which the body of the work placed under the direction of one assistant is not so large that his administrative duties make serious encroachments upon his time.

FROM SALT LAKE CITY TO WASHINGTON

It was probably with this happy idea of agreeably combining a moderate amount of administrative responsibility with a larger amount of individual scientific work that early in 1881, Powell called Gilbert from Salt Lake City to Washington, nominally "to complete his report on Lake Bonneville," and on that date therefore Gilbert's service began as Powell's closest adviser in a great and worthy task; but unhappily his share in that task, as well as in its extensions on which Powell from time to time embarked, soon required him to withdraw from work in the Cordilleran region; for although at about that date the work of the survey was carried beyond the "public domain" to which it was at first limited and extended into the older States, there was no corresponding increase in the appropriations. Western work already entered upon was sacrificed to a greater or less degree to new work in the East. The investigation of all the Pleistocene lakes of the Great Basin, as at first planned under King, had gone pretty far, for Gilbert reported soon after his return to Washington that, besides the three larger Pleistocene lakes, 25 of the smaller ones had been explored, although "it is probable that a still larger number remain to be examined;" [1] but no special account of the 25 smaller ones has been published. Under Powell the investigation of only the three larger lakes, Bonneville, Lahontan, and Mono, on which study was either begun or well advanced, was continued, and "the examination of the more southerly valleys . . . the study of the brines and saline deposits, and the elaborate measurement of post-Pleiocene displacements" were indefinitely postponed. The survey as a whole was doubtless stronger under the new director than it had been before, but Gilbert's opportunity for personal research along lines that he wished to continue was greatly curtailed. In his case at least, administrative duties soon made very serious encroachments upon his time. Who can say whether the net result for the science of geology was a profit or a loss? From the date of Gilbert's return to Washington in 1881, he was continually in close relations with Powell, who consulted him on matters of every kind, and he continually exerted a wise influence on the conduct of affairs during the formative period of the survey's history. From that time on, his own work suffered greatly from interruptions and distractions, and an important share of his thought was represented in the plans and assignments announced in administrative reports over the director's name instead of over his own. Like other loyal members of the survey, he recognized that the work of subordinates in a great governmental institution must go frequently, necessarily, and properly to the credit of those "higher up," and he uttered no word of complaint at the personal sacrifices thus called for.

DISTRACTIONS OF OFFICE WORK

An illustration of this subordination of personal credit to institutional advantage appears in the first annual report of the new director, the second of the series, for the year, 1880–81, which contains an elaborate discussion of the use of colors on geological maps; the general choice of colors there presented may well be credited to Powell, but the careful elaboration of the details was probably the work of Gilbert. He showed himself indeed so competent in many tasks that he was more and more called upon to guide things aright; and his own work suffered in conse-

[1] U. S Geol. Survey, Bull. 11, 1884, 9.

quence. His intention on returning to Washington was at once to complete the work on Lake Bonneville, but he was continually distracted by "various minor duties," as is shown by brief paragraphs in successive administrative records. Thus in the third annual report of the survey, 1881–82, Gilbert states that a part of his time has been occupied with (1) maps and engravings of the Lake Bonneville report; (2) the manuscript of the report; (3) the report on Lake Lahontan by Russell; (4) the literature of the Great Basin and of other continental basins; and (5) barometric hypsometry. As to the third of these tasks, Gilbert explains that the "Sketch of the geological history of Lake Lahontan," published on pages 195 to 235 of the same annual volume, was written by Russell in camp, but that at Russell's request "the paper was revised and edited by me, so that I share with him the responsibility of many of the details." In spite of the work implied in the five tasks above named, a significant postscript shows that they were often subordinated to other duties: "My own time has largely been occupied by various minor duties connected with the general work of the office." The fourth annual report, 1882–83, tells a similar story: "My own time has been largely devoted to duties of a general nature connected with administrative work of the central office." Hence the report on Bonneville, for the completion of which Gilbert was nominally ordered back to Washington, was continually delayed.

A western journey in the summer of 1883 must have been a welcome interruption of office work. Russell being at that time still engaged in studies that had been planned by the division of the Great Basin, Gilbert went out to see how the work was progressing. He left Washington early in June, spent a few weeks in the Bonneville area where certain supplementary observations were needed for his own report, and then after meeting Russell at Mono Lake, on the desert side of the Sierra Nevada near the mid-eastern boundary of California, went farther south to examine the geology of the Inyo earthquake district in Owens Valley. It may be inferred that special attention was given during the stop at Mono Lake to the great moraines of ancient Sierran glaciers and the deposits left by the expanded predecessor of the present lake; for the single example of a moraine that reaches the Bonneville shore lines does not clearly exhibit its relations to the lake deposits. Five days were afterwards spent in the mountains, where Mount Lyell was climbed; a few statements then made concerning forms of glacial origin will be quoted in connection with Gilbert's work in the Sierra Nevada, 20 years later.

On the return journey some days were again spent at and near Salt Lake City; and nearly a week was given for no recorded reason to a district in eastern Utah, where the Rio Grande Western Railway runs not far south of the great escarpment of the Book Cliffs, between the Green and Grand Rivers, which there approach each other on their way to unite in the Colorado. This must have been the occasion when Gilbert learned that the summits of the Henry Mountains, about 80 miles away to the southwest, are visible from a rise of ground a short distance to one side of the railway line; for eight years later, when the excursion party of the International Geological Congress that had been in session at Washington was making its western circuit, Gilbert had the excursion train stopped in the desert not far from Green River crossing while he led a small number of the more active members a rapid march to the view point and back.

The work of the following winter is stated in the fifth annual report of the survey, 1883–84, at first in indirect, third-person style:

> The writer has continued the preparation of his long-delayed memoir on Lake Bonneville, and has specially arranged a chapter of it, "The topographic features of lake shores," for publication in this volume. A portion of his time has been occupied with the discussion of the influence of terrestrial rotation on the character of river valleys and with the elaboration of a plan for the subject bibliography of geologic literature.

And then, in reporting to the director the suspension of the Salt Lake City office he adds, not a petulant complaint but, as if his feelings could not be wholly suppressed, a patient regret in the more courageous first-person style:

> While I recognize fully the considerations which led to the closing of this investigation of the Great Basin, and while the wisdom of your decision is unquestioned, I yet find myself unable to lay the work aside without the tribute of regret and the expression of a hope that it may some day be resumed by another if not by myself.

Thus saying farewell to it, he points out three lines of further inquiry: The brines of the Great Basin and their products, an economic problem; the records of extinct lakes in the southern part of the basin, a climatic problem; and the deformation of the Bonneville floor as

if because of the withdrawal of the lake water, a problem that would to-day be called isostatic. Curiously enough, the basin-range problem is not here specified. Over a year later, after the geology of the Appalachians had become his chief concern, he again referred to the problem of his preference and his regret on abandoning it, when discussing the post-Bonneville deformation of the Great Salt Lake Desert in an address, analyzed below, before the Society of American Naturalists. The real lesson of this part of Gilbert's life is that of loyal sacrifice: Powell needed his counsel in the conduct of a great undertaking, and he therefore gave up the work that he most enjoyed and stood faithfully by his chief.

Although this recital necessarily has a melancholy tinge, it should not be understood that Gilbert was unhappy in his return to Washington. He was devotedly attached to Powell, personally as well as scientifically, and was always ready to serve and to aid him. The satisfaction of renewing with "the Major" the close relations that had been previously established during five years of membership on the Powell survey must have been some compensation for giving up Bonneville, and the satisfaction must have been increased by his recognition of the many ways in which his chief's nature could be complemented by his own. Powell, although a thoughtful observer, enjoyed and excelled in administration; after his first brilliant work in western exploration, he gave most of his attention for the rest of his life to problems of organization; the establishment of the Bureau of American Ethnology and of the national Geological Survey were his great accomplishments in this direction. On the other hand, Gilbert, while fully competent to administer the responsibilities laid upon him as chief of a survey division, excelled his senior as a philosophical geologist, and must have been of immense service to the senior in the discussion and settlement of the numerous technical and scientific problems that arose as the survey expanded. He not only had a logical mind that led him to just conclusions, but also the happy faculty of presenting his conclusions in a manner that greatly promoted their acceptance; and for both reasons his opinions were greatly respected among his associates. All things considered, he must have found much in Washington to make up for his abandonment of the great investigational field of the West. In any case, when the director called him to the East, he dutifully put aside a cherished plan of further work in the Great Basin, and with self-denying devotion took up the tasks that were assigned to him.

THE GREAT BASIN MESS

Life includes small events among the greater ones; great organizations have smaller ones included within them. Hence it must be recorded that shortly after Gilbert's return from Salt Lake City to Washington in 1881, a notable little institution had its beginning within the survey under his leadership. It should be recalled in this connection that the survey included at its outset two very unlike groups of geologists; a smaller group which preferred and was rich enough to afford the metropolitan luxuries of a conventional civilization; and a larger group, the members of which were of limited means and were perhaps for that reason more at home in the cosmic freedom of simple surroundings. It need hardly be explained that Gilbert and his western associates, Johnson, Russell, and McGee, were members of the latter group; but it needs to be told that this quartette gave daily expression to their preference for simple living by eating a frugal noon meal together in one of the survey rooms, their gathering naturally being baptized after the scene of their previous explorations, the "Great Basin mess." This institution endured for a generation, and like other enduring things it had an evolutionary development. Its career included an early home-rule period and a later caterer-controlled period. In the first, each cenobite used to take his turn, week by week, at bringing in a basket carried on his arm a home-prepared lunch for the favored four. Wooden plates and paper napkins sufficed at first, as they could be burned when the meal was over; but a coffee pot with china cups and saucers was an early innovation. The success of the mess was such that new members were added from time to time, the formalities of election or rejection being brief but emphatic and effective. In time the numbers became so large that the caterer-controlled period set in, and lunch was eventually served by a professional expert in a room hired for the purpose across the street from the survey building.

The history of the Great Basin mess is inseparably associated with that of the survey itself, of which it was for 30 years a characteristic though sectional feature; and for the most of that time Gilbert was, when in Washington, the leading spirit as well as the senior member. How genial was his smile of welcome; how jovial were his stories and how joyous the laugh that left his friends happier for having been with him! His letters to members absent in the field frequently contained references to the mess and its fortunes. It was locally known for the good fellowship that the members enjoyed together, and it was widely renowned for the pleasure that an invitation to sit in with members gave to many a geological visitor in Washington. Melancholy indeed was the fate of such a visitor if, invited to the mess one year, he was not invited the year following when he was again in the Capital City. But that was the fate of few, for the mess was truly hospitable; its visitors, numbering two or three a week, counted up to a total of many hundreds. They included such men as Hermann Credner, E. D. Cope, G. M. Dawson, James Hall, Joseph Leconte, Emm. de Margerie, O. C. Marsh, Raphael Pumpelly, N. S. Shaler, and other geological personages. It may be well believed that the visitors were sometimes amused if not amazed at the wide range of personal and pungent remarks by which the lunch was flavored; wagers on the outcome of elections and congressional measures were frequent, the stakes usually being some choice dish of dessert. On one occasion, a member tabulated the qualities of the others, and rated Gilbert as zero in cheek, combativeness, diplomacy, verbosity, and vanity, but 100 in honesty and caution. It is not to be denied that good fellowship sometimes detained the mess members around the table after the end of the noon hour, thus endangering their good repute in the director's office; but as to that, it is credibly reported that a critic of the survey one day, seeing the director and his chief adviser playing tenpins in an alley near the office, snapped a photograph of them with a clock in the background, its hands pointing to a mid-afternoon hour, in evidence of the way in which leading geologists wasted their time.

With increase of membership and change to the caterer-controlled period, the mess outgrew its original simplicity, and none of its later arrangements compare in primitiveness to those of its first years. The little company of four then sometimes sat on rolls of bedding around a packing box, as if to keep up the pleasant impression of lunching in a Great Basin camp. On one occasion when they were thus grouped, Gilbert, vividly recounting a rare instance of horizontal refraction in the desert, became so absorbed in his narration that he tossed a well-picked chicken bone over his shoulder, as if the party were really seated in the sagebrush wilderness; and the bone haply struck an office messenger who entered the door at that very moment to summon the narrator to the presence of the director; the messenger returned forthwith, reporting that he had "'tracted Mr. Gilbert's 'tention." Of such are the joyous memories of long-gone years still current among the surviving few!

CHAPTER XIII

LAKE BONNEVILLE

GILBERT'S FIRST ASSIGNMENT ON THE NATIONAL SURVEY

It has already been noted that, when the consolidated national survey was organized in 1879 under the direction of Clarence King, Gilbert was assigned the study of Lake Bonneville, with headquarters in Salt Lake City. For reasons that are not explicitly stated, but which were probably connected with the completion of certain tasks that had been begun under Powell's survey, he remained in the East through the summer of 1879, spending part of the time with his family at Winchendon, Mass., and did not reach his western field until October. Then, as if to make up for a late start, he continued field work until stopped by stress of weather in the middle of January, 1880. Willard D. Johnson, a young topographer of exceptionally fine spirit, who thought much about the origin of the land forms that he surveyed, was Gilbert's first assistant, and was charged with making maps of critical localities. The intimacy thus begun between the two men was continued through many years of close relations. Gilbert went back to Washington in February, gave up his house in Le Droit Park in June, and then returned to Salt Lake City with his family, establishing his residence there and renting office rooms for his staff. I. C. Russell was chief assistant during the field season of 1880, and was later assigned under Gilbert's direction to the study of certain other extinct lakes, especially Lahontan, farther west in the Great Basin. McGee and others were members of the field parties from time to time.

Thus it appeared that Gilbert was to remain indefinitely in the West, and he began to establish personal relations with the Salt Lake community; for in January, 1881, he lectured on Lake Bonneville in "Independence Hall." But his own view of the situation was otherwise; he had written to King regarding his Bonneville studies on November 16, 1880:

> . . . I have no occasion to take the field again in person, but begin the preparation of my report upon the subject. For the present I can work to best advantage here, but when the field notes of the season, both my own and my assistants, have been elaborated and when the map drawing is well in hand so that it can be completed without my supervision, it appears to me desirable that I go to New York and Washington, so that I can have better library facilities and so that I can initiate the preparation of engravings for illustration.

It must therefore have been a satisfaction to him, in some respects at least, that, when King relinquished the directorship of the survey and Powell was appointed to it in March, 1881, his presence was needed at the central office "on duty supposed to be temporary," but which proved to be long lasting; so he returned to Washington in April and his family followed in June; and there, after the long delays referred to above, the Bonneville report was eventually completed.

EARLIER WORK ON LAKE BONNEVILLE

It is interesting to trace Gilbert's progress in detecting the essential elements of the Bonneville problem. The abandoned shore lines of the ancient lake had been known in a general way for many years before he reached the Great Basin in 1871. His first season there with the Wheeler survey was mostly spent to the west and south of the Bonneville area; but during the second season he saw much of the shore lines, and on August 10, 1872, when the extinct lake was still unnamed, the following exceptionally deliberate entry was made in one of his notebooks: "Theory, that the great lake whose bed we are travelling is a phase of the glacial epoch"; the leading word, "Theory," being inclosed in a penciled rectangle, as if to guard against mistaking the inference it introduces for a record of fact. Two days later, when some large piedmont gravel fans must have been in sight with the beach lines engraved upon them, the following significant notes were added:

> On the Mt. ["Goshoot" or Gosiute"] NW I can count 11 terraces. The gravel slopes made since the lake are a small item compared with those made before. The lake episode is in the history of these valleys a very recent one.

Hence, if the lake episode were both recent and brief, a long nonlacustrine period must have gone before it. In the Wheeler report, the prelacustrine, lacustrine, and postlacustrine intervals are given as roughly proportional to 50, 10, and 1. It was evidently during the prelacustrine period that the great amount of intermont aggradation, mentioned above in the account of the basin ranges, must have taken place.

Singularly enough, the warping of the Bonneville shore lines appears to have been detected in the summer of 1872 about a month earlier than the date of the above notes, when the wave-cut benches on the Oquirrh Range, 15 or 20 miles to the west, were in sight from the corresponding benches on the flanks of the Wasatch Range near Salt Lake City. A first record is here corrected by a second. The first reads:

My impression is that the upper beach is continuous & level and only depressed in the distance by the curvature [of the earth].

But later in the same day, after levels had been sighted across the depression between the two ranges and earth curvature and refraction had been allowed for, it was noted:

From this it appears that the beach near Camp Douglass [not far from Salt Lake City] is 76.5 ft higher than on the Oquirrh Range. The distance may have been a little underestimated & the refraction overestimated. The allowance for refraction—$^1/_7$ of that for curvature—may apply only at sea level.

Later measurements reduced the difference of altitude to a smaller measure, and closer study of the ground discovered a 50-foot post-Bonneville fault along the base of the Wasatch which had to be allowed for; but a difference of 22 feet still remained to be explained by warping. A problem of crustal warping was thus opened, to which Gilbert later devoted much thought and from which he turned aside with regret when other duties held him in the East.

A special phase of this same problem is alluded to in the Wheeler report, in which the name, Bonneville, was first proposed for the extinct lake. After an account of the desert plain on which the shallow water sheet of the present Great Salt Lake lies, attention was called to the position of the lake on the eastern part of the plain as giving "evidence of the novelty of the present relation of altitudes of different portions of the plain, which is far from an equilibrium. Nearly the whole present increment [of detritus] to the desert floor comes from beyond [east of] the Wasatch Mountains, and is deposited . . . on the eastern margin of the lake. Since the lake has no outlet, but parts with its surplus by evaporation, its area rather than its level tends to constancy"—a very neat point, that—"and as the eastern shore increases, the water will rise, *pari passu*, and encroach on the western" (66). The eccentric position of the present lake was evidently taken to indicate a recent warping of the area near the mountains, whereby the aggradation of that part of the basin floor had been in part counteracted. Reference was made on a later page to the probable deformation of the Bonneville beach, which was thought to be 300 feet higher in a southern arm than farther northeast; and it was noted that if future observation confirms these inferences, the deformed beaches "will have special interest as the record, in the middle of the continent, of undulations of the solid earth, produced at so late a geological date that we may presume them identical with changes now transpiring" (93). This topic was fully discussed in the final statement of the Bonneville problem and was made the subject of an important address on scientific method in 1885, as will be told below.

THE BONNEVILLE OUTLET

An overflow for certain stages of Lake Bonneville was early inferred because of the long maintenance of its surface at certain levels, as indicated by the strongest shore lines (Wheeler, III, 90); and a northward outlet was suspected by various observers from what was known of the general "lay of the land." This aspect of the problem had evidently been talked over with Powell; for at the opening of the second season of field work on the Powell survey, Gilbert appears to have been authorized to make a northward detour from Salt Lake City in search of the suspected point of Bonneville overflow, before crossing the mountains and plateaus to the east of the city on the way to the Henry Mountains, which were his main object of study that year. The outlet was thus in August, 1876, proved to be, as Bradley of the Hayden survey had

in 1872 suggested that it might be, at Red Rock Pass on the flat floor of Cache Valley, adjoining the Port Neuf Range of southern Idaho. The floor of the pass was reached on August 16 and was described in a notebook as "so flat that it is a marsh with a growth of wire grass and sedge" for 7 miles.

The red rock is only one of a number that are here *exceptionally* bared, in testimony to the stream that washed them in degrading this pass. Above the beach level the rock exposures are inconspicuous. If there was a current through here when the lake was at its full height, it must have continued until the outlet was deepened several hundred feet. There is no evidence yet of the direction of flow but there will be evidence if the beach is found not to continue to the north.

Later in the day, after an advance to a point which commanded a broad northward view, a cautious note was added:

I can neither affirm nor deny beaches, but I think they are absent. The valley northward *should* exhibit them as well as that at the south if it ever had them. . . . On the whole it is extremely probable that an outlet (and the last outlet) of Lake Bonneville was here.

In explanation of the outlet, as discussed in the field notes, the hypothesis was at first entertained that the highest or Bonneville beaches as they came to be called, were related to an outlet at some other point, from which it was transferred to Red Rock Pass by a crustal tilting; but a second hypothesis was framed the same day, according to which the lingering of the lake at its highest level, as indicated by the great Bonneville beaches, was explained by a small excess of the gradually increasing water supply over evaporation, so that the resulting volume of discharge would be so small that the outlet must be for a time very slowly degraded; but as the water supply increased more definitely and the outflow gained in volume, its channel would be rapidly degraded to the present level of the pass; then a decrease of supply, while still permitting a small volume of overflow, would practically be unable to accomplish further degradation; thus the lake would be held for a considerable period at the pass level and permitted to form the strong Provo beaches. A different interpretation was given later, when more value was attached to the resistance of the rocks encountered in the bed of the outflow channel or pass, as a cause for the Provo level being so long maintained.

During the following year, while Gilbert was working on irrigation problems he gave attention also to the ancient shore lines, and resolved the doubts felt at the beginning of the previous season concerning the Red Rock outlet; he wrote to Powell from Ogden, Utah, under date of October 9, 1877:

I should like to go by rail to Humboldt Wells and thence by horse down Steptoe Valley, to settle the last question about the outlet of Lake Bonneville. I have this year seen the whole northern border of the lake and have made sure that the only northern outlet was through Cache Valley. That outlet I have revisited and studied with more care than before, and I now think its phenomena all consistent with the hypothesis that there was no other outlet. Still I should like to go to Steptoe and make sure.

But this desired excursion was not made until two or three years later.

The establishment of Red Rock Pass as the outlet of Lake Bonneville was announced by Gilbert in the spring of 1878 in a short article[1] which led him into more of a controversy than he engaged in at any other time in his life. His claim of discovery was after the fashion of the time disputed by Peale, of the Hayden survey, who asserted in an article [2] bearing the same title as Gilbert's that the real outlet lay in a more open valley about 45 miles farther north, where the original level of overflow was higher than Red Rock Pass, and higher indeed than the highest of the Bonneville shore lines. Gilbert did not reply until two years later, after he had revisited the localities and satisfied himself on three essential points: The error of Peale's determination of certain stream terraces as lake shore terraces; the vastly greater age of the more northern open valley, which Peale had taken to be the outlet, than of the comparatively narrow and recent incision at Red Rock; and the absence of shore lines in the intermediate basin. The controversy went no further; there was no place left for altercation. A characteristic passage may be quoted from Gilbert's closing article:

[1] The Ancient Outlet of Great Salt Lake. Amer. Jour. Sci., XV, 1878, 250–259.
[2] Amer. Jour. Sci., XV, 1878, 439–444.

A careful reëxamination of the locality has convinced me that I was in error [as to the place of the outlet], and has led me to assign it a position two miles north of Red Rock. Dr. Peale placed it about 45 miles north of Red Rock, so that my new determination is nearer to his than my old was.[3]

Not a few less magnanimous geologists would have phrased this conclusion:

My old determination was much nearer to the true location than his was!

But perhaps there was a touch of mischievousness in Gilbert's magnanimity.

TWO HUMID EPOCHS

It was by these successive advances in the earlier years of Gilbert's western work on the Wheeler and the Powell surveys that approach was made to an explanation of the Bonneville problem which seemed compulsory; but thus far only a single humid period of lake expansion had been recognized. In view of the great interest of the problem and of Gilbert's acknowledged mastery of it, its further study was naturally given a leading place in a main division of the national survey as soon as it was organized, as has already been noted. Progress was then made more rapidly.

Evidence of the occurrence of two humid epochs of lake expansion separated by an arid epoch of lake contraction or extinction was discovered during the first season's work for the national survey in the winter of 1879–80; and was briefly referred to in Gilbert's first formal report to King, dated at Salt Lake City, October 1, 1880, when an account of the lake-floor sediments included the following passage:

It was already known that they consisted of marls and clays and sands, but no considerable section had been measured, and no constant order of sequence had been observed. It was ascertained last winter that the marls invariably overlie the clays and form a relatively thin deposit. At one locality a beach gravel was found immediately beneath them, and in such relation as to demonstrate that a very low stage of water had intervened between two high stages. This is a capital discovery, proving, as it does, that the humid epoch was interrupted by an epoch of dryness.

The similarity of Bonneville and Lahontan histories, next announced, although not confirmed in all respects by later investigations, must have furnished pleasant writing to the geologist at the Salt Lake headquarters and agreeable reading for the two-year director in the Washington office:

The discovery [of a dry epoch between the two humid Bonneville epochs] confirms in a most gratifying manner an independent conclusion of Mr. King's. Reasoning entirely from mineralogical facts and the necessary conditions of chemical reaction, that geologist was led to conclude that Lake La Hontan, the contemporary and [western] neighbor of Lake Bonneville, was first flooded for a long period, without overflow, and then, after an interval of desiccation, was refilled for a shorter period during which there was a discharge. The history of Lake Bonneville is based purely on stratigraphic and topographic data, and is identical in every determined particular. The basin was flooded for a long period represented by ninety feet of clay; there was then a desiccation, shown by intercalated shore deposits; and there was finally a second flood stage, represented by fifteen feet of marl. The fact of overflow is proved by the discovery of the channel of discharge, and it has been shown that the second epoch of flooding was accompanied by overflow. Whether the first epoch was similarly characterized has not been ascertained, but it is a significant fact that the deposits thrown down during those two epochs have a marked difference of composition. If a relation can be established between the clay and marl as indicative of continence and overflow respectively, the parallel will be absolutely complete.[4]

The ingenious hypothesis by which the anticipated relation was established in the following year is noted below.

In the meantime a letter to King, dated a month and a half after the official report, above cited, announced two important advances. First. "The evidence of a long dry epoch interjected near the end of the Bonneville epoch is no longer restricted to a single locality nor to a single phenomenon"; that is, in addition to the discovery of new low-lying sections in which gravels were found between the lake-floor clays and marls, certain high-level deposits were found, against which the later-formed beaches lay unconformably; and these deposits were therefore taken to represent the littoral phase of the lake-floor clays that were laid down during the earlier humid epoch, just as the apposed beaches represented the littoral phase of the lake-floor marls laid down during the later humid epoch.

---

[3] The Outlet of Lake Bonneville. Amer. Jour. Sci., XIX, 1880, 341–349.

[4] First Annual Report, U. S. Geol. Survey, Washington, 1880, 23–26.

Second. "The idea that the accented beaches lying between the Provo and Bonneville levels were formed by the lingering of the water during its fall from the Bonneville level to the Provo is completely exploded. Those intermediate beaches were formed in ascending order—the lowest first, the highest last—and all of them are older than the Bonneville beach." This important conclusion was based on sections in which it was seen that the higher members of the intermediate beaches were successively superposed upon the lower ones, while the lower-lying Provo beach was apposed upon or built forward from the sublacustrine slope of the lowest intermediate beach; and all of these beaches except the highest ones were found, at one place or another, to rest upon previously formed deposits which were taken, as above noted, to represent the littoral phase of the lake-floor clays formed during the earlier humid epoch.

A third topic of equal interest was not carried to so satisfactory a conclusion; this was the relation of the Lake Bonneville to the glacial period, concerning which Gilbert's theoretical surmise of August, 1872, has already been quoted. Analogy pointed strongly to the synchronism of these two similarly complex manifestations of past climatic changes, but the only locality at which glacial and lacustrine deposits were found in contact—namely, where the beautiful lateral and terminal moraines of Little Cottonwood Canyon in the Wasatch Range a few miles south of Salt Lake City, advances across the belt of lacustrine shore lines—"failed . . . to yield crucial evidence for which search was made, and practically afforded no contribution to the subject." [5] A more confident opinion was reached later, when the evidence furnished by moraines in the Mono Lake basin at the eastern base of the Sierra Nevada, and that furnished by the depauperization of molluscan fossils confirmed "the presumption derived from the recency and exceptional nature of the lakes and glaciers, that the two phenomena were coordinate and synchronous results of the same climatic change." [6]

BONNEVILLE CLAYS AND MARLS

Another matter which remains to be considered may be regarded as having received one of the most venturesome and most purely hypothetical interpretations that Gilbert ever published. It concerns the relation already alluded to between the lacustrine clays and marls as indicative of lake continence and lake overflow, respectively, a relation which Gilbert felt would, if it were established, render the parallel between Bonneville and Lahontan "absolutely complete." To appreciate the offered interpretation it must be understood that the Bonneville clays and marls did not differ greatly in composition, but that one merely contained more argillaceous and less calcareous material than the other; and that both deposits might therefore be regarded as having been supplied by land-derived detritus of the same constitution, provided that a reason could be found for distributing the different constituents of the detritus in different proportions in the off-shore lake waters of the two humid epochs. It must also be noted that sedimentation experiments were made with the lake-floor clays, which indicated a five-fold more rapid settling in fresh water taken from an inflowing stream of to-day—City Creek at Salt Lake City—than in brine taken from the present lake. Whether the settling was due to a chemical reaction between the salts of the brine and certain salts dissolved in the streams that washed the sediments into the ancient lake—like the reaction employed for the clarification of certain turbid rivers for city water supply, the Mississippi water at St. Louis, for example—does not appear; but the result is peculiar in view of the generally accepted experimental conclusion that the salt of sea water accelerates sedimentation. In this connection reference may be made at once to the final monograph, in which it is said that the water of Bear Creek, which enters the basin farther north than City Creek, precipitated clayey sediments as rapidly as the brine of Great Salt Lake; thus rendering the conclusion previously reached somewhat uncertain. Citation may also be made here of a footnote in the final monograph which states, in view of sedimentation experiments that had been made by various observers and that came to Gilbert's attention in Washington: "It is not to be supposed that the sodium chloride and other constituents of the Salt Lake brine *retard* the precipitation of sediments," but "that they promote it less than the mineral constituents of City creek water."

[5] Second Ann. Rept. U. S. Geol Survey, 1881, 189.

[6] Monogr. I, 1890, 315.

With the conclusions of his experiments in mind, Gilbert made a number of hypothetical assumptions of greater or less plausibility in explanation of the supposed relation of lake continence to the deposition of the heavy clays, and of lake overflow to the deposition of the thin marls. The assumptions may be presented in two groups. Those of the first group concerned the composition of the lake in its earlier and later epochs of expansion: The lake waters in the earlier humid period having no outlet must have been saline; during the intermediate epoch the lake must have been evaporated to dryness and the resulting salt beds were buried under inwashed detritus; and the lake of the later epoch must have contained fresh water because it failed to redissolve the buried salts of its predecessor and because its own stages of no overflow were so short compared to its stage of overflow that its waters could not become saline. It may be interpolated that good support for the possibility of this group of assumptions was later found by Russell in his studies of the Lahontan area. The assumptions of the second group concerned the effects of lake-water composition on the deposition of inwashed sediments and are that: the processes of sedimentation must have been delayed by the salinity of the earlier Bonneville waters; and hence that a large share of its inwashed sediments must have been swept offshore into the body of the lake before they settled to the bottom, where they formed the relatively thick lake-floor clays; but that sedimentation must have been accelerated by the fresh water of the second lake, and hence the clays must then have settled nearer the shore and a larger proportion of calcareous material must be found in the relatively thin lake-floor marls. Conversely, the shore deposits of the later lake should show relatively heavier clay deposits than those of the earlier lake; but this consequence of the hypothetical assumptions can not be tested, because of the prevalent coarseness of the deposits near the shore in both the earlier and the later lakes.

The hypothesis, as thus elaborated, explained very well the facts that it was made to explain, but it did little more; it embodied, however, a group of inferred consequences—the buried salt beds and their shroud of sediments laid down in the arid epoch—the actual occurrence of which would give strong support to the hypothesis if they could be discovered by borings in the lake floor; but they have not yet been discovered. Even the gravels intercalated between the clays and the marls were not found at a less altitude than some 200 feet above the present lake; and borings later made in the Lahontan sediments failed to discover any purely saline precipitates; hence it must be inferred that if complete dessication took place, the inwash of muddy detritus during the arid period rendered the precipitates impure beyond the point of identification. Certain parts of the Bonneville history therefore remained unproved.

It may be well understood that so logical a thinker as Gilbert recognized the unproved elements of his hypothesis, for directly after announcing the above-stated explanation of the clays and marls he added:

> On the whole, the theory that the lake became fresh by desiccation finds too little positive support to entitle it to unreserved acceptance, but it is contradicted by no single known fact, and may therefore be considered to hold the position of a plausible working hypothesis.[7]

It is, indeed, not improbable that many readers of the Bonneville monograph came to have a greater measure of confidence in this "plausible" chapter of the lake history than Gilbert had himself; for when he finally summarized "the general history of Bonneville oscillations" and confidently announced the occurrence of the yellow clays and the white marls of the two lacustrine epochs as well as the complete desiccation of the lake waters during the intermediate arid epoch, he added that he was "practically without information" as to the degree of desiccation attained in the arid epoch, and he qualified the explanation by which the clays and the marls were differentiated with the halting phrase: "If it be true that the water was so constituted . . ."[8] But in spite of these guarded phrases, a computation, which "under this postulate indicates that the first high-water epoch was not less than five times as long as the second," was given graphic representation by a curve; and it is very probable that the unqualified character of such representation, taken with the ease of apprehending its intended

[7] Contributions to the history of Lake Bonneville. 2d Ann. Rep. U. S. Geol. Survey, 1881, 169–200; see p. 180.
[8] Monogr. 1. 1890, 260, 261.

meaning as compared with that of an explanatory text, have contributed to give a greater measure of certainty to the published interpretation of Bonneville history, as it is generally understood among geological readers, than Gilbert himself, with his exceptional capacity for balanced judgment, actually intended.

PRELIMINARY REPORTS ON LAKE BONNEVILLE

Progress in the discovery and the interpretation of the facts concerning Lake Bonneville having now been outlined, an account may be given of the reports in which the history of the lake was published. It should be here noted that field work had been substantially completed in 1880, for on November 16 of that year Gilbert wrote to King from Salt Lake City:

> The data for a final map of Lake Bonneville are now complete. Every part of the peripheral coast has been seen by some member of the party, and all the principal islands have been determined. The altitude of the highest water-line has been measured by spirit level at five new points and a good series of barometric observations has been made for its determiuation in the southernmost Escalante region. The difference of altitude between Bonneville and Provo beaches has been measured by spirit level at twelve points. Local maps have been made of seven different groups of wave-formed bars and at each of these a measured profile has been made for the purpose of exhibiting the inter-relations of the strongest water-marks of the series. The comparative study of these profiles is believed to have an important bearing on the question of the origin of certain of the beaches.

The time for office work and report writing had thus been reached. Yet so many and so absorbing were the distractions by which Gilbert's attention was turned from the Bonneville problem after his return to Washington in 1881 that 10 full years elapsed between the completion of field work and the appearance of the famous Bonneville monograph.

The contents of the monograph were foreshadowed by several preliminary statements. The earliest was in Gilbert's first administrative report to King, dated at Salt Lake City, October 1, 1880, and was so attractively phrased as to awake an expectant interest in the fuller statement that was to follow. Two paragraphs from it may be quoted:

> The Great Salt Lake Desert and a congeries of valleys connected with it were filled with water at a period so recent that the vestiges of the flood are little impaired at the present time. The sea cliffs that were covered by the dash of the ancient waves are sea cliffs still, though they stand a thousand feet above the present level of Great Salt Lake. The bars and beaches of sand and gravel that were built by the ancient currents are furrowed here and there by the rains that have since fallen on them, but they are furrowed only, and not destroyed; and the imagination is not strained to fill the gaps and restore their full contours. The fine silt that settled quietly in the deeper waters still forms the floors of the valleys. To the geologist accustomed to speak familiarly of millions of years, it is the veriest yesterday when all these things were wrought; nor can any one who stands on the quartzite shingle of one of the old beaches, and contemplates the rounded pebbles, gleaming with the self-same polish they received when the surf laid over them, fail to be impressed by the freshness of the record.
>
> There is a topography of the land and a topography of the water. The forms of the land are sculptured by the beating of the rain and by the flow of rills, and creeks, and rivers, and they have peculiar characters accordant with their origin. The forms of the beds of lakes and oceans, and especially the forms of shores, are sculptured by the sway of waves and currents, and are distinguished by characters equally peculiar. All the hills and mountains above the shore line of Lake Bonneville bear witness of the play of subaerial agents, while below that line the slopes betray their subaqueous shaping. There is a trenchant line between them, and their peculiarities are beautifully contrasted. A careful inspection, however, shows that subaqueous characters are superimposed on subaerial characters. The forms belonging to the dry land are continued down past the shore line, and the sculpture of the lake has been superficially impressed on them without entirely obliterating them. It is thus made evident that before the epoch of the lake, the land it covered was dry, just as it is now. The lake had a beginning as well as an end. It came, it lingered long enough to make an unmistakable record, and then it departed as it came.

The expectations excited by the brief summary from which the preceding paragraphs are quoted were well satisfied by the longer account published under the title, "Contributions to the history of Lake Bonneville," in the first one[9] of the many handsome and instructive annual reports by which Powell's administration of the survey was characterized. The treatment here given to the problem is concise and direct rather than argumentative, and thus places its readers promptly in possession of the essential elements of Bonneville history. The succession of

[9] 2d Ann. Rep. U. S. Geol. Survey, 1881, 169–200.

events was made clear by a diagram and explanatory text, in which the whole story was epitomized: After a very long prelacustrine arid period, during which a great erosion of the surrounding mountains was accomplished and large piedmont alluvial fans were formed, came the first lacustrine epoch of relatively long duration and moderate humidity, causing a rise of the lake to a high level but without overflow for its waters, which therefore remained saline; the shore deposits of this epoch are not now decipherable, but the bottom deposits are represented by 90 feet of yellow clay; then came an intermediate arid period causing the disappearance of the lake, the deposition of its salts, and their burial under inwashed sediments; next, the second lacustrine epoch, of shorter duration and more pronounced humidity, causing the gradual rise of the lake and the formation of an ascending series of superposed shore terraces until the Bonneville level was reached, 90 feet above the highest record of the first humid epoch; thereupon overflow took place at Red Rock Pass, and the outlet channel was rapidly cut down in the weak material first met, but when the lake had been lowered 365 feet a body of resistant limestone was encountered in the channel bed and further lowering was practically arrested; the lake then long remained at the level where the strong Provo beaches were formed, until the desiccating climate of the present postlacustrine epoch set in and the great sheet of water was reduced to about its present small dimensions. Thus the results of long-continued field studies and of much reflection upon them were simply and compactly summarized.

In the illustrations of this report the abandoned shore lines of Lake Bonneville were for the first time revealed to distant readers in all their marvelous magnitude and distinctness. One plate, however—a woodcut which appears to have been misinterpreted by the engraver—is curiously erroneous in representing a large pre-Bonneville alluvial fan with its apex built up against the mountain slope a little to one side of the valley from which its detritus is derived, leaving the other side of the valley too far removed and exposed to too low a level. A late chapter treats the relation of lake history to mountain building and presents a most edifying discussion of catastrophic and uniformitarian views; it was here the belief was expresssed that "the Wasatch range, the greatest mountain mass of Utah, has recently increased in height and presumably is still growing." Reference to this chapter will be made again in a later discussion of the basin ranges.

THE TOPOGRAPHIC FEATURES OF LAKE SHORES

A chapter from the final Monograph of Lake Bonneville on "The topographic features of lake shores" was printed in advance in the fifth annual report of the survey (1884), and gave delight as well as information to many readers; it furthermore made clear Gilbert's predominant interest in physiography as contrasted to historical geology, and it developed in abundant detail the thesis earlier stated in the Powell-like phrase: "There is a topography of the land and a topography of the water." All manner of lake-shore forms were described and explained with a fullness and a clearness that were both satisfying and gratifying, and that contributed greatly to confirm the awakening conception of land sculpture as a worth-while study to which geographers as well as geologists must give heed. The treatment throughout was thoroughly Gilbertian in its breadth and deliberation, especially in the pages devoted to the discrimination of lake-shore features from imitative features of other origins.

Two peculiar features, the V terrace and the V bar, for which, the text states, "no satisfactory explanation has been reached," were really better understood than that phrase would imply; for although the cause of the eddying currents to which they may with much confidence be ascribed was not discovered, the responsibility of such currents for the V-like forms appears to have been clearly understood, witness the statement:

> The formative currents must have diverged from the shore at one or both the landward angles of the terrace, but the condition determining this divergence does not appear.

Yet although a reasonable origin was thus found for these peculiar forms as well as for all others by which normal lake shores—that is, shores of lakes that have not been excavated by glacial action—are characterized, they are all treated as ready-made products; the earlier

stages of development through which they must have passed as well as the later stages through which they would have passed, had the shore processes continued to act for a longer period, are as a rule not emphasized; the treatment was explanatory but not evolutionary.

In one respect the treatment of lake-shore processes is particularly instructive as illustrating the rightful place of violent agencies in uniformitarian geology; namely, the relation of storms to the production of shore features. This is introduced by reference to the action of river floods. After an explanation of the manner in which a running stream does its work, it is shown that a flood gives to a stream—

a transporting power scarcely to be compared with [that is, immensely greater than] that of the same stream at its low stage, and it gives to the exceptional flood a power greatly in excess of the normal annual flood. Not only is it true that the work accomplished in a few days during the height of the chief flood of the year is greater than all that is accomplished during the remainder of the year, but it may even be true that the effect of the maximum flood of the decade or generation or century surpasses the effects of all minor floods. . . . In littoral transportation the great storm bears the same relation to the minor storm and to the fair weather breeze. The waves created by the great storm not only lift more detritus from each unit of the littoral zone, but they act on a broader zone and they are competent to move larger masses. . . . It follows that the habit of the shore . . . . is determined by and adjusted to the great storm.

THE BONNEVILLE MONOGRAPH

It had been planned that the first monograph of the national survey should be an account of the Comstock lode at Virginia City, Nev., by Clarence King; but when that versatile geologist gave up the direction of the survey and turned his attention more to business affairs, he appears to have given up the monograph also, and the first number of the series was thereupon reserved for Gilbert's report on Lake Bonneville. But so greatly was his work upon it delayed by office duties, as will be told on following pages, that it had been preceded, when at last published in 1890, by 15 other monographs, among which was Russell's Lahontan. Its contents have been sufficiently indicated by the preceding account of the observations and inferences which it summarizes, so that no abstract of it is here necessary; but it may be characterized as a whole. It represents, in the first place, Gilbert's most important, longest-continued and fullest published investigation. His early Wheeler reports were fragmentary in various respects; although the problems then encountered were numerous and novel, the field observations had been hurried and the conditions of publication were not attractive. The Henry Mountains report for the Powell survey, although a satisfying effort to its author, represented a relatively brief period of field study in Utah and an extraordinarily short period of writing in Washington. Studies of Niagara and the Great Lakes, to be reviewed on later pages, were continued intermittently for a good number of years, and were reported upon in several admirable papers, but the whole of that fine story was never brought together in a single volume. Of two later efforts made by Gilbert to return to the study of the basin ranges, the first, in 1901, was almost fruitless as far as publication is concerned, and the second, undertaken shortly before his death, is represented only by an unfinished essay. Between the two efforts he did a remarkable piece of work on the distribution of mining débris in California, which almost rivaled his work in the Great Basin. The Bonneville monograph is the greater study; it is Gilbert's masterpiece.

In the second place the Bonneville report is by far the most thorough study of a large quarternary lake in a now arid region that has yet been made in any part of the world, and as such it sets a high standard up to which any later studies must attempt to rise. It is deliberate, thorough, compendious, both as to the record of observed facts and as to their theoretical interpretation. In some cases the discussion is almost too deliberate, as Gilbert himself seems to have sensed. He wrote in June, 1888, to a correspondent:

In my Bonneville report I am discussing the correlation of the lake history with the ice [glacial] history in a very full and I fear in a somewhat labored way. I try to bring up all the convergent lines of evidence and leave no stone unturned.

But be this as it may, the discussion of successive stages of Bonneville history expresses the extraordinary balance of mental judgments in which Gilbert was, as has been well said,

probably unsurpassed by any geologist of his time, or, as may be better said, never surpassed by any geologist of any time, for no geologist was ever his superior in that faculty, so invaluable in his highly speculative science. His power of analysis, as here represented, is admirably patient and impartial, and his style of exposition is so clear that the reader, overlooking in the lucid text the laborious search for the facts in a half or wholly desert region and the many tentative and alternative interpretations of preliminary study, is almost tempted to regard his final conclusions as obvious. The earlier pages of this chapter, in which the gradual growth of the conclusions is sketched, may lead to a better understanding of the years of work that they involved.

The illustrations of the monograph, many of them from expressive drawings by the unrivaled hand of Holmes, add greatly to the value of the text. They bring out sharply the prevalent simplicity and uniformity of the well-carved basin-range slopes above the highest lake level, as the result of normal subaerial erosion at so far advanced a stage of dissection that differences of rock structure are for the most part masked under a well-graded though thin sheet of creeping waste, and they thus show the striking contrast between the slanting and divergent profiles above the Bonneville shore line and the level and parallel lines of lake beaches on the lower slopes. The map that accompanies the volume profits from the experience of the artist and the lithographer on the corresponding map in Russell's previously issued report on Lake Lahontan; that was good, but the Bonneville map is still better in its colors and its expression of relief.

The delay in the appearance of the monograph was therefore in some respects advantageous, for it not only permitted the utilization of certain results gained by Russell in his study of Lake Lahontan, the field on which work was begun after that on Bonneville was completed; it also insured the seasoning of all explanatory discussions. But in another respect the delay in the appearance of the Bonneville monograph was disadvantageous. It followed so many excellent predecessors that reviewers, accustomed to a high standard of the survey volumes and already fairly well informed on Bonneville history by earlier partial reports, rarely gave the merited attention to the volume when it finally came out. Thus a writer in Nature, after briefly transcribing an outline of Bonneville history, concludes with not even half a loaf of commendation, but with what can only be called a peroratorical sandwich composed of a layer of sincere praise between two slices of plain speaking, the upper slice intimating that the author is sometimes too wordy and the lower slice complimenting especially the artists and the printer:

> The author errs occasionally on the side of prolixity, but he brings together so much valuable information that the book will be indispensable to all who wish to study the history and phenomena of lakes and inland seas. We lay it down with a deep sense of gratitude to him for the loving labour which he has evidently bestowed on this memoir, and will only add that, high as the standard already attained by the American Geological Survey may be, this monograph, especially in the work of the printer and in the number, interest and excellence of the illustrations, more than attains to it.

Two comments by Gilbert himself may close this chapter. He wrote to a friend in March, 1891, shortly after the monograph had appeared:

> I have been interrupted by a reporter. He interviewed me today on Lake Bonneville and came in this evening with his report for me to revise. He says that he has sold it to the N. Y. Tribune and it will probably be telegraphed tonight to appear in tomorrow's (Thursday's) paper. It strikes me as very comic that what I found out years ago should be sent to N. Y. by telegraph instead of mailing the MS. But the reporter never heard of it before, nor have the readers of the Tribune.

One can imagine the merry chuckle of the writer as he penned those lines.

A quarter century later Gilbert wrote to his son in the West:

> Have been reading my Bonneville report and find I have forgotten a lot of things I knew when I wrote it

Many readers would say the same thing after the lapse of 25 years, for they also have probably "forgotten a lot of things" that they understood when they read the great monograph, but they will never forget the monograph itself, and in particular those who received a copy of it, with their name written on a flyleaf in Gilbert's own hand, will never forget the pleasure they had in receiving it. Its publication was a great event in the history of American geological science.

CHAPTER XIV

INCREASING SCIENTIFIC RELATIONS: 1881–1890

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

The decade, 1881–1890, was marked by an increase of Gilbert's scientific relations in many directions; he was, so to speak, "discovered" by the scientists of the country during this period, and they were not slow to show their appreciation of his fine qualities. As in the previous decade, he was a frequent speaker at the meetings of the Philosophical Society of Washington, which he served as secretary from 1883 to 1886, as vice president from 1887 to 1891, and as president in 1892, when he delivered a notable address to which reference will be made in the account of that period. The subjects that he presented before this society were as a rule of a general nature; they included a "Graphic table for computation," in 1880; the "Response of terrestrial climate to secular variations of solar radiation," in 1883; the "Diversion of water courses by the rotation of the earth" (mentioned again below); the "Problem of the knight's tour" and a "Concrete problem in hydrostatics," in 1884; "Graphic methods of research" (probably an abstract of his second presidential address before the Society of American Naturalists as noted below); "Statistics of the society since its foundation" and "Stages of geologic history of the Sierra Nevada," in 1887; and the "Soaring of birds," in 1888. The last subject had very likely been suggested by observations during a voyage across the Atlantic and back in the summer of that year; that his treatment of it was not superficial may be judged from the following extract:

> After a discussion of various qualifying factors, it was stated that when the orbit of the bird [soaring with outstretched wings] is circular, and lies in an inclined plane rising toward the wind, and when the horizontal velocity of the air diminishes uniformly from the highest point to the lowest point of the orbit, the velocity gained by the bird in making the circuit is equivalent to the differential velocity of the highest and lowest layers of air traversed, multiplied by $\frac{\pi}{2}$ into the cosine of the angle of inclination of the plane of the orbit.[1]

A REVIEW OF WHITNEY'S "CLIMATIC CHANGES"

The subjects of two others of the above communications, "Response of terrestrial climate to secular variations of solar radiation" and "Stages of geologic history of the Sierra Nevada," both very briefly abstracted in the society's Bulletin, appear to have been treated more fully in a critical, not to say controversial, review of Whitney's "Climatic changes of later geological times,"[2] a review that deserves reading still to-day, as well because of its closely reasoned quality as because of the corrections it provides for certain very questionable conclusions announced in that work. Whitney's leading idea was that a progressive weakening of solar radiation through geological time was responsible for the climatic changes by which former glaciers had been diminished and former lakes had been desiccated. Gilbert took issue with the principle here invoked, and argued that each increment of 4½° C. in mean annual temperature in the past as a result of stronger solar radiation would not only cause increased precipitation, but would also "double the conjoint power of evaporation and melting to remove precipitated snow"; that the glacial period must therefore have had a lower temperature than now; and that the postglacial desiccation of certain lakes in arid interior regions had resulted from a rise, not from a fall of temperature.

> Excessive aridity, therefore, as well as excessive humidity, is caused by solar heat; and every increment of solar radiation tends to magnify the contrast between moist regions and dry regions, making the moist moister and the dry drier.

[1] Science, xii, 1888, 267, 268.
[2] Science, i, 1883, 141–142, 169–173, 192–195.

The reasons adduced in support of the important principle involved in this quotation merit careful examination in the original review, which then proceeds to correct another error. Whitney had explained the auriferous gravels on the western flanks of the Sierra Nevada in California, where he had been for several years State geologist, as the deposits of larger rivers during a former time of higher temperature and greater rainfall, and had also explained the narrow canyons by which the gravel-covered uplands are now trenched as the work of the diminished successors of the former larger rivers. Gilbert reversed this explanation, arguing that, had the Sierran highlands remained unchanged in attitude, as Whitney assumed, the change from deposition to erosion would indicate an increase, not a decrease, of river volume; but he goes on to show that the attitude of the Sierran highlands has not been unchanged, and explains that their present altitude has been lately acquired by slanting uplift after they had been reduced to low relief in a former period of erosion; this part of the review has already been referred to in connection with Gilbert's views on the basin ranges.

The review closes with the nearest approach to severity of treatment that is to be found in any of Gilbert's writings.

> If a rise of temperature is not favorable to glaciation, if a fall of temperature does not make deserts drier and if river terraces are not indicative of waning precipitation, it might seem that our author's theory is badly off; but the case is not hopeless. The paleontologic evidence, and the doctrine of the dissipation of solar energy remain; and if he will now devote himself to the investigation of the glaciers that are known to have recently increased, to the dry countries in which civilization and wealth have supplanted barbarism and poverty, and to the rivers that are engaged in filling up the valleys they once excavated, he may yet find in recent history the evidence that he seeks of secular change.

The competent handling of meteorological problems in the discussion abstracted above shows not only a wide range of reading on Gilbert's part, but, what is much more important, a deep penetration of thought. This quality is shown again in an attempt to calculate the percentage of correctness of tornado predictions that were made a year later; but the article [3] when published contained so many typographical errors that Gilbert lost all interest in it. On the other hand, the severity, not to say asperity, of treatment found in the last quotation from the review of Whitney's volume is altogether exceptional in Gilbert's writings. His usual form may be fairly characterized in the terms in which he described a volume of essays that he admired:

> The style is peculiarly genial and entertaining—a merit unfortunately rare in the writings of modern geologists. . . . In the whole collection there is nothing polemic, nor anything that could even be called controversial. Attention is never directed to an error, except as the merest incident to pointing out that which is true. No words are given to the censure of others, but many to their praise.[4]

THE AMERICAN NATURALISTS AND THE AMERICAN ASSOCIATION

The American Society of Naturalists made him its president for two successive years; his first address before them, given in Boston in December, 1885, was a notable deliverance and brought him so admiring an acquaintance of many biologists who had previously known him little more than by name, that they immediately elected him president for a second term. The address, the subject of which was "The inculcation of scientific method by example," is reviewed in a special section below. His address the following year on "special processes of research" has already been alluded to.

In the early months of 1884, Gilbert took up the study of German, but did not carry it far. He attempted French after returning from a trip abroad in 1888 but did not continue it long. In the summer of 1884 he attended the Montreal meeting of the British Association for the Advancement of Science, the first colonial meeting of that important body, and presented a plan for a subject bibliography of North American geology. His presence 10 years earlier at the Hartford meeting of the American Association of similar name, which he commonly abbreviated to "A^3 S" in his notes, has already been told. No record is found of his presence

[3] Amer. Meteorol. Journal, 1884, 166-172.
[4] Review of Geikie's "Geological sketches at home and abroad." Nature, xvii, 1885, 237.

at its later meetings until 1885, when he acted as secretary of section E, geology and geography, at Indianapolis, gave an account of the old shore line of Lake Ontario, and reported the sectional proceedings anonymously in Science. He attended the Buffalo meeting of the Association the next year, and spoke on "Niagara Falls as a time measure," a subject which he afterward developed in a remarkable measure, as will be told below; it may be noted that in an anonymous report in Science of that year (viii, 1886, 205) he was misrepresented as giving a more definite age for the falls than he intended. In 1887 he was chairman of section E at the New York meeting of the association, and read an address on tho work of the International Congress of Geology especially concerning the nomenclature of time periods, their stratigraphic subdivisions, and a color scheme for their representation on geological maps; all theso subjects having received much consideration from him as Powell's leading scientific adviser in Washington, and the last of them being treated in the New York address in much the same manner as in Powell's first report as director of the survey, above alluded to.

This address closed with such excellent counsel as to the duties and limitations of the International Geological Congress, that it is here quoted:

> The proper function of the Congress is the establishment of common means of expressing the facts of geology. It should not meddle with the facts themselves. It may regulate the art of the geologist, but it must not attempt to regulate his science. Its proper field of work lies in the determination of questions of technology; it is a trespasser if it undertakes the determination of questions of science. It may decree terms, but it must not decree opinions.

During one of the sessions of this meeting most of those present were well satisfied by a pertinent parliamentary ruling of the chairman. In the course of a discussion in which Powell had taken part, a member whose manner had only too frequently disturbed scientific gatherings replied to Powell directly, addressing him by name; he was promptly called to order by Gilbert who, rapping on the table, said as sharply as he ever spoke: "The speaker will please address the Chair"; and the disturbing member had at least the sense and the grace to accept the reproof, saying at once to the chairman, "You are perfectly right, sir," and governing himself accordingly for the remainder of the debate.

Although Powell as president of the association attended the meeting at Cleveland in 1888, Gilbert was held in Washington by administrative work. On the other hand, in the summer of 1889 Gilbert attended the meeting of the association at Toronto and performed the difficult duty of reading for Powell, who was then the retiring president but who was unable to be present on account of new duties in connection with irrigation and reclamation of western lands, an address surcharged with Powellian mannerisms on the "Evolution of music from dance to symphony." It must have been a curious experience for those of the audience who knew Gilbert's own simple manner of presentation, to hear him repeating, as a means of giving emphasis to principles with which he had no personal concern, the redundant series of exuberant, rhapsodic assertions and the surfeit of quaintly phrased illustrations which, particularly in this address, characterized his chief's extravagant style; but the real Gilbert and his exceptional capacity in scientific exposition were manifested in a public lecture on a subject that was a favorite theme of his own for the next 10 years, the history of Niagara River, which he was then interpreting in a truly marvelous manner; this address is outlined in a later section.

THE NATIONAL ACADEMY OF SCIENCES: DEFLECTION OF RIVERS

Gilbert was elected a member of the National Academy of Sciences in 1883, at the age of 40 years, and was a frequent attendant at the spring meetings in Washington thereafter. His first communication to the academy was made in 1884, and concerned the effect of the earth's relation in deflecting river courses,[5] an old subject to which he contributed a helpful step not previously noted, by showing that it is not the whole current of a river that will suffer deflection so much as the fastest or medial current; and that for rivers in the Northern Hemisphere this current, already displaced by centrifugal force toward the concave banks of a meandering stream, will be in consequence of the earth's rotation alternately a little more pressed against

[5] The sufficiency of terrestrial rotation for the deflection of streams. Amer. Journ. Sci., xxvii, 1884, 427–432.

the concave banks on the right and a little withdrawn from concave banks on the left; and thus a right-handed shift for the whole course should be gradually effected. He calculated that for the Mississippi "the selective tendency [to deflect the thread of maximum velocity] toward the right bank is . . . nearly nine per cent greater than toward the left." To test his views he sent an experienced observer to Long Island, to examine the valleys there which E. Lewis had described in 1877 as exhibiting unsymmetrical cross profiles due to the deflective force of the earth's rotation; and the results thus secured were briefly summarized:

> The south side of Long Island is a plain of remarkable evenness, descending with gentle inclination from the morainic ridge of the interior to the Atlantic Ocean. It is crossed by a great number of small streams which have excavated shallow valleys in the homogenous modified drift of the plain. Each of these little valleys is limited on the west or right side by a bluff from 10 to 20 feet high, while its gentle slope on the left side merges imperceptibly with the plain. The stream in each case follows closely the bluff at the right. There seems to be no room for reasonable doubt that these peculiar features are, as believed by Mr. Lewis, the result of terrestrial rotation.

Although he quoted Buff as according "a more important influence to the prevailing winds than to the rotation of the earth," he seemed to dissent from that view; and said with more definiteness than usual in announcing a scientific conclusion: "It is my present intention to maintain the sufficiency of the cause"—the deflective force arising from the earth's rotation—for the deflection of rivers. Yet it has been later shown by a study of detailed maps of the Lower Mississippi on which the river course is shown at the time of two surveys separated by an interval of about 13 years, that the displacement of the later course with respect to the earlier is clearly to the east or left, as if because of the winds prevailing, and not to the west or right, as the earth's rotation would have it. It may therefore be inferred that the wind exercises a stronger effect on large rivers than on small streams; but it does not follow that no other control than the earth's rotation has determined the asymmetry of the valleys eroded by small streams of Long Island. The slant of rain in west winds may be important there.

AGE OF THE EQUUS FAUNA

Another communication was made to the academy in 1886, on the "Age of the Equus fauna," a subject which is treated in the ninth chapter of the Bonneville monograph, later issued, and which may surprise some readers by the suggestion it gives that Gilbert was making an excursion into paleontology; but as a matter of fact his chief object appears to have been to teach a lesson that he thought paleontologists ought to learn from physiography.

The need of the lesson arose as follows: Few fossils had been found in the Bonneville beds, but the deposits of a smaller and not distant extinct lake had been discovered to contain a rich assemblage of mammalian fossils, to which the name *Equus fauna* had been given, and for which a Pliocene date was given by a leading vertebrate paleontologist of the time. But Gilbert was persuaded by the freshness of the Bonneville shore lines that they could not be Pliocene, and by the freshness of the deposits in the other smaller basin that they must be substantially contemporary with the Bonneville deposits; the latter opinion was held with all the more confidence because the deposits in each basin implied a former moist climatic period which must have been contemporaneous in the two districts. He therefore undertook an examination of the principles of correlation by which the Equus fauna had been and should be dated, and showed that for the case in hand the resemblance of fossil forms, in America and Europe, in view of which the American fauna had been made the equivalent of the European Pliocene, was a less trustworthy guide than the physical contrasts between the surface features of the formation that contain the American fauna and those of the European Pliocene, and that in view of these contrasts the two formations should be regarded as of different ages; for while the original limits of the latter are to-day hardly identifiable and their original surface is obsolete or obsolescent, the shore lines of the former are fresh and their original surface is unworn. Hence, in spite of the paleontological argument, Gilbert maintained that the Equus fauna, like the Bonneville deposits, should be classed as Pleistocene and referred to the later half of the later Glacial epoch.

This conclusion was enforced by the statement of an elementary physiographic principle, which Gilbert seemed to feel had been insufficiently considered by paleontologists:

> When a surface shaped by some other agent than the atmosphere . . . is exposed to atmospheric agencies, its sculpture begins. For a long time its original features continue to be the characteristic ones, but they eventually become subordinate and finally disappear. The original forms are at first new and fresh, then old, worn, and hard to discover; and finally the fact that they once existed can be known only from the internal structure of the deposits to which they belonged.

It is striking testimony to the recency with which the rational study of land forms had then been entered upon to find that principles so simple as these were deemed important enough by so profound a student of geological philosophy as Gilbert, as to warrant their presentation at a National Academy meeting! The annual report of the academy notes that Gilbert's paper was discussed by Cope, Marsh, Powell, and Gill; but no record is preserved of their scintillating remarks, nor of the measure of acceptance given by the paleontological three of the four to Gilbert's physiographic lesson.

JOINTS IN BONNEVILLE CLAYS

During the first 10 years of Gilbert's residence in Washington as a member of the national survey, he was so occupied with the duties of his office that he seldom had leisure to write out in full the substance of the communications made at meetings of scientific societies; even the results of his field work in western New York were inadequately published, as will appear in the sections devoted to the problems there investigated; it is therefore not surprising that he seldom found time for the preparation of independent articles for scientific journals. One of the few subjects that he treated in this way was the origin of joints, with particular relation to those that he had seen in the Bonneville clays.[6] His discussion of this topic is objectively a useful contribution to a difficult problem, but it is here of greater interest as an illustration of his capacity and his habit, one might almost say, his preference for maintaining a suspended judgment in matters regarding which any shade of doubt remained. He described the facts and then examined the explanations that had been proposed for them:

> If the considerations here adduced have weight, then neither hypothesis [shrinking or compression] is satisfactory, and the problem is an open one. It is certainly hard to correlate the parallelipipedons into which the clays of the Salt Lake desert are divided with the polygonal prisms normally arising from shrinkage; and it is especially hard to admit that the clays have been subjected since their deposition to coercive pressures from two independent directions. In my judgment it is proper to conclude, first, that the joints are not due to shrinkage, and second, that the theory which regards them as identical with slaty cleavage and ascribes both to compression is untenable. If pressure and compression suffice for the explanation of slaty cleavage, then jointed structure is something distinct from cleavage and needs an independent explanation. If joints and cleavage are merely diverse expressions of the same general structure, then the theory of slaty cleavage which has been so widely received fails to comprehend all the facts and needs to be revised.

It may be added that while this problem was in Gilbert's mind, his few diagrams of basin-range structures represented their inferred marginal faults by vertical lines, this being the graphic expression of his belief that their uplift resulted from vertical displacement without lateral compression; and that it was from this indirect argument he excluded compression from all share in the production of joints in the Bonneville clays. It was not until a number of years later that he came upon certain features of the range fronts which indicate a slanting attitude of the master fault planes, and it would seem that the oblique displacement of the mountain blocks on such faults might well occasion a considerable amount of compression in the surface parts of the blocks at least; but it is hardly possible that the small amount of oblique displacement in post-Bonneville time could compress the clays sufficiently to joint them.

HOME AFFAIRS

It is sad to have to relate that during this period of growing scientific relations Gilbert's home life was clouded by a heavy affliction. For two years after returning from Salt Lake City his family had no fixed residence in Washington, and it was during this unsettled period that

[6] Postglacial joints. Amer. Journ. Sci., xxiii, 1882, 25–27. On the origin of jointed structure. Ibid., xxiv, 1882, 50–53; xxvii, 1884, 47–49.

to his profound grief his little daughter, Bessie, whom he "loved more than anyone else in the world," died of diphtheria, May 8, 1883, in her seventh year. A short time afterwards the father, mother, and aunt were attacked by the same disease, then so much dreaded, but all recovered. Gilbert's recovery must have been slow, for a burden of grief at the loss of his daughter weighed upon him. He went to Virginia the following summer for a month's rest alone in the country. It was probably to this painful period that he alluded, when talking years afterwards with a friend to whom he confided many intimate matters, as a time in which he had "fought out" certain questions of inmost religious belief; but on such subjects he rarely spoke to anyone.

A touching allusion to the death of the little daughter is found in one of his letters written many years later to his elder son. He briefly mentions seeing "a quiet little girl of Bessie's age," thus showing that she remained a child in his memory, although she would then, if living, have been a middle-aged woman. It is always so when the young die; more fortunate than the Sibyl who escaped death at the heavy cost of growing very, very old, children on dying preserve a long-lasting youth in the hearts of their parents. A still later memorial of the sad summer of 1883 was found in Gilbert's will, a paragraph of which read:

> I bequeath to Emma Dean Powell, widow of the late John W. Powell, the sum of one thousand dollars in loving remembrance of her great kindness to me and mine in time of need.

The cause of this bequest appears to be that, the time of Bessie Gilbert's fatal illness being before the modern era of professional nursing, the care of the little sufferer was shared by members and friends of the family. Her father watched by her bedside until loss of sleep made him distrust his capacity to give her proper attention, and Major Powell's wife was one of those who relieved him when he was exhausted.

It was at a somewhat earlier date that the prolonged ill health of Gilbert's wife began. Its first impairment, attributed to coal-gas poisoning which affected all the family, but the mother most severely, was not so serious as to interfere greatly with the usual course of home life; this was indeed stabilized in October, 1883, by the purchase of a house, 1424 Corcoran Street. As the father's salary, even when supplemented by the installments of an inheritance which his wife had received from her mother, was not greatly in excess of the family expenses, a good part of the cost of the house was left on mortgage; but the mortgage was steadily reduced by annual payments and canceled in 1886. This house continued to be Gilbert's residence until his wife's death, 16 years later, although in the meantime the family was much divided by reason of the father's absences on field work, the mother's illness which sometimes necessitated her withdrawal to a convalescent hospital, and the boys' attendance at boarding school. The Corcoran Street house was nevertheless a preferred gathering ground for the neighborhood playmates of the two boys when they were at home, for despite her illness the boys' mother always made the "crowd" welcome. One of them, who still recalls with gratitude the good times he enjoyed there, imagines that the bill for ginger snaps and milk on which the "crowd" was often regaled must have been enormous.

Each year during the heat of the summer, when Gilbert was absent for longer or shorter periods, Mrs. Gilbert and her sons usually went out of town; to a Virginia village, in 1881; to Asbury Park, N. J., in 1884 and 1885; to Hamilton, Md., in 1886; to Mount Desert, Me., in 1887; and to Rye Beach, N. H., in 1888. In the spring of 1887, Gilbert took up bicycle riding, for which the smooth streets of Washington were well adapted; and a number of entries in his pocket diary of that year record the hiring of a "Sociable," or two-seated bicycle, on which he and his wife rode together.

Such was Gilbert's thrift that during the same year in which he finished paying for his house, he made also the last payments on a life insurance policy for $9,000, and yet saved $780 at the end of the year. Nevertheless, the margin of income over expenses was frequently narrow; for although a little over $1,000 was saved in one year of exceptional economy, the savings of the next year were reduced to $80 by the cost of repairs on the Corcoran Street house; and a trip to Europe in 1888 left a balance of only $55 at the close of that year. These items

are recorded here in order that future historians and economists may know the conditions of life imposed in the latter part of the nineteenth century by the richest country in the world on one of the ablest men in its service. It may be added that in 1889 the Corcoran Street house was painted; that event being here chronicled, not because of its intrinsic importance, but because it serves to introduce a characteristic passage from one of Gilbert's letters to an intimate correspondent:

> I have had my house painted a pale green-gray and the trimmings a deep but rather quiet green—window sashes red. I know it is all right, first because I got Mr. Gill, one of our [Survey] artists, to tell me what colors to use, second because it *looks* all right. If someone *tells* you you are happy and if you *feel* happy, why of course you *are* happy.

After mention of other members of the family, the letter continued: "The old man is still serenely bobbing upwardly."

Gilbert had great need of his serenity, for his wife's health had changed from bad to worse and in time reached the stage of chronic invalidism. As she could give little care to the household, the two boys spent a large part of each year with their uncle at Rochester or at boarding schools and summer camps; the "old man" was much alone. Although he gave every care and made every effort to restore his wife to health, she never recovered her strength. His devoted attention during the long period of her invalidism excited the admiration as well as the sympathy of his associates. Even when oppressed by care and grief, his patience was untiring, his thoughtfulness unfailing; and as far as the outside world could see his serenity was preserved unruffled. Indeed, so buoyant was his nature and so well was he sustained at time of trouble by a courageous and cheerful philosophy, that in spite of the disappointment caused by the transfer of his work from the Great Basin to the Appalachians, and in spite of the distractions caused by home cares, his life always seemed to be joyous.

GEOLOGY OF THE APPALACHIANS

The Sixth Annual Report of the National Survey for 1884–85 announced that Gilbert, then a little over 40 years of age, had been placed in charge of the division of Appalachian geology, but the next year he was relieved of its Archean rocks. Gilbert commented on the task thus assigned to him as differing from his previous work "not only in its character but in the fact that it already possesses a copious literature." It may be doubted whether the resulting necessity of looking up all the fragmental studies of his many predecessors made the Appalachian field attractive to his original and independent mind. However, he accepted the duty and reported for 1885–86 that a comprehensive subject-bibliography of Appalachian geology had been begun, and that under his direction some 6,000 bibliographic cards had been prepared; the next year the number of cards reached 11,000. A later consequence following from this particular manifestation of Gilbert's all-round capacity was his appointment in 1891 as the American member of an international commission, under the chairmanship of de Margerie of Paris, to prepare a bibliography of geological bibliographies, which was published in 1896, as noted below. In the meantime, the completion of the Bonneville report was still delayed.

Appalachian field work was begun in the second half of 1884, when Gilbert spent a fortnight of August in the mountains of North Carolina, Tennessee, and Georgia. It is significant that his observations there were not stratigraphic, but had "special reference to the terrace system of the mountain valleys," a distinctly physiographic problem. Three weeks were given somewhat later to similar studies in New England and eastern New York; and from this it may be inferred that the chief of the Appalachian division expected to gather a larger volume of novel results by the application of new methods of physiographic interpretation, learned in the West, to this long-studied eastern region, than could be gained in the same amount of time from a revision of its stratigraphy. The physiographic nature of his interest in the Appalachians was further emphasized in his administrative chapter of the annual report for 1885–86, in which it was announced that he reserved for his own study the "evidence of elevation and subsidence existing in the topography of the entire district." But although his intention thus appears to have been all-embracing at first, his published work on the modern or physiographic phase of

Appalachian history is almost wholly limited to the region of the lower Great Lakes, where his field work began in the summer of 1885, as will be narrated below. Regarding the physiography of the remainder of his eastern field, his associates and disciples, inspired in the next following years as much unconsciously as consciously by his suggestive teachings, published far more than he did; yet he was so generous as to congratulate them on their work. He wrote to a correspondent in July, 1891:

> When I was called east to take charge of the Appalachian division, the part of work I reserved for myself was the correlation of the coastal plain formations and unconformities with the baselevels of the Appalachians; but I never got fairly at it, and so ——— and ——— have cut in ahead of me. As I do not believe in the establishment of scientific preserves, I have no complaints to make, and only a shade of regret that I am not in it; otherwise I am proud of the way the work is being done.

As to the Paleozoic geology of the Appalachians, Gilbert appears never to have exercised more than a supervising control. He retained charge of the division for five years, and visited his field parties in the Southern States on several occasions, but the actual field work was done by his assistants. The first of these, appointed in 1885, were Russell and Willis, who like their chief had previously worked in the West; they were assigned to the study of several transverse sections widely spaced from Maryland to Alabama. Hayes, Keith, Geiger, and Darton were added in 1887, and in that year the transverse sections were completed and areal work was begun. Along with this, Gilbert proposed the preparation of a soil map of the Appalachian belt, but the proposal was not realized. Much of his own work was still, as reported to Powell in the ninth annual, 1887–88, turned to a variety of small tasks:

> My attention in Washington has been directed largely to administrative details under your immediate direction, but I have also spent some time upon the long deferred report on the history of Lake Bonneville, now nearly ready for the press.

His share in Appalachian work was therefore more in the way of advice and inspiration than performance. That he left to his associates, to whom he opened wide opportunity and gave full responsibility and full credit, and from whom he therefore received the most loyal service; for there is nothing that binds a junior to his senior so much as to be trusted.

As areal work soon advanced at such a rate that paleontological correlation could not keep pace with it, a serious problem arose, for which an expedient solution was announced in the tenth annual, 1888–89; the field parties were "compelled to define and map formations as local masses"; or, as stated more fully a year later in the eleventh annual, "formations rather than names should be mapped; that is to say, the actual distributions afforded by the stratigraphy of the given area are to be represented on the map in preference to distinctions belonging to the stratigraphic succession of other areas." In consequence of these instructions the field geologists had to introduce a large number of new names for locally recognizable groups of strata which could not be accurately assigned to the standard subdivisions of the geological scale, previously established chiefly with reference to those in New York. This decision, which had Powell's full approval but which was undoubtedly formulated by Gilbert, has been adversely criticized by those who desired to see what they regarded as a more scientific method of progress in the national survey; but the reason, not to say the necessity, for the decision is fairly manifest in the case of a survey that depends for its continued existence on annual appropriations from a Congress not always interested in the expenditure of large sums of money for scientific purposes. The funds available each year truly might have been so apportioned that the area mapped according to visible formations by the field geologists should not have been extended faster than fossils could have been searchingly collected from each of the formations on the ground and studiously compared with the standard collections of the National Museum and elsewhere, so that, theoretically at least, a single set of time names might be applied to all areas; but on such a plan the progress of the geological map, the preparation of which was the survey's prime duty, would have been so slow as to endanger the continuation of congressional appropriations. Hence the available funds were so apportioned that areal work should advance rapidly enough to make what might be congressionally considered a good showing for the funds

expended. The practical exigency was met in a practical way, and as a result the effectively mapped Appalachian area increased rapidly. A number of years later, when paleontological studies were more advanced, they were given greater application.

While the work of the Appalachian division was in successful progress, a new task of large importance was laid upon its chief. The growth of the survey necessitated that descriptions of all formations belonging under the chief divisions of the geological time scale in every part of the United States should be gathered from the countless articles and reports in which they were recorded, comprehensively reviewed, and summarized in standard form for ready reference. Hence a series of bulletins, called "Correlation Papers," were begun by various authors under Gilbert's supervision, as will be further told below. But in spite of these added duties, he reported on July 1, 1889:

> The monograph of Lake Bonneville is now complete in manuscript, and will be transmitted in a few days.

It might be thought that he would have thereupon turned his attention more closely to the Appalachian field, but a very different fate awaited him. The direction of work on the Appalachians was handed over to Willis, and Gilbert was appointed to the new office of "chief geologist" in order to relieve Powell of administrative details; as a result he was withdrawn from nearly all his own studies and held in Washington for three years even more closely than before.

CHAPTER XV

THE INCULCATION OF SCIENTIFIC METHOD BY EXAMPLE

GILBERT'S FIRST PRESIDENTIAL ADDRESS

Gilbert's election in 1884 as president of the Society of American Naturalists—the first scientific society to discover that he was made of presidential timber—placed upon him the duty of preparing an address that should be acceptable to scientists of both the indoor and the outdoor kind. It may be inferred that in casting about for a subject to his mind, he recalled a passage he had written during the year of his presidency in a review of Geikie's "Geological sketches at home and abroad," which runs as follows: Two of the sketches were selected for particular mention; one—

describes a journey to central France, undertaken for the purpose of studying the extinct volcanoes of that region as an aid to the imagination in restoring the condition of Scotland during the Carboniferous period; and another describes a journey to Norway with the parallel purpose of rendering vivid the mental restoration of Scotland in Glacial times. These two are perhaps the most instructive of the collection, for besides making definite additions to the geological history of Scotland, they present admirable illustrations of one of the most valuable methods of scientific investigation. The principles which distinguish modern scientific research are not easily communicated by precept, and it is by no means certain that they have yet been correctly formulated. However it may be in the future it is certain that in the past they have been imparted, and for the present they must be imparted, from master to pupil chiefly by example; and whoever in publishing the result of a scientific inquiry sets forth at the same time the process by which it was attained contributes doubly to the cause of science.[1]

The principle stated in the last sentence was exemplified in his presidential address.

The meeting of the naturalists was held in Boston in December, 1885, and there Gilbert came for the first time into acquaintance with a good number of biologists. He not only delighted them by his fine presence, but his presidential address on "The inculcation of scientific method by example" was at once recognized as so masterful a production that he was forthwith reelected as president of the society for a second year. The address[2] has ever since held a high place among American scientific essays and still merits attentive study by every geologist who has not read it; indeed it deserves rereading by any geologist whose first encounter with it was a score of years or more ago, provided that he aspires to a conscious inspection of his own mental processes as a means of improving them. As one measures himself up against the method here set forth, he may make a helpful estimate of his scientific method, and learn how far he has advanced toward the high standards of impartial objective research that Gilbert announced. An attentive reading of the essay will encourage the more careful formulation of half-conceived thoughts; it will focus attention on one's half-recognized deficiencies and promote their correction; it will inspire the thoughtful student of natural science to self-examination and thoroughness in every stage of his investigations; best of all, it will awaken the generous and loyal elements of his nature and set them to guard against egotism and selfishness. Although the problem that Gilbert selected as an example with which to illustrate the method of science was that phase of the Quaternary geology of Utah which is concerned with the disappearance of Lake Bonneville, the facts and the inferences of the case were so successfully subordinated to the logic of their treatment as to appeal strongly to all members of the audience, whether they were geologists or not.

The address was opened by a consideration of certain familiar principles, the common property of scientific men, along with the essential nature of scientific research and the process by which science advances. Attention was then directed to the importance and the difficulty of scientific observation, which "endeavors to discriminate the phenomena observed from the observer's inferences regarding them," and which as it advances "seeks new facts that will

[1] Nature, xxvii, 1885, 261.
[2] Amer. Journ. Sci., xxxi, 1886, 284–299.

augment the established groups," and thus enables the observer "to concentrate his attention and sharpen his vision." The inductive or empirical classification thus begun "is a stepping stone to a logical, or rational, or, more strictly, relational classification . . . it leads to the understanding of those deeper relations which constitute the order of nature."

With the progress of observation and classification, the desire then arises to discover the "antecedents" of which the observed facts are the "consequents." Here enters the aid of hypotheses, which as they are invented—usually by an unconscious mental process—must be subjected to tests for the determination of their competence; and here lies "the prime difference between the investigator and the theorist. The one seeks diligently for the facts which may overturn his tentative theory, the other closes his eyes to these and searches only for those which will sustain it." Is there any reader of this statement who, on scrutinizing his own record, can declare himself always to have been an investigator, as thus defined, and never a theorist? It is next pointed out that an investigator will find "advantage in entertaining several [hypotheses] at once, for then it is possible to discover their mutual antagonisms and inconsistencies, and to devise crucial tests . . . until he finds an hypothesis that remains unscathed after all the tests his imagination can suggest. . . . Evidently, if the investigator is to succeed in the discovery of veritable explanations of phenomena, he must be fertile in the invention of hypotheses and ingenious in the application of tests."

This declaration introduces a striking passage, already quoted in connection with Gilbert's education at Rochester: "The practical questions for the teacher are, whether it is possible by training to improve the guessing faculty, and if so, how it is to be done." Another soon follows: It is recognized that a majority of students "ask only for a store of knowledge," but the modern educator believes that training the mind is more important than storing it, and he therefore employs "such methods in storing the minds of his pupils with knowledge that they acquire at the same time the best training." It is as a part of training in method that the "guessing faculty" is given so high a rank. If the teacher, imbued with the vastness of science, yields to the temptation of giving his pupil, especially those whose careers are not to be scientific, "a maximum number of facts . . . there is reason to fear that a permanent misconception is established, and the essence of science is not communicated." The teacher "will do better to contract the phenomenal, and enlarge the logical scope of his subject, so as to dwell on the philosophy of the science rather than its material." Young teachers in particular will profit by reviewing their growing experience in the light of this advice.

THE SCIENTIFIC GUESS

The "scientific guess as a mental process" is then analyzed. In so far as it embodies a search for an explanatory hypothesis of certain observed facts, it is shown to depend on "analogic suggestions." That is: "Given a phenomenon, A, whose antecedent we seek. First, we ransack the memory for some different phenomenon, B, which has one or more features in common with A, and whose antecedent we know. Then we pass by analogy from the antecedent of B to the hypothetical antecedent of A, solving the analogic proportion—as B is to A, so is the antecedent of B to the antecedent of A. Having thus obtained an hypothesis, we proceed to test it," as above. But testing hypotheses is given a lower value rank than inventing them.

> The great investigator is primarily and preëminently the man who is rich in hypotheses. In the plenitude of his wealth he can spare the weakling without regret; and having many from which to select, his mind maintains a judicial attitude. The man who can produce but one, cherishes and champions that one as his own, and is blind to its faults. With such men, the testing of alternative hypotheses is accomplished only through controversy. Crucial observations are warped by prejudice, and the triumph of the truth is delayed.

How few can, near the end of their career, read those lines without wishing that, on some occasions at least, they had taken more the attitude of a judge and less that of a champion!

The investigator who has reached a sound conclusion involving a new principle is then urged to "describe the route by which his end was reached." Let him—

> recite every hypothesis that occurred to him in the course of his search, telling, if he can, how it was suggested. Let him lay bare the considerations which rendered it plausible, the tests that were conceived, and those which

were applied. Let him show in what way the failure of one hypothesis aided in the invention of another. Let him set forth not only the tests which verify his final hypothesis, but the considerations which leave a residuum of doubt as to its validity. And finally let him indicate, if he can, the line or lines of research that promise to throw more light.

But it is recognized that an essay thus prepared will not have a great number of readers:

The majority of those who examine an essay seek only to learn its conclusions and have time for nothing more. For their use there should be appended or prefaced a concise summary of results.

There can be no question that the author of a scientific essay should consciously scrutinize not only the matter that he treats but also his method of treating it; but it may be doubted whether all authors of essays which even in their own opinion involve new principles should publish the many detours on their routes of mental travel in such detail as is here recommended. It is to be feared that scientific journals would be thereby too largely filled with futilities by workers of a vainglorious type, and that others, indeed the very ones whose mental routes their colleagues would be glad to know, would hesitate to adopt a method of writing that might seem to them like exclaiming: "Look at me; let me show you how to do it!" Scientific diffidence of that sort is responsible for much of the obscurity from which the history of science suffers and by reason of which the biography of scientific men is often too largely a narrative of their physical performances rather than of their intellectual characteristics. Even in Gilbert's own case, the mental methods of his earlier progress are lost, because he then did not adopt the self-revelation that he later advised.

But in his fuller maturity when he wrote the "Inculcation" essay in his forty-second year, he practiced his preaching and told in much detail how he had gradually worked out an explanation for the warping of the Bonneville shore lines, this being the particular "example" he selected for analysis. It had long been tacitly postulated that the shore lines were level; but when it was found that a certain member of the group, the continuity of which had been traced by direct observation, was higher in the southern than in the northern part of the former lake area, the tacit postulate was overthrown and "an hypothesis immediately took its place," the hypothesis of "unexplained undulation." It was later discovered that a fault ran along the base of the Wasatch Range and it was then recognized that faulting might modify undulation; but it was eventually learned, as quantitive records were increased, that deformation by faulting was much smaller than that by undulation; thus the latter became the chief factor, and search was made for its cause. As a guide in this search the measures of deformation were charted, and it was thus found that the figure of deformation was "a low, broad dome, having its crest over the main body of Lake Bonneville." Therefore the additional hypothesis was introduced that "the deformation stands in some necessary or causal relation to the lake and its disappearance"; and as the lake was disposed of by evaporation, it was concluded that "the change in the lake was the cause, and the change in the land was the effect." The manner in which the disappearance of the lake might lead to the deformation of the shore lines was next taken up, and three explanatory hypotheses were introduced, but at this point the discussion becomes so detailed that it can not be advisedly followed further here.

FIRST VIEWS ON ISOSTASY

Faithful to his principles, Gilbert concluded his address with a brief indication of the uncertainty attending the provisional conclusion that he reached, and with some suggestions concerning important lines for further research. The most promising of the latter would be "an exhaustive hypsometric survey of the Bonneville shore line, including all bays and islands," for this might "render possible an evaluation of the rigidity of the earth's crust"—a subject which he treated some years later in greater detail—and the behavior of the earth's crust must throw light upon the condition of the earth's interior, "one of the great problems of our generation." Those who have approached this problem—

from the geologic side have based a broad induction on the structural phenomena of the visible portion of the earth's crust, and have reached the conclusion that the nucleus is mobile. Those who have approached it from the physical and astronomic side have reached the conclusion that the nucleus is rigid. Here seems an oppor-

tunity for a crucial observation. If the crust of the earth floats upon a fluid nucleus, the evaporation of Lake Bonneville, by lifting from it a great weight, must have produced an uplift of determinate form. If the whole earth is solid, such a result could not have been wrought. The decisive phenomena are known to exist, and to be accessible, but they are scattered over a broad desert, and they can be gathered in only at the cost of much money and great labor.

There is outlined the problem to which Gilbert's genius would have been directed, had he been free to follow his manifest inclination, instead of being loyally bound to support his director, Powell, in the administration of a great national scientific organization. And at the beginning of the paragraph from which the last quoted lines are taken stands the touching phrase in which he, as it were, takes leave of his personal ambition: "It is hardly necessary," he said to his audience, "to assure you that my personal regret in abandoning this research at its present stage is very great." That phrase was an epitaph.

CHAPTER XVI

THE PROGLACIAL GREAT LAKES

REACTION OF THE WEST UPON THE EAST

After Gilbert, in his capacity as geologist in charge of the Appalachian division of the national survey, had reserved for his own study the "evidence of elevation and subsidence existing in the topography of the entire district," as above noted, he curiously enough restricted himself almost wholly to the area traversed by the abandoned shore lines of ancient lakes in New York, Ohio, and Michigan, an area that lies well to the west of the Appalachian belt proper. As to his reasons for this choice of field, it may be that he wished to complete certain inquiries which he had opened 15 years before when reporting upon the Maumee Valley for the Ohio survey; or perhaps he was led by a half-conscious homing instinct to return to the neighborhood of his native city of Rochester and once more concern himself with the origin and age of Niagara Falls, a subject that he had touched upon briefly in his first published essay of 1867; but it is more probable that he turned to the shore lines of the ancient Laurentian lakes in the hope of solving there a problem that had deeply interested him during his studies of the Bonneville shore lines in Utah. This supposition is supported by two allusions in his western reports to the possible tilting of lake shore lines in the Laurentian region, as follows:

The first Wheeler survey report mentioned the then suspected deformation of the Bonneville shore lines as of especial interest because, if proved to be true, it would show "undulations of the solid earth, produced at so late a geological date that we may presume them identical with changes now transpiring"; and then after pointing out that records of such undulations are found only where a broad water surface, like that of the ocean, affords a datum plane of reference, he added:

> There are, however, a few interior lake systems, broad enough to tell us something of the warping of their shores, and our continent contains two, at least, that are of value—that of the Laurentian lakes, and the one that we are considering (III, 1875, 93).

Similarly, in the Henry Mountains report, brief reference is made to the tilting of the earth's crust as causing changes in the outlets of the Laurentian lakes (1877, 126). It therefore seems likely that on transferring his field of work from the West to the East, Gilbert brought with him the general problem of crustal warping as recorded in the shore lines of extinct lakes; as he could no longer work upon the problem in the basin of Great Salt Lake, he took it up with respect to the extinct lakes of the Laurentian region. It is true that his first field studies in the East were concerned with river terraces in the southern Appalachians, but he soon turned his attention to the ancient shore lines in the Northern States above mentioned, as probably affording better "evidence of elevation and subsidence." He at once found the evidence to be both plentiful and definite; for in the summer of 1883, his first season in search of it, he discovered that the ancient lake shore lines in New York and Ohio do not lie level but, as others had already learned for New York, gradually rose to the northeast. He thus entered upon an important chapter in the physiographic geology of northeastern North America. Many pages of that chapter will always bear the marks of his genius. Curiously enough the crustal tilting that he so promptly detected was precisely the opposite of that which he had postulated 15 years before in the Maumee Valley report as necessary to hold up the waters of the enlarged Lake Erie; the shore lines showed that the region of the lower St. Lawrence had been lower, not higher than now in the late stages of the glacial period; and he was therefore led to accept the presence of a broad glacial barrier—the retreating ice sheet—in the St. Lawrence Valley as a means of explaining the higher stand of the lake waters, essentially as Newberry had suggested in his Maumee footnote. He at first phrased the change of level as a "local relative depression of the land" in the region of Lakes Erie and Ontario, but later regarded it due to uplift of the St. Lawrence region.

Gilbert spent from one to three months of seven successive summers, 1885 to 1891, in western New York and neighboring areas; Canada was visited between Lakes Ontario and Huron in the summer of 1887 at his personal expense, as survey funds could not be used for studies beyond the national boundary. He gathered a great body of information on ancient shore lines, much of which he never found time to state in his printed articles, which were as a rule very brief. He reached the conclusion that "in a general way the old water-plane is nearly flat at the west end of Lake Erie, rises to the northeast about one foot per mile in eastern Ohio, and rises about two feet per mile in Pennsylvania and New York." But he was by no means alone in this work. Other observers also engaged upon it proved the occurrence of crustal warping in various parts of the Great Lakes region that Gilbert did not visit; yet no other observer generalized the results obtained so widely or so wisely as he did. As it was impossible for him to devote more than a fraction of his time to field study, he welcomed all the results gained by others and gave every encouragement and assistance to his fellow observers by freely communicating his own ideas to them. Thus he wrote in 1888, to one of the most assiduous students of the problem: "The ground that you and ——— are studying I had hoped to study myself. As it now seems improbable that I shall be able to visit . . . this summer it occurs to me that there may be some advantage in my communicating to you such working hypotheses as I have framed"; and thereupon follow four pages of helpful notes. Another letter to the same worker, written in 1890 when parts of his work in the Ontario basin remained unpublished because he could not advance them further at that time, made a generous offer:

I shall be very glad to communicate any data in my possession which may aid you in your investigations or conclusions. If you can come to Washington before you publish, I shall not only be glad to go over the entire subject with you, but shall esteem it a privilege to exchange views.

Similarly, a few years later, he wrote to another observer in the same field, urging him to go ahead on his part of the problem:

I have little sympathy with the dog-in-the-manger policy of those who fear a deprivation of the fruits of their labors.

THE SHORE LINES AND OUTLET OF LAKE IROQUOIS

A phase of the problem in which Gilbert's observations were critical and decisive concerned the distinction between the shore lines of ice-dammed lakes and those of arms of the sea; for while it had long been generally understood that the lower St. Lawrence Valley had been occupied by marine waters after the withdrawal of the ice sheet from it, difference of opinion existed as to the altitude of the uppermost marine limit. Gilbert resolved this uncertainty by a careful examination of the northern slopes of the Adirondack Mountains, where he spent two months in the summer of 1887, mostly on foot. No shore lines were found there at levels corresponding to those which occurred "on a magnificent scale" around the western slopes; hence, he concluded that while the shore lines of the western slope were in process of formation, the northern slope as well as the whole breadth of the St. Lawrence Valley was occupied by the retreating ice sheet. The northern slope was not only free from shore lines; it was also free from the eroded channels of lake outlets—except that, as found several years later, one high-level example of such a channel, locally known as "the Gulf," was found in the Potsdam sandstones on the northern side of the Adirondacks close to the New York-Canadian boundary.

In the absence of such channels at lower levels on the north of the Adirondacks, the outlet of the falling proglacial lake must have been elsewhere. It was found at the lowest point of the broad divide southwest of the Adirondacks, between the present drainage areas of the Lake Ontario and the Mohawk-Hudson River. Part of the channel floor at the divide is occupied by a small town bearing the name of the Eternal City of the Old World; this point of discharge is therefore known as the Rome outlet. The shore lines diverged from that point to the north and the west; hence the lake water lay to the northwest of it; and for this lake the name Iroquois was adopted. Eastward the channel, soon entered and followed by the little Mohawk from the north, had a gradually descending bed, broadly eroded in shales and limestones and inclosed by subparallel banks; hence the discharge of the lake was in that direction.

At a point where the outflowing waters farther downstream crossed a small area of resistant crystalline rocks, the channel assumes the form of a narrow, rocky gorge of steeper descent, where the lake waters must have rushed in tumultuous cascades and rapids; the Mohawk River still cascades there, and on its northern bank is the manufacturing town of Little Falls.

After the outlet channel of Lake Iroquois was found, Gilbert made one of the finest of all his beautiful interpretations concerning the postglacial physiographic history of the region. He saw that if the upheaval of the land in the northeast continued while the discharge of the lake was maintained at the Rome outlet, the shore should progressively emerge from the lake to the north of the outlet, and the lake should progressively submerge the shore to the west. In other words, the successive attitudes of the lake margin to the north of an axial line of no change drawn northwestward through the outlet should be marked by a series of lower and lower shore lines, each of which would be left undisturbed by the withdrawing waters; and the shore lines thus formed, all uniting at the outlet, should diverge northward, so as to be separated by increasing vertical intervals with increase of distance from Rome. On the other hand, the successive lake margins to the west of the axial line should rise upon the land and, pushing the beach gravels before them more or less successfully, should tend to obliterate the first formed shore lines, and therefore leave a simpler record than to the north.

Later observations completely verified these expectations. To the north there are multiple and gradually diverging beaches, which at their greatest distance from Rome range over a vertical interval of scores of feet; the development of these shore lines "on a magnificent scale" was presumably favored by their situation at the eastern end of a large lake prevailingly swept over by westerly winds. To the west of the Rome outlet the shores are marked by simpler beaches, often by only a single beach, the structure of which shows an upward and landward growth. A long stretch of this single beach is displayed in the drumlin district between Syracuse and Rochester, where the beach usually consists of shoal-water bars forming a long linear succession of short members, each slightly concave to the north, between the clift ends of numerous drumlins; farther south many other drumlins are intact; farther north a plain slants away and drumlins are wanting for a considerable distance, as if the encroaching waves of the slowly rising lake had cut the drumlins away there as they pushed the beach bars farther and farther south. The beach would undoubtedly have been pushed farther south still had not the ice barrier in the St. Lawrence Valley withdrawn sufficiently to open a lower passage, whereupon the Rome outlet to the Mohawk was abandoned, the St. Lawrence River came into being, Lake Iroquois was lowered to Lake Ontario, and the beach bars were left high and dry. It all became astonishingly simple and clear as soon as Gilbert showed how clear and simple it really is.

A detail may be here added. A southward extension of the Ontario lowland permitted the extension of a long bay branch of Lake Iroquois into the north-south trough now occupied by Cayuga Lake, one of the several Finger Lakes of western New York; and it might be expected that the Iroquois shore lines which are so well developed east and west of this bay branch, should be found looping southward around the sides of the trough and thus marking the extent of the bay. But as a matter of fact the shore lines are not visible in the farther part of the trough. Gilbert explained their invisibility not by their absence, but by their submergence beneath the Cayuga waters, which in consequence of the uplift of the land to the northeast have risen increasingly southward. This is a good example of the interlocking of small items by which Gilbert's studies on the proglacial lakes were so largely characterized.

The single western beach is found in good force in the form of an offshore bar at Lewiston, on the American side of the Niagara, as it flows northward from its gorge across the low land that leads to Lake Ontario. The structure of the bar, well revealed in a gravel pit, is that of overwashed layers of gravel that slant backward, away from the lake area and toward the land border; that is, the bar was retrograded as the lake slowly rose. Gilbert's letters tell of the satisfaction he had on coming upon this part of the beach and finding that the arrangement of its layers gave independent confirmation of the inference as to retrogression that he had made many miles away to the eastward. He had the pleasure of explaining the beach and

its structure to a party of European and American geologists who made an excursion to Niagara during a recess of the British association meeting at Toronto in the summer of 1897.

But it was not only the shore lines of the extinct proglacial lakes that were shifted by the tilting of the region; the shore lines of the existing lakes show the effects of similar changes, and thus indicate that the tilting has been recently and is perhaps still in progress. The present Lake Ontario, which lies wholly southwest of an oblique northwest-southeast line of no tilting across its St. Lawrence outlet, has risen at its western end so as to drown certain valleys that were eroded with respect to its surface as baselevel before the final rise took place. The lower (northernmost) part of Niagara River is a fine instance of this kind; it is not a river proper but a narrow embayment, deep enough to be navigable, which half drowns the valley that the river had eroded across the lowland north of the Niagara escarpment before the southwestern end of the lake had risen to its present level in response to the tilting of its basin. A little farther east Irondequoit Bay, on which Gilbert had a skating adventure in his boyhood as already told, is a similar instance, although its cavity is not of so simple an origin as that of the lower Niagara. Lake Erie repeats the same story; and the little embayments near its western end, which Gilbert had acutely interpreted as drowned valleys in his Maumee Valley report of 1871–73, are thus explained. The most important of them constitutes the harbor of Toledo. The navigable depth of Detroit River, a matter of vast economic importance, is similarly accounted for. How far the light of a true explanation throws its illuminating rays!

A POPULAR ARTICLE ON THE GREAT LAKES

The first published reports of Gilbert's progress in his Great Lakes studies were brief and incomplete, but in 1888 he brought out a popular article[1] which summarized them admirably. It was seldom that he approached the general reading public in this manner; but his success in this instance was such as to make one wish he had oftener done so. His manner of presentation was graceful and pleasing, being intermediate between the sententiousness of some of his formal reports and the jocosity of his familiar letters. The following extracts are therefore presented as much in illustration of his easy style as in explanation of his results.

After noting that "rivers are the mortal enemies of lakes," and recounting the manner in which lakes are destroyed by their enemy rivers, he goes on:

> Nevertheless, in spite of this warfare of extermination, waged in all lands and through all time, there continue to be lakes, and so there must be in nature lake-producing as well as lake-destroying agencies. There are indeed many such, but a few only need be appealed to to explain the great majority of lakes, and the chief are upheaval and glaciation.

These chief agencies are then briefly explained. As to the first, the—

> great natural process of uplift and downthrow tends to produce lake basins, and . . . its tendency is opposed by the great natural process of erosion and deposition by rivers. The two are so nearly balanced that the scale is thrown to one side or the other by the accident of climate. Where much rain falls the rivers are powerful and prevail, sawing gorges through ridges as fast as they rise, building up the floors of valleys as fast as they sink. Where little rain falls the streams are weak, and the displacement of the earth's crust shapes the land into lake basins.

Hence lakes of this kind characterize recently disturbed arid regions. The effects of glaciation are then taken up.

> A glacier is aptly called a river of ice. Like a river of water it has an upper surface sloping continuously from source to goal, and like a river of water it rests on an uneven bed of its own shaping. When an aqueous river is suddenly deprived of its supply of water, there remain along its channel a series of pools recording the inequalities of erosion. When a great glacier is melted away the inequalities of its erosion are recorded in a chain of lakes.

The behavior of the ice sheet of the glacial period is briefly considered:

> Instead of flowing from a mountain down a sloping valley, it flowed radially from a central plateau of ice, with little regard for the slopes of the land over which it passed. We do not yet know the center of dispersion, but the ice entered our land as an invader from Canada. . . . Twice the van was pushed far into the domain

[1] Changes of level of the Great Lakes. Forum, v, 1888, 417–428.

of the sun, and twice it was compelled to retreat; but when the sun finally surveyed its reconquered territory, the land was no longer simply graven with a tracery of rivers; it sparkled with the sheen of innumerable lakes.

The next passage to be cited is of special interest, as it contains the first statement, and apparently the most definite statement that Gilbert made concerning the long-vexed question as to the origin of the basins in which the Great Lakes are contained. It is explained that in certain areas the ice sheet was—

a powerful agent of erosion, scooping out great hollows from the solid rock. For some reason not clearly understood the erosion was greatest along a zone parallel to the margin and a few hundred miles back from it, and here were formed the basins not only of the Laurentian lakes from Ontario to Superior, but of Winnipeg, Athabasca, Great Slave, and Great Bear Lakes. Within this zone of greater erosion the points of greatest erosion were determined chiefly by the pre-glacial shape of the surface. . . . How deep the original valleys were cannot be told, for the details of the old topography have been ground away, but we may be sure they were shallow compared with the existing troughs;

for the depths of some of the lakes "reach from three hundred to five hundred feet below the level of the ocean, and their origin cannot be referred to stream erosion without incredible assumptions as to continental elevation."

The remainder of the article is concerned with the gently inclined shore lines of the extinct proglacial lakes, and with the changes of those lakes as the ice melted back and the northeastern land rose toward its present level. Then follows a remarkable inference: Had this rise been continued to the amount of 3 inches in a mile more than the amount that is recorded in the slanting shore lines, it "would send a great river from Chicago to the Mississippi, reverse the current of the Detroit, stop Niagara Falls, and rob the upper St. Lawrence of seven-eighths of its water." Gilbert's later work showed that such a change is within the possibilities of the future, as will be told in a later section.

CHAPTER XVII

THE HISTORY OF NIAGARA RIVER

THE RETREAT OF THE FALLS: 1886

Gilbert's studies on the proglacial Great Lakes led him to look into the evolution of Niagara River and its great cataract, and this fine subject became peculiarly his own. The logical ingenuity with which he eventually combined many far-separated features of extremely unlike character and arranged them all in an orderly sequence which led to the birth of Niagara River and the retrogressive erosion of its gorge has hardly been excelled in the history of geological science; and the marvelous manner in which the various features were found to fit to each other when they were placed in the relations to which Gilbert assigned them is strong testimony for the essential accuracy of his views. Some of the features that he thus linked up with their proper associates had previously been fairly well explained in a local way by earlier investigators; others had not been properly understood even as individual problems. It was left for Gilbert's genius gradually to bring them all together in a comprehensive sequence and thus to make clear the place that each held in the evolution of the region. As was usually the case with Gilbert's studies, the field work was done by himself; he had no assistants. His methods were moreover almost exclusively physiographic; they had little to do with stratigraphy and nothing to do with paleontology or petrography; but they had everything to do with surface forms. It is also noteworthy that this research was given a strongly quantitative flavor, at first with regard to the erosion of the Niagara gorge and the recession of the Falls at its head, and later with relation to the modern tilting of the Great Lakes region; this quantitative flavor as well as the physiographic quality being strikingly characteristic of Gilbert's habit of thought and treatment. Unfortunately, the results gained were announced only in many separate papers; they were never brought together in the monographic form that they merited.

Gilbert's first important contribution to the Niagara problem was made at the Buffalo meeting of the American Association for the Advancement of Science in the summer of 1886, when he analyzed the many factors on which the recession of the Falls depends; thereafter Niagara River was his subject in many other communications. The idea that the Falls were worn back by the river was then already at least a century old; in one of Gilbert's later papers in which a historical review of the problem is given, it is noted that one McCauslin, who resided at the Falls from 1774 to 1783, had even then found that everyone believed the cataract to have been originally at the face of the upland escarpment which separates the higher basin of Erie from the lower basin of Ontario, and back from which the 6-mile gorge has been cut; and this early observer went so far as to propose and to discredit a curious method of determining the rate of the Falls recession: "If we adopt the opinion of the Falls having retired six miles, and if we suppose the earth to be 5,700 years old, this will give about sixty-six inches and a half for a year, or sixteen yards and two-thirds for nine years, which I can venture to say has not been the case since 1774"; but he does not go on to discover how old the earth must be if it has witnessed a slower retreat of the Falls, much less does he inquire how much earth history had elapsed before the Falls began!

All through the following hundred years it seems to have been generally assumed that the recession of the Falls had been at an essentially uniform rate; so that if the present rate were determined, the age of the gorge could be calculated by the "rule of three." It was Gilbert's peculiar merit to point out that the rate of recession depends on many factors, such as height of plunge, volume of water, and resistance of rocks, most of which may have varied during the excavation of the gorge; and although he did not then attempt to evaluate the effects of these factors, he showed clearly that not until they are evaluated can the age of the gorge be correctly determined. It may be recalled that an anonymous report of his communication in

Science erred in attributing to him a definite age for the falls. He did, however, point out that the chief recession was at the apex of the Canadian or Horseshoe falls, where the retreat is so rapid that the American falls seem not to recede at all in comparison; so rapid, indeed, that if the young couple whose "wedding journey" was described by Howells had, instead of behaving foolishly on an islet in the upper rapids, gone to the upland on the farther side of the river and observed the form of the horseshoe reentrant in a sensible manner, they might, if Niagara had been included in their silver wedding journey, have easily detected a visible change in the vertex of the reentrant on repeating their observation.

In this connection Gilbert, then or later, pointed out that at no very distant epoch in the future, the Horseshoe falls will, on receding above the head of Goat Island, draw off all the water from the American falls; and thereupon Goat Island and the dry channel behind it will repeat on a larger scale the features already found at Wintergreen flats, on the Canadian side of the gorge below the whirlpool; for there also the original river was divided into a larger and a smaller channel by an island; and there also, when the cataract receded to the island, it was divided into two falls of unequal breadth; and there the recession of the broader falls long ago accomplished just such a diversion of current from the narrower falls on the Canadian side as the broad Horseshoe falls of to-day will in the future accomplish with respect to the narrow and slowly receding American falls. It was by the introduction of realistic items of this sort that Gilbert made his account of Niagara so vivid.

A great advance was made when the volume of Niagara River, which Gilbert had treated as a possible variable in his analysis of 1886, was shown by another investigator in 1888 to have actually suffered a great diminution of volume for a considerable period by reason of the diversion of the discharge from the upper Great Lakes through a temporary short-circuit outlet that was opened across the Province of Ontario by the retreat of the ice sheet, before the post-glacial upheaval of the land in the northeast had raised that district to a higher level than the outlet of Lake Huron past Detroit; and at about the same time Gilbert himself discovered a second path of diversion of the upper lakes through another short-circuit outlet of later adoption farther north than the first. The problem thus became beautifully complicated. Unfortunately there is no adequate record of Gilbert's method of work upon it or of his progress; but those who occasionally worked with him believe that he was constantly as active in hypothetical speculation as he was in outdoor observation, and that his various speculations were continually tested by comparing their "consequents," as he called them, with appropriate facts. His observations were therefore not made at random; they were guided by what the sagacious Playfair called the "clue of theory," and were thus led to "those *instantiae crucis* . . . that exclude every hypothesis but one, and reduce the explanation given to the highest degree of certainty."

THE TORONTO LECTURE: 1889

But although his method of progress is not recorded, the conclusions that he reached after several years of work were most beautifully summarized in a lecture that he gave at the Toronto meeting of the American Association for the Advancement of Science in the summer of 1889. He wrote at that time to a friend: "Fair weather, the hospitality of the Canadians, and a rather good attendance conspired to make the meeting successful"; but he said nothing of his own lecture, which was a part of that conspiracy and a very delightful part. Its presentation was popular in the sense that various important conclusions, based on others' work as well as on his own, were presented briefly and without full proof, but he had full proof in reserve. The story told was fascinating in that it gave a brilliantly rational and coherent meaning to various matters of fact which, more or less known as isolated items, had never been fully understood in their relations to each other; and the story was all the more appealing because it centered upon the great cataract of Niagara, shared and enjoyed by the peoples on its two sides as the chief natural wonder of the eastern United States, during that period of American history when the West was the Far West and when its greater natural wonders were known only to the adventurous few. So lively indeed was the interest excited by the lecture that it was given

double publication; first in the Sixth Annual Report of the Commissioners of the [New York] State Reservation at Niagara, and again in the Forty-fifth Annual Report of the Smithsonian Institution of Washington.

The history of Niagara River, as presented in the lecture, involves an understanding of three main factors: The general relief of the region, the progressive retreat of the ice from it, and the upheaval of the land in the northeast during that retreat. The manner in which Gilbert analyzed the interaction of these factors excited universal admiration. As to the first, the region concerned owes its larger features to long-enduring subaerial erosion in preglacial time, as a result of which the Laurentian uplands of ancient crystalline rocks on the north were stripped of the sedimentary strata that stretched a certain distance over them, and thus came to be flanked by a series of broad and shallow lowlands excavated along belts of relatively weak, gently south-dipping strata, alternating with broad uplands that survive along belts of more resistant strata. One of the upland belts is maintained by the so-called Niagara limestone, which falls off in a well-defined north-facing escarpment in western New York and the adjacent part of Ontario; a second upland of greater altitude but not so closely related to the Niagara problem is the Appalachian plateau, which begins in the southern part of New York and the adjoining part of eastern Ohio and extends far southward. Of the two lowland belts, the one north of the Niagara upland belt is now occupied in part by Lake Ontario and by the Georgian Bay extension of Lake Huron; the one to the south is in part occupied by Lake Erie.

Gilbert does not appear to have expressed any opinion as to the preglacial form of these belts, other than that noted in the preceding section, probably because that problem still remained largely indeterminate; but he made it clear that, as already explained, there was a time in the retreat of the ice sheet when it obstructed the Mohawk Valley between the Adirondack Mountains and the Appalachian plateau on the south, as well as the St. Lawrence Valley between the Adirondacks and the Laurentian highlands on the north; and that a great proglacial lake then had a southwestward outlet which flowed to the Mississippi. The lake surface then stood at so high a level that not only were the two lowland belts submerged as far as they had been evacuated by the retreating ice, but even the Niagara upland between them had a hundred feet of water over it. But after a time the continued retreat of the ice margin opened a passage between the Appalachian plateau and the Adirondacks—a later chapter will tell of the dramatic incidents of this opening as other of Gilbert's discoveries—and as this passage was lower than the southwestward outlet that led to the Mississippi, that outlet was abandoned and an eastward discharge was adopted in its place. The lowering of the great proglacial lake that followed was sufficient to lay bare the Niagara upland; thereupon the single proglacial lake was divided into two smaller ones; a higher one occupying the basin of Lake Erie on the southwest of the upland; and a lower one, the Lake Iroquois already noted, the precursor of Lake Ontario, on the northeast of the upland. The higher lake selected the lowest points of the upland for its point of discharge to the lower Lake Iroquois—the manner of selection was detailed in another of Gilbert's later discoveries—and thus the Niagara River came into existence. How far this sequence of events had been established by others need not be here inquired into; it was never so clearly set forth as in Gilbert's lecture at Toronto.

It must be remembered that when Niagara River was thus born the land to the northeast was lower than now, for at present the shore lines of all the extinct lakes gradually ascend in that direction. The river was of large size, for the upper Great Lakes appear then, as now, to have drained by the roundabout Detroit channel into the Lake Erie basin. But because of the lower stand of the land in the northeast, the Lake Erie of that time did not extend so far to the southwest as that lake does now; hence the outlet of Lake Huron southward and eastward was not only a longer river than the Detroit River of to-day, but eroded its channel with respect to a lower baselevel than that of the present Erie. It is the flooding of this deeply eroded channel by the backward extension of Lake Erie to its present size, as the land rose in the northeast, that has made the Detroit River navigable, as above noted.

The young Niagara, cascading over the north-facing escarpment of the limestone-capped upland, at once proceeded to cut back its gorge and continued to work valiantly at this task until the gorge was some miles in length. In the pre-Gilbertian studies of the age of Niagara Falls, it had been tacitly assumed, as has been pointed out above, that the river had been of constant volume and that the lengthening of the gorge by the recession of the Falls had gone on at a constant rate; under these assumptions the age of the Falls could be determined by dividing the length of the gorge by the retreat of the Falls in a single year. Gilbert's method was more circumspect. He had in 1886 searched out every factor that is involved in the erosion of the gorge, and then and later he scrutinized every factor to learn how far it may have varied. The stratified rocks of the Niagara upland were found to be remarkably constant through the whole length of the gorge, except that at the Whirlpool the river had found a previously eroded and drift-filled valley, concerning which speculation had been active before Gilbert's time. The recession of the Falls during the reexcavation of this valley must have been rapid. The height of the Falls has long been decreasing, not only because of the gentle southward dip of the capping limestones and the southward rise of the rapids gives the retreating Falls a smaller and smaller measure, but also because at an early stage of their history Lake Iroquois was, by reason of the small amount of land uplift in the northeast then accomplished, for a time lower near its western end than Ontario is now; hence at that time the river north of the escarpment deepened its valley with respect to the lowered level of the lake into which it ran, and the Falls plunged down into the deepened valley. The subsequent invasion of the deepened valley by the waters of the present Ontario, as the uplift of the land in the northeast caused the lake waters to rise toward the southwest, has already been told. A variation in the height of the Falls being recognized, it was inferred that their recession would have been more rapid when the plunge pool beneath them was excavated in weak strata than when it was excavated at least in part in more resistant strata.

VARIATION IN THE VOLUME OF NIAGARA

But the chief cause of variation in the recession of the Falls was found in a variation of river volume. So long as the ice sheet lingered over much of the lowland north of Lake Ontario the drainage of the upper lakes could find no cross-country outlet to Ontario and the St. Lawrence Valley, even though the lowland was at that time, in consequence of the small uplift of the northeastern area then accomplished, lower than the Detroit outlet. But when the ice margin withdrew sufficiently, the lower stand of the lowland permitted the upper lakes to short circuit their discharge across it by the so-called Algonquin River or Trent outlet, running from Georgian Bay of Lake Huron to Lake Ontario, as told above; and thereupon Niagara River, receiving only the drainage of the Erie basin, must have been greatly reduced, and the recession of the Falls must have been correspondingly slow. With the farther retreat of the ice, a second and a more northern short circuit, known as the Nipissing outlet, was opened at a still lower level in the then attitude of the land, and thus the reduction of the Niagara to small volume was prolonged. Whether an intermediate epoch of roundabout lake discharge by Detroit temporarily gave Niagara its full volume was not then determined. But eventually the continued rise of the land in the northeast raised even the northern short circuit to a higher level than the Detroit channel; then the roundation discharge was resumed, Niagara regained its full volume, and the recession of the Falls went on at a livelier rate again. And so the recession has continued ever since. Although the southern of the two short-circuit discharges had been found and its significance recognized by another observer before Gilbert came upon it, he traced both short circuits along parts of their length, and briefly described the features by which they are shown to be abandoned river channels; and he eventually made it clear that while the volume of Niagara River was diminished, its gorge was cut to a less width and less depth than before; but that after its full volume was restored, the gorge was again cut to a full measure of width and depth. Thus the narrows of the gorge where two great railroad bridges cross it above the Whirlpool are accounted for as the work of the diminished river; while the wider uppermost part of the gorge and the deep pool there evacuated are the work of the increased river in its thundering plunge of a full-volume cataract. Where can a more strik-

ing example be found of the interaction of many factors in the explanation of a great natural feature of the earth's surface than in the evolution of Niagara Falls? Where can the significance of the surface forms and the importance of accounting for them be better illustrated than in the narrowing and widening, the shoaling and deepening, of the Niagara gorge? And yet a still further refinement in the analysis of Niagara's history was made several years later.

The reader of a convincing explanation sometimes gains the impression that the problem treated is wholly solved. Gilbert's lecture, convincing as it was as far as it went, was so contrived that it should leave no such impression of finality, for near its end a list of 16 unsolved questions is presented, and it is then remarked:

> The tale of the questions is not exhausted, but no more are needed if only it has been shown that the subject is not in reality simple, as many have assumed, but highly complex. Some of the questions are, indeed, easily answered. It may be possible to show that others are of small moment. . . . But after all paring and pruning what remains of the problem will be no bagatelle. It is not to be solved by a few figures on a slate, nor yet by the writing of many essays. . . . It is a problem of nature, and like other natural problems demands the patient gathering of many facts, of facts of many kinds, of categories of facts suggested by the tentative theories of today, and of the new categories of facts to be suggested by new theories.

It is presumably because Gilbert so fully recognized the complications of the problem that he refrained from—one might say, resisted the temptation of—giving any numerical estimate of the age of the Falls. To a man of smaller caliber the temptation would have been irresistible.

THE HUMAN ELEMENT

The last page of the Toronto lecture contains one of its most interesting if not most important announcements. Early man was a witness of early Niagara.

> On a gravelly beach of Lake Iroquois . . . he rudely gathered stones to make a hearth, and built a fire; and the next storm breakers, forcing back the beach, buried and thus preserved, to gratify yet to whet our curiosity, hearth, ashes and charred sticks. In these Darwinian days, we cannot deem primeval the man possessed of the Promethean art of fire, and so his presence on the scene adds zest to the Niagara problem. Whatever the antiquity of the great cataract may be found to be, the antiquity of man is greater.

It is pleasant to know that to Gilbert's keen observation the discovery of this ancient hearth was due.[1]

[1] The geologic history of a prehistoric hearth. Amer. Anthrop., ii, 1889, 173–176.

CHAPTER XVIII

A TRIP ABROAD

SCIENTIFIC MEETINGS IN ENGLAND

Gilbert's only visit to Europe was made in the autumn of 1888. The journey was at his own expense, and was undertaken chiefly for the purpose of attending the Fourth International Geological Congress in London. He sailed from New York, in company with several other American scientists, on the *City of Berlin*, August 24, and reached Liverpool on September 3; return was made two months later. He called the journey "a jolly trip." On landing Gilbert went, according to the record in his diary, at once to Bath and there attended a meeting of the British Association for the Advancement of Science, September 4 to 14, after which a week end was spent at "Rushmore," the country seat of General Pitt Rivers. Here two pages of the diary, covering the days from September 15 to 18, appear to have been skipped by accident, thus making the following entries four days late; for according to his record London was not reached until September 21, whereas according to the published proceedings of the congress he was probably there on the 17th and surely on the 18th. Again, three extra days appear to be credited to London at the end of the congress, so that the date of setting out on a post-congress excursion is a whole week too late. The error thus introduced appears to be continued to the end of his stay abroad; yet he did not miss his steamer for the return voyage, although his record of the date of sailing, November 7, should have been October 31. It would seem, therefore, that he must have kept account of his time by the weeks as they passed instead of by the day of the month in the diary. The diary record shows only 4 days instead of 11 for the return voyage, yet no correction of the error is entered even there. In the following transcript, the corrected dates are given.

Gilbert's first stay in London was during the session of the geological congress from September 17 to 22. He then took part in an excursion to the Isle of Wight, September 22 to 29, and visited the office of the ordnance survey at Southampton on the way, after which he returned to London again, crossed the Channel and spent three days, October 1 to 3, in Paris. On returning, 2 days were given to London, 7 to Ireland and 10 to Scotland. London was reached for the fourth time, October 25 to 27, and Oxford for the first time, October 28 to 31. Liverpool was left on the *City of Chicago*, November 1, and New York was reached November 11. It is interesting to note the cost of this trip: Passage out and back, \$110; two months in Great Britain, including three days in France, \$304; purchases, \$124.40; total, \$538.40. In view of the distances covered during the 58 days ashore, an average of but little over \$5 a day for traveling suggests that expenses were prudently guarded. Ten days as a guest of the city of Bath and a week end at a country house may have aided in keeping expenditures down; but the acceptance of the latter form of hospitality is not always consistent with economy.

Gilbert made no communication to the geological section of the Bath meeting, but spoke on the topographic map of the United States, the first sheets of which were then about to be issued by our national Geological Survey, before the geographical section; for unlike its American counterpart, the British association has long had enough travelers, explorers, geodesists, and hydrographers among its members for them to have, with the occasional addition of a geographer, a section all to themselves. He was much pleased with the general character of the meeting and his presence appears to have given, with perhaps one exception, equal pleasure to those whom he met. One geologist still recalls that he was strongly impressed with the fine face of the American as he sat attentively listening to the proceedings of a sectional session. Another tells that the visitor was welcomed as one of the most distinguished of American geologists, and looked upon as a representative of the British race in America, a link between the two countries, and was therefore received as "one of the family." But a

third, who was studying certain British laccoliths at the time, appears to have been somewhat disappointed; for when he ventured to put a question on his problem, the distinguished originator of the laccolith idea "was not a bit interested, and professed to have been so much occupied with other things since his Henry Mountains work that he had forgotten all about it." Apropos of this disappointing story, it may be noted that some 20 years later Gilbert remarked to a friend that the British geologists had praised him much for very little work on his part in this matter: "The laccolites were there for the seeing." But it may be doubted if he would have made that comment had anyone expressed admiration for his discussion of the physical conditions of laccolithic intrusion; that surely had to be worked out. Another sequel to the same story came 10 years later still, as will be told in due time.

The field excursions at Bath in connection with the geological section of the association pleased Gilbert greatly; he later wrote to a geologist who was concerned with planning excursions for a meeting of the American association:

> Our Association excursions are usually chaotic affairs, without adequate provision and without guidance. The British Association excursions are far superior. They are elaborately planned in advance, and each as a rule is provided with two functionaries, a leader and a whipper-in. It is the duty of the leader to direct the movements and to explain matters of interest or to see that others do so. It is the duty of the whipper-in to bring up the rear and look after the stragglers. Thus managed, excursions have definite purposes and accomplish them.

However hearty was the welcome given to the "representative of the British race in America," he appears to have retained an American point of view while in Britain, for certain formalities there in vogue seemed to him overconventional. He wrote his impressions on this matter to a friend:

> The giving of thanks is a flagrant example . . . The chairman makes a speech and introduces the lecturer; the lecturer gives his lecture; the chairman makes a little speech and introduces Mr. A.; Mr. A. makes a speech and moves a vote of thanks; the chairman introduces Mr. B.; Mr. B. makes a speech and seconds the motion; the chairman puts the motion; the lecturer makes a speech to thank the audience for thanking him and Mr. A. and Mr. B. for moving the vote; the chairman dismisses the audience in another speech, if he can think of anything else to say.

He was much amused by the formalities of a banquet given by the city of Bath, and presided over by the right worshipful mayor, behind whom stood a professional toastmaster and a red-coated trumpeter:

> We found our chairs and stood behind them. The trumpeter tooted. The toastmaster, intoning his words, shouted, "Silence, gentlemen, grace will be said." Canon Somebody said a ten word grace. . . . Whenever the toastmaster had anything to say he was preceded by the trumpeter.

There can, however, be no question that these devices are effective; for the resounding flare of the trumpeter causes immediate silence even in a gathering of hundreds of lively talkers around the festive board; and the toastmaster, on stentoriously repeating the toasts proposed by the chairman, whose voice may be feeble, insures the audibility of every word; nevertheless the devices cause surprise in most Americans when they are first seen and heard.

An American should, however, feel no surprise if one of his countrymen, who has spent a good part of his life in western deserts and waited till his forty-sixth year before going abroad, confesses to being somewhat overpowered with the attentions showered upon him by the municipality where he finds the British association holding a meeting; for Gilbert's experience at Bath was not a repetition of anything he had enjoyed or suffered from in any city where he had attended the meetings of the American association. But a British reader of this page may perhaps wonder why such an American should hesitate to tell of it, if any American municipality had ever shown him similar attentions, as is intimated in the second line of the following extract:

> It seems a little strange to be a foreigner. "Prophets are not without honor, save &c." No American city ever boarded, lodged, wined and carriaged me for a week—or if it did I am not going to tell of it. Never before have I assisted a baroness over a stile, and afterwards found a nobleman's card awaiting me at my lodgings. It is quite overpowering.

But like an Afrikander visiting the home country after a life in the open veldt, Gilbert was not pleased with certain English restrictions; he did not like the high walls around—

all small private enclosures. A town street is a box-cañon with occasional doorways through the walls. . . . The habit of barring out the wayfarer is a mere survival of lawless times many centuries ago, and our conservative cousins are simply slow to discover that they have become civilised. The mere neglect of the 10-foot walls will not destroy them in a generation for like everything else they are built for eternity.

A WEEK-END AT A COUNTRY SEAT

Gilbert wrote the following account of his week-end at "Rushmore."

It is the house of the country gentleman of whom we have all read in English novels—the owner of all the surrounding landscape. . . . Rushmore proper, the country place in a single enclosure with gates and porter's lodges at its various exits, is perhaps a square mile in area, but the entire estate . . . includes 35,000 acres. The stone house is but two stories high and not very imposing but it covers much ground and I think thirty guests must have lodged here night before last. The place is full of the choicest things. Modern and ancestral paintings, armor, vases, busts, elegant furniture and quaint furniture. The grounds are a park with forest and copse as well as lawn. In them roam not only cattle of various choice breeds but fallow and Japanese deer and llamas. In smaller enclosures are pheasants, parrots, kangaroos, marmots. The table bears at dinner time such a display of gold plate as I have never seen before and the meats are served on silver. The order of meals is breakfast 9–10 (a singularly informal meal), tea at 2, and dinner at 7.45. . . . General Pitt Rivers is an archeologist and antiquarian of the scientific type. He has dug over the site of an ancient village near by and recovered everything that could be found to trace its history, finding it to be a British village occupied during the Roman occupation. He has also explored at great expense and monographed a pre-Roman fortified camp. And he has spent the day showing these places to his guests. The vases, implements, ornaments and skeletons from these places form the nucleus of a museum devoted to ethnography and most thoroughly arranged and labelled. He has raised a local storm by throwing it open on Sundays. We have also seen today a park of a few acres tastefully arranged and practically dedicated to the laboring people. One of its features is a music stand where an orchestra of about fifteen pieces play every Sunday from 3 to 5—to the disgust of the clergy. The members of the orchestra are all laborers from Rushmore, but they made very fair music. . . . Am I not a fortunate Yankee to have this glimpse of high life dropped into my path. . . . I am sure that I am enriched by the experience.

A few days later he naïvely wrote:

Already I feel very distinctly the broadening effect always wrought by foreign travel.

IMPRESSIONS OF A LONDON CLUB

On reaching London, Gilbert found among his letters "an invitation to the Athenæum Club, an honorary membership for 30 days," and the amusing account of an evening that he spent there is quoted below; but the account needs two comments. In the first place, the equation intimated in his opening line between the severely exclusive Athenæum Club of London and the genial meet-you-more-than-halfway Cosmos Club of Washington is not likely to satisfy a London reader who happens to know something of the American capital, and still less a Washington reader who has wished to know something of the British capital; and next, the desertlike silence which Gilbert encountered in the Athenæum, when he went there unannounced, alone, and unknown, ought to have set over against it the enthusiastic greeting accorded to John Fiske by the Cosmopolitan Club after he, 10 years earlier, had made himself most acceptably known in London by his lectures on America, for on that occasion a London club actually thawed. Nevertheless, the desolating silence of one occasion is as true to life as the rare enthusiasm of the other; for even Punch has pictured the cold and repellant stare with which London club men know how to intimate to a guest that he is something of a trespasser. Gilbert's account follows.

As the Athenæum is the Cosmos of London I was glad of an opportunity to see it and determined to do so that very evening, though I had no reason to expect to meet my friend there at that time. So I arrayed myself in my coat of ceremony, called a cab, and went "to dine at the club." As the Athenæum has 1200 members, all distinguished, I was prepared to find a larger house than that of the Cosmos but I was not prepared for the actual magnificence. Spacious rooms, high studded, stuccoed, fretted, frescoed, gilded, decorated with statues and paintings. In the dining room a row of small tables all about the sides, each for one person. Twenty or thirty gentlemen were eating while I ate, all deliberate, calm, dignified and self-sufficient. In only a single

instance did I see one of them speak to another, & I of course assumed to address none but the servants. Then I strolled through two reading rooms and up the grand staircase to the library occupying four or five large rooms above. There were readers and writers in all the rooms and I lingered to read and even wrote a letter, but I saw no person speak to another, except that occasionally a servant came to tell a gentleman that his dinner was ready. Oppressed by the silence I continued my promenade looking for a smoking room, for there if anywhere the English club man must thaw out. For a time my search was in vain, but at last I asked the doorkeeper and was directed to the basement where smoking is permitted in a card room and a billiard room. The card room contained five gentlemen, four of whom were engaged in a game. The billiard room has but a single table where two men were playing and a third observing. I watched the game, which is very different from the American, and when it was finished the players invited the observer and me to join them in a four-hand game which we did. This is all I know of the English club, but I strongly suspect that it spells "social" with a small "s."

On reading this lugubrious description, one must wonder whether Gilbert did not compare the elegant isolation of that evening in the vast wilderness of London during his first trip abroad with the forlorn desolation that he had seen in a frontier city of Arizona 17 years before during his first season in the wilderness of western America; and whether he did not feel about as lonesome amid the decorated splendor of the spacious clubrooms in the greater city as in the unredeemed squalor of the Free Press office in the smaller one. But London must have been more agreeable the next morning, when he attended the International Geological Congress under the presidency of Prestwich, in what was then the University of London, part of a rectangle of buildings known as Burlington Gardens, off Piccadilly; rooms near by being occupied by the Royal Society and the Geological Society, while the Geological Museum, with the office of the Geological Survey, as well as the building of the Royal Geographical Society, were in the immediate neighborhood. He must surely have found a hearty welcome there.

The Congress was attended by 240 members from 25 countries; Gilbert was listed among them as a "Prof." The proceedings indicate that he spoke only once, when he urged that the lower Paleozoic should be divided into only two systems, one of his reasons being that there would be hardly enough colors available for uniformly colored international geological maps if more divisions were established; but his opinion was not followed. Personal records concerning this meeting are unfortunately wanting, as they are concerning the following excursion to the Isle of Wight, and concerning Gilbert's own travels in Ireland and Scotland; his diary indicates, however, that he went far enough in the latter country to see the "Parallel Roads" of Glen Roy, to which he afterwards referred in the Bonneville monograph as showing no more change by erosion than the Bonneville shore lines. As to the visit to the office of the ordnance survey at Southampton, a letter briefly notes that his object was especially to learn "the methods of making topographic surveys in the field," and there as well as in Paris his visit "led incidentally to the gathering of information of value to the Geological Survey." It was shortly after this trip abroad and apparently in consequence of what he then learned that he advised Powell against making over the hachured maps of earlier surveys into contoured maps to be published by the United States Geological Survey, but unfortunately his advice was not accepted in this matter and in consequence of its nonacceptance some extremely inaccurate contour maps were issued.

THREE DAYS IN PARIS

If Gilbert felt himself a stranger in England where the language was his own, he must have been completely a foreigner in France where the language was wholly unintelligible to him; yet he was there given the opportunity of penetrating French home life to a degree that is rarely opened to visitors. During his brief stay in Paris he was the guest, at their residence, 132 Rue de Grenelle, in an old and aristocratic quarter of the city on the "left bank" of the Seine, between the Chamber of Deputies and the Hotel des Invalides, of a family distinguished in its own country by the accomplishments of its elder members in literature and diplomacy, and known to all the world of geology and geography by the generous erudition of a younger member. Indeed, this actual entrance of Gilbert, truly American explorer of western deserts, into the household of the purely French de Margeries, not one of whom he had ever seen before, seems more fabulous than the storied reception of Christopher Newman, same-aged American maker of washtubs, at the "hôtel," Rue de l'Université, of the exclusive de Bellegardes!

There was no session of any geological or geographical congress to attract Gilbert to Paris; it would seem as if his wish to make the personal acquaintance of Emmanuel de Margerie, with

whom he had been in correspondence for a few years, was his chief reason for crossing the Channel. The wish was delightfully gratified, for as just told de Margerie opened his home to the American visitor, and exhibited to him the remarkable collection of books and maps there gathered. Like others to whom that privilege has been graciously extended, Gilbert was deeply impressed not only with the wealth of the collection but still more with its owner's intimate knowledge of its contents. He later asked the Chief of Engineers in Washington to send the maps of the Mississippi River Commission to his friend in Paris, explaining that "Mr. de Margerie is better informed in the geography and geology of the United States than most Americans, not excepting members of the geographical and geological surveys." While in Paris Gilbert was accompanied by de Margerie, than whom there could be no better guide, to various Government offices where geological and geographical work was carried on. The rooms on the Boulevard St. Michel near the Jardin du Luxembourg, occupied by the "Service de la carte géologique de la France," and crowded with records and portfolios, seemed disappointingly insufficient when judged by American standards; and the methods of map production there in vogue did not tempt imitation. On the other hand, the visitor was much impressed by the maps and models in the bureaus of the "Service géographique de l'Armée." He was there introduced to Col. G. de la Noë—afterwards general and director of this service—and was pleased to find on the small bookshelves near the colonel's desk a copy of the "Geology of the Henry Mountains"; all the more so when he was told that the photo-reliefs in that report had inspired de la Noë and de Margerie to introduce in their joint work, "Les formes du terrain," published a short time before, similar reliefs of districts in France; a sample plate was given him representing the former course of the Moselle when, instead of turning as now northeastward at the Toul elbow of capture on the way to the Rhine, it followed a meandering valley northwestward through a well-defined cuesta to join the Meuse near St. Mihiel, a locality more famous in the twentieth century than in the nineteenth; it doubtless then gave to that now diminished river the force that it must have greatly needed to maintain its antecedent course farther downstream across the rising mass of the Ardennes. But as the French officer understood no English and the American geologist no French, conversation between them was rather laboriously carried on through de Margerie as interpreter; and intercourse of that kind seldom leads to intimacy. Probably for this reason, Parisian personalities appear to have been less interesting than French processes. Gilbert was much impressed by what he saw in the rarely entered workshops of the army geographical service, secluded in the attics of the Hotel des Invalides, where he inspected the making of delicately carved reliefs of significant French areas; and also in the bureau of printing of the same service, where the substitution of zinc plates for lithographic stones seemed to him a practical advance. It was in view of these novelties that he exclaimed to his guide: "Our survey ought to have a permanent representative in Paris for information on these matters, and the representative ought to be you"; but no official action in Washington seems to have followed from this enthusiastic outburst.

Probably owing to his lack of knowledge of French, Gilbert met few scientific men in Paris, visited no laboratories in the Sorbonne, and attended no meetings of scientific societies; but it is remembered that he ascended to the belvedere on the roof of the de Margerie residence to enjoy the panorama of Paris there disclosed; and after his friend pointed out the Arc de Triomphe rising over the houses on the higher ground across the river, and the Cathedral of the Sacré Coeur, then in construction on the Butte Montmartre farther away to the north, he brusquely exclaimed: "What a worthless use of money!" It might be inferred from this remark that his positive spirit, previously concentrated upon the geology of the Rocky Mountain region and never concerned with the historical and moral evolution of Europe, was awakened to no warm enthusiasm on viewing the superb monument erected by an emotional people to the glory of their arms under the great Napoleon, and remained unmoved at the sight of the growing edifice dedicated to worship by fervent believers in a mystical faith; and so his remarks appear to have impressed his host. But those who, overlooking his not infrequent brusqueness of manner, knew him well enough to appreciate the depth and seriousness of his inner nature, may perceive in his apparently ungracious words not an expression of coldness in the presence of architectural exultation, or of indifference in the sight of ecclesiastical aspiration, but an earnest and unrestrained protest against the glorification of war in an age when war should be condemned as barbarous, and against the perpetuation of ancient beliefs unchanged in an age which is coming to be more and more converted to a philosophy of evolution in religion as in everything else.

CHAPTER XIX

THREE YEARS AS CHIEF GEOLOGIST OF THE NATIONAL SURVEY: 1889–1892

CHANGE FROM SCIENTIFIC TO ADMINISTRATIVE DUTIES

It is certainly desirable that the work of a national scientific organization shall be well administered; and a member of such an organization may therefore sometimes feel the necessity of subordinating his preference for scientific work to the loyal execution of administrative tasks that are placed upon him. When as a result of such necessity a man who combines an exceptional capacity in original investigation with a large measure of wisdom in the conduct of practical matters is compelled to stop his investigations in order to apply his wisdom to the determination of important lines of policy and to the solution of serious problems of management, his judicious-minded friends may find, in the safe conduct that he helps to give to a great institution, some compensation for the cessation of his researches; but when much of his time has to be given to a multitude of petty details, the judicious minded are disposed to grieve rather than to rejoice. So it was when Gilbert, a born investigator and a wise counsellor, was diverted from his problems of preference and given, in 1889, the honorable but burdensome appointment of chief geologist; his friends had grounds for regret as well as for rejoicing. However, he accepted the appointment loyally and worked under it conscientiously for three years, yet not with full satisfaction; and many who had enjoyed and profited from his geological studies were disappointed at his distraction from them.

Those who recall the first 10 years of Powell's administration of the national survey know that period as one of extraordinary expansion. The survey was organized for the study of the "public domain"; it soon grew to cover the whole of the United States. It was at first, as a geological survey, charged only with the preparation of a geological map of the country; but as no topographical maps were to be had on which the geology could be represented, a topographic branch was organized in 1881; in 1883 that branch had 49 skilled members, besides many untrained employees, and in 1889 it had 84 skilled members. Again, the separate departmental surveys of earlier years had entrusted the printing of such maps as they issued to outside engravers and lithographers; Powell soon had to face the necessity of organizing a bureau of engraving within his survey building. Little wonder that when it was proposed in 1889 to add irrigation to geology, the director found that he could not attend personally to all his many responsibilities, and decided that some of the able men on his staff must take a large share of them.

His first proposal in this emergency was that irrigation should be made a division of the survey in charge of Gilbert, who had already been assisting the Director in many administrative tasks and who on February 3, 1889, wrote to a friend about the offer as follows:

> The proposition is very attractive . . . because the investigation is a great one, because the problems have a direct human interest, because it offers out of door work, because it gives me a profession by which I can earn a living after the U. S. discharges me, because the western fever is more or less chronic with all who have stridden the occidental mule. Per contra, I am doing and fostering many scientific works with which I am loath to part, and I don't know but my boys need me more at home than western work would permit.

Two weeks later, after a decision against his taking charge of irrigation had been practically reached, he wrote again:

> The discussion of the matter has developed two things that are gratifying to me, and as they are highly personal you must take them as confidential. One is that the administrative work I have been doing has been so acceptable to those concerned that many of them have interviewed Powell to urge my retention here. The other is that Powell cannot readily fill to his own satisfaction the places that would be vacated by my transfer to irrigation. The outcome is likely to be that I shall be given general charge of the geological part of the Survey—coordinate with the paleontological and geographical divisions.

Powell announced his action in the matter in the tenth annual report of the survey. He there stated that the director found it impossible to give the necessary time to problems of a strictly geological nature, and so—

determined to delegate a portion of his administrative supervision. Questions arising in connection with the work in geology, paleontology, topography, etc., were referred from time to time for consideration and determination to chiefs of divisions and other officers of the survey, and especially to Mr. G. K. Gilbert, geologist in charge of the Appalachian division and of the division of geological correlation. In order to give Mr. Gilbert more time for such duties he was relieved from charge of the Appalachian division, and it was finally determined . . . to place under his charge all of the divisions (12 in number) constituted for purposes of geologic research.

Thus after serving as Powell's most trusted counsellor since 1881, and after practically acting as chief geologist from the beginning of 1889, Gilbert was officially given that title in the following July, and thereupon became essentially the director of the geologic branch. The appointment had the appearance of a promotion, but scientifically it was a demotion. To assign a man of Gilbert's exceptional originality in investigation to administrative tasks was, as a noted educator said when a preacher of remarkable power was "promoted" to be the bishop of his diocese, "like caging an eagle." For the following three years Gilbert did comparatively little work of his own; the nature of the work that he did for the survey may be gathered from the following paragraphs.

THE LARGER DUTIES OF A CHIEF GEOLOGIST

It was for the chief geologist in consultation with the divisional geologists to determine problems and areas for field study, to consider plans and to advise in the choice of men to conduct the studies, to settle the amount of money to be allotted to each problem as well as the reallotment of unexpended balances, and to know the quality and quantity of the results so closely that he could either hold the slower workers to the completion of their undertakings, or make just proposals for the promotion of the more active and successful workers as their tasks advanced. He had also to consider applications for positions in the geologic branch of the survey, and all letters of commendation were therefore first submitted to his examination. One such letter seems to have amused him, for he wrote to its author:

Your recommendation of Mr. ——— is couched in such glowing terms that I tremble for him, it sounds so much like an obituary notice.

It is not to be doubted that many a geologist who is now some 30 years older than when his fate rested largely in Gilbert's hands would enjoy, or profit by, reading what was then said of him.

Here, for example, is a delightful illustration of the critical and cordial way in which the chief geologist advised the director of the survey regarding the increase of an assistant geologist's salary:

Mr. ——— gives but a small portion of his time to the work of the Survey and receives compensation only for the time thus given. His geological work is of the first quality, and such administrative work as he has executed for the Survey has been eminently efficient. He has employed with signal economy the small amount of money allotted for the investigation placed in his charge and has so directed the work of a corps composed chiefly of young men that it has been productive of a large body of valuable results.

If the subject of this commendation can identify himself in it, he may reasonably wish to have it included in his own biography when the time for that writing arrives.

Two of the most important tasks of the chief geologist concerned geological maps and correlation papers, both of which are treated in special sections below. It was also his duty to confer with the heads of State surveys and to advise the director of the national survey on the delicate business of cooperating with them. In one case where Gilbert deemed such cooperation inexpedient, he frankly explained his opinion in the matter to the geologist of the State concerned, as follows:

With a strong desire to assist you, and to do it in your own way if possible, we find that our ways are so different that we cannot fall into yours without so far deranging our plans that the assistance would not be really cooperation.

In matters of this kind as in all others, Gilbert always strove to reduce disputes, for he held that "controversy is bad for science, because with many people its quarrels will make a deeper impression than its achievements." He always conducted the administration of his office in a purely impersonal and scientific spirit; it has been well said of him that he never played politics, and never practiced that sort of diplomacy which shades off by insensible degrees into duplicity. He enjoyed giving encouragement, but the expression of censure was disagreeable to him. Yet he could be direct and definite enough, if in his judgment the occasion required it. In one instance he was under the necessity of instructing a geologist, whose office work was far behind his field work, to finish his old problems before taking up new ones, and his letter on the subject was direct and specific. One of his first official acts was to see that certain existing appointments should be brought into accordance with the law governing those matters; for it appears that some of the survey geologists had assistants who were classed as "messengers" and who as such had not been required to pass a civil service examination, but who were in reality acting as clerks and as such should have been examined before their appointment. Although Gilbert's official correspondence was as a rule gentle and genial, in this exigency his letters peremptorily instructed the offending geologists to discharge their "messengers" at the end of the month.

It was not alone the geologists and assistant geologists of the survey who received helpful suggestions from the chief geologist; the director himself was repeatedly aided by good counsel from his chosen lieutenant. Thus, although scientific results interested him much more than the administrative plans by which the results were attained, it was Gilbert who advised Powell to omit all mention of the outcome of geological investigations from the administrative reports which form the first chapter of the annual survey volumes, and to limit such chapters solely to brief statements of official action; and it will be told later that, when various practical matters concerning the preparation of geological maps came up for decision, it was largely Gilbert's sagacity which determined the policy of the survey regarding them, although Powell, who was finally responsible, properly announced as his own the policy that he had adopted largely on Gilbert's recommendation. In one matter, however, Gilbert's judgment was not followed; this was, as above noted, the question of transforming the shaded or hachured maps of some of the earlier departmental surveys into the contoured maps of the national survey without revision in the field. Gilbert advised against so fictitious a topographic method, but unfortunately another opinion prevailed and some very poor contour maps were published.

CRITICISM OF MANUSCRIPTS

One of the most fatiguing of the chief geologist's tasks was the revision of manuscript reports, preparatory to advising their authors concerning the changes that seemed desirable before publication. It was probably his experience in these quasi-editorial duties that caused him to reply to a college teacher who asked his advice as to the subjects which should be emphasized in preparing students for work on the survey:

> Their geology is all right; teach them to write better English.

An excellent example of his manner in suggesting rather than in ruling that a submitted manuscript needed further attention from its author is found in a letter to a divisional geologist, as follows:

> The manuscript is pervaded by the originality of your amanuensis, and I fear that our editor, in eliminating that, may fail to attain that combination of accuracy and grace which would result from your own careful revision.

Surely the geologist who submitted the manuscript can not have had a heart so like the rocks he recommended for highway construction as to withstand such an appeal. "Originality of your amanuensis" is a charming phrase!

Gilbert's careful consideration of the feeling of authors whose manuscripts were adversely criticized is well shown in a letter addressed to a semiattached worker, when a report was sent back to him after it had gone through the survey mill:

> Having expressed my views with all possible freedom above, it occurs to me that you may possibly not be used to it, and so I add, by way of explanation, that it is a widely prevalent custom in the Survey for authors to abuse one another's work before it is printed just as much as possible. We believe that in that way we avoid the necessity of a certain amount of criticism afterwards.

What is here called "abuse" was well enough when it was performed by men of Gilbert's even temperament, but when undertaken by men of a more retaliatory disposition it may possibly have sometimes led to excess.

Criticisms of survey publications by outsiders were also referred to Gilbert to decide if comment or reply were desirable. In one such case, when a geologist of foreign birth, but long resident in the United States, complained that his early work had been disregarded in a survey bulletin, Gilbert took the complainant's side in discussing the matter with the authors of the bulletin and advised them to be more careful in future to give full credit to their predecessors; but to the complainant he wrote, after acknowledging the bulletin's deficiency, in a somewhat different tone:

> In my opinion it makes little difference to the scientific world by whom discoveries are made, and I regard public discussion of questions of authorship and priority as a burden to the literature of science, occupying space and costing energy which could be better devoted. In my own writings I endeavor to give credit to those whose ideas and work I use, but I do not demand that others shall treat my work in the same way, and I do not propose ever to make reclamation of ideas borrowed or observations duplicated by others.

He admitted to friends that some might think his impersonal attitude "cold and unfeeling," and it is probable that most of his colleagues would regard that part of it which renounced reclamation of ideas borrowed as unattainable even if desirable; but the complainant must have regarded his attitude as incomprehensible. Shortly after Gilbert dispatched this letter he sent another to a young American geologist, expressing satisfaction that he was not "fickle of purpose or the possessor of sensitive intellectual corns, such as make the lives of some scientific men burdens to themselves and their friends." It would seem as if the second of these two letters had been inspired by the first.

Although Gilbert was hearty in commending essays and reports that he approved, he did not hesitate to pronounce an unfavorable opinion on a manuscript if his judgment condemned it. Of an essay submitted by a nonmember of the survey for official approval he wrote to the director:

> Its author is a man with a theory, who seeks facts to support it and tries to explain away facts which stand in the way. His mental attitude is unscientific.

On another manuscript he reported even more severely:

> It contains very little new information. Its contribution to the subject, if set by itself, would readily be contained on a single page. I will not mention certain defects of style, as these would be eliminated by editorial revision; but it is my judgment that the publication of so large an amount of compiled material with so small a measure of local and novel information would excite an amount of derision which the Survey can ill afford. I therefore recommend that the MS receive such disposition as will effectively prevent the possibility of its publication.

STANDARD GEOLOGICAL MAPS: 1887–1889

No part of Gilbert's work, first as unofficial adviser to Powell and later as his chief geologist, was of greater import than that concerned with the development and adoption of a carefully considered scheme for the preparation of the geological maps in which so large a share of the labors of the survey culminates. It involved the standardization of many matters regarding which a great diversity of practice had grown up to meet the various needs of State surveys, yet regarding which a comprehensive and long-lasting uniformity was manifestly necessary for the needs of the national survey. The nature of the areal subdivisions to be traced in the field, the systematic terminology by which the subdivisions should be named, the colors by which they should be represented on maps, and the texts by which they should be described and explained in the geologic folios—all these fundamental matters had to be defined in a manner that should, if possible, enjoy a long-continued acceptance by the geologists of the country; and various practical problems in the way of map making had to be solved at the same time. Powell was indeed fortunate to have a man of Gilbert's knowledge and temperament to take charge of this large and difficult piece of work. The general satisfaction that is still felt in the style of the folios, after 30 years of trial, is good testimony to the wisdom shown in formulating the plan of their publication.

The standardization of the survey maps began in 1881 with an announcement in Powell's first report—the second of the series—regarding a scheme of geological colors to which reference has already been made. A more important step was made in 1887 when Gilbert, acting for Powell, sent a letter to 12 leading geologists, inviting them to a conference on map problems. The letter included the following significant statement concerning map sheets:

> As our plan involves the preparation, sooner or later, of about 6,000 of these sheets, the method of publication is worthy of the most careful and thoro investigation we can give it.

Following the conference Gilbert issued an account of the plan adopted. After declaring the survey areal unit to be an atlas sheet, not a State, he went on:

> We assign an atlas sheet to a field geologist, who is not a paleontologist, and instruct him to map the formations that he finds without regard to correlation, treating his area practically as though it was the first area ever mapped. For his purposes, a "formation" is a stratigraphic unit of such order as his map scale permits him to plat. He collects fossils but does not study problems of correlation. . . . Formation names are local and so are formations. Some will appear on but one atlas sheet, and we propose to carry none farther than we can find good physical basis. Detailed correlation is a work for the paleontologist, and for the broad view. It is to be prosecuted in the interest of general geologic history, and it has slight bearing on problems of stratigraphy and economic geology. It can be done at any time, and can be done over as often as the progress of the science requires. But the map, if faithfully presenting the physical or stratigraphic phenomena, is of permanent value, and needs no amendment to bring it into conformity with new views. This plan of ours is the outcome of not a little study and experiment, and it appears to us that if we attempted the alternative plan of correlating in detail before we decided what to map and what names to use, we should never accomplish anything in areal geology.

Another step was taken at the end of 1888, when a circular letter, this time signed by Powell, was sent out to a larger number of geologists, asking for comments and suggestions on the general questions involved in map publication; and the replies were considered in January, 1889, by a committee of 18 members under the chairmanship of the director. One of Gilbert's letters summarizes the action then taken more pointedly than was done in the formal report upon it:

> The past week has been chiefly devoted to a conference on the plan of publishing our geologic maps. Powell called here a number of his assistants living in other cities, and these, with the Washington chiefs of divisions, made a roomful, who spent four whole days discussing the various scientific and practical questions involved. Everybody had ideas and opinions and each recognized great gain from the interchange. Most of the questions were satisfactorily settled, and the principal one left unsettled was one requiring experimentation. Powell shines as moderator of such an assembly, for he has rare power of keeping the main point in focus.

The results thus reached were published in the tenth annual report of the survey. It may be questioned whether any other national survey ever began the publication of its maps with so carefully considered a plan touching every detail of terminology and coloration. As examples of the rulings then established the following extracts are presented:

> A formation should be recognized and called by the same name as far as it can be traced and identified by means of its lithologic characters, aided by its stratigraphic association and its contained fossils. . . . In the application of formation names, the laws of priority and prescription shall be observed. . . . Maps are designed primarily for the use of the citizens of the United States, whether geologists or laymen. . . . The unit of publication shall be an atlas sheet with legend on margin, accompanied by a full sheet of letter press description (so prepared as to be intelligible to men who are not trained geologists), and where necessary, by a sheet of sections exhibiting the structure of the area.

The nongeological nationals of the United States ought to have been as much flattered by the consideration given to their needs in this plan as they must have been dismayed on seeing the measure of intelligence expected of them when the maps were issued.

One of the novelties of the plan was the adoption of blue instead of gray for the coloring of Carboniferous formations; but it was properly pointed out that, while gray might be appropriate if the Carboniferous were always carboniferous, blue was as appropriate as gray for a formation that, in the United States at least, was represented by a marine limestone over a larger area than that in which it included coal beds. But this was a subordinate matter; the marvel of the color scheme adopted was the ingenious manner in which it was made available for several thousand map sheets and for several hundred formations. It was explained that if each local formation had a special combination of color and pattern, the scheme would become

so cumbrous that no one could learn it; if the scheme were specially adapted to the needs of each separate sheet it might be simple enough in each instance, but the color alphabet would then vary from sheet to sheet in a manner that would be confusing. Hence after mature deliberation a scheme was adopted in which both of these difficulties are reasonably avoided; a scheme which calls for the assignment of certain well-defined notational devices or "patterns" to certain classes of formations, but not to specific formations. The formations were to be grouped in four classes; crystalline schists, igneous rocks, fossiliferous clastics, and superficial deposits. Each class would use the entire gamut of color, but the colors would be printed in different patterns for the different classes. Thus the scheme was made "uniform with reference to the broad features of classification, and plastic with reference to details."

The "study and experiment," of which the standard map scheme now familiar in the geologic folios was the outcome, appear to have been devoted not alone to the choice of colors and patterns with regard to the legibility of their combinations, but also to the practical details of reproducing the patterned colors satisfactorily by lithography instead of by hand, and this was something of an innovation; for it will be remembered that although a number of colored maps had been produced by mechanical presswork for certain departmental and State surveys in an earlier decade, their color spaces were seldom of fine texture; and also that the geological maps of European surveys were in the eighties usually colored by hand. Hence the proposal to run off from a press a large number of sheets in a large series of maps with many small and intricate color spaces was something of a novelty; and as the proposal also included minute color patterns which had to vary with the intricate color spaces, it was a complicated novelty. Study and experiment were evidently necessary before it could be safely undertaken.

When the final plans were formally announced, Powell properly assumed full responsibility for them, but at the same time expressed his indebtedness to his fellow workers for the aid they had given:

This subject has been one of constant study by the Director for many years. He is indebted to his colleagues on the Survey for much careful experimentation and many valuable suggestions. He is especially indebted to Messrs. Gilbert and McGee to whom the subject in all its parts and in all stages of investigation has been referred from time to time. Mr. Gilbert has supervised the experiments and selected the specific patterns in the several classes.

The precise extent to which Gilbert, as chief adviser, collaborated with Powell in the development of the color scheme and other features of the plan is not known; for although it is recalled that the various kinds of lines, triangles, and dots used in the color patterns were all minutely tested and specified by Gilbert before he regarded the scheme as satisfactory, there is unfortunately no sufficient record of the contributions which he and his senior made to the total result. The director of the survey undoubtedly had much to do with the scheme in a general way, consistent with his capacity as an original thinker and his duty as an administrator; while his chief geologist also had much to do with it, as would be expected from his painstaking ability to follow a problem to its very end. There must have been from time to time occasions when wise choice had to be made among several competing devices, and there must have been continued need of keeping a trained and patient hand on every detail of the various devices with respect to their practical availability. We may therefore believe that the choice was the choice of Powell, but that the hand was the hand of Gilbert; and it may even be surmised that the patient hand sometimes guided the occasional choice.

No formal changes were made in the plan of the geologic folios for over a decade; the changes that were gradually introduced were those of natural and healthy growth. The most important was the development of the folio texts far beyond the original intent both in quantity and quality. The text has often come in later years to occupy several full sheets of letter press in a folio and to involve technicalities of nomenclature and complications of discussion that can be understood by none but a trained geologist, and a well-trained geologist at that. But it will appear in a later section that when it came to be Gilbert's duty to prepare a folio he held more closely than many of his colleagues to the idea that the text should be easily intelligible, although in so doing he omitted matters that might, in the opinion of some folio users, have been profitably included.

REVISION OF 1902–3

Not until the winter of 1902–3 was a formal revision of the earlier plan undertaken. A conference was then held, with Gilbert as its chairman, for the purpose of settling certain questions that had arisen as the work of the survey advanced; but the changes resolved upon were mostly of a subordinate nature.[1] One was the merging of the superficial or Pleistocene deposits with other clastic deposits, thus making three instead of four classes of structures to be dealt with. Others were the addition between Cambrian and Silurian of Ordovician, which had been excluded before, and the adoption of Pennsylvanian and Mississippian as subdivisions of the Carboniferous; also a relaxation of the rule regarding priority in the naming of formations. On the last point Gilbert had previously expressed his opinion in a discussion concerning the name, Newark, proposed by the survey to replace the various names by which the so-called Triassic strata of the eastern United States had been called:

> Much time and ink have been wasted in discussing the claims of alternative stratigraphic names. In many instances controversies arise over matters of fact, but there are also numerous cases in which the facts are well understood, and individuals disagree only as to the bearing of the facts on the questions of nomenclature. Opinions differ so widely as to the principles which should determine the selection of names that facts which some regard as conclusive appear to others not at all pertinent. The road to ultimate peace lies through a war of principles, and the valuable controversy is one in which the fundamental postulates of the contestants are exposed.

He then proceeds to state the two postulates on which his preference for Newark is grounded: First, the formational unit in question should have a single name over its entire area; and, second, the name should include a geographical term. Newark is preferred as the geographic term because it has a definite association with the formation under discussion, because it has priority of use, and because it is free from other association with geological terminology.[2] Nevertheless, the generally established use of Triassic was in the end allowed to prevail over the effort of the survey to revive an early but obsolete name for use in a way that was in accord with its scheme of systematic terminology. At a later date, after the action taken by the survey committee in 1903, Gilbert expressed himself characteristically in a "notice" of the new rulings and relegated mere priority to a low rank:

> In all the various discussions as to the choice of names and the determination of principles by which to regulate such a choice no account has been taken of the personal factor. Where priority has been the criterion of selection, it has been used because it affords a rule of simple application, and not because the authors of names are supposed to have "rights" in the matter. For myself I share the view of Darwin, that the accentuation of personal credit for the giving of names is the bane of systematic terminology in biology, and believe that it should be scrupulously avoided in geology.[3]

Thus from first to last Gilbert had a hand, and a very influential hand, in establishing the plan on which the geologic folios of the survey are constructed; he may be regarded as one of the leading architects in preparing the design of what is growing to be a great national monument.

CORRELATION PAPERS

Gilbert's duties in connection with the "correlation papers," already alluded to, were more to his taste than administrative tasks, although they interfered seriously with field work. These duties were assumed in 1888, the completion of the Bonneville report and his withdrawal from the charge of the Appalachian division giving him the freedom necessary for them. A conference was held in order to agree upon certain essentials, chief among which was that the units of correlation were to be large time periods, like Cambrian and Cretaceous, and that the correlations were to be based chiefly upon marine invertebrate fossils. Time for study and reflection was here taken freely, both by Gilbert, as editor in charge, and by the authors of the several essays, and as a result an extremely useful series of volumes was eventually issued. The editor's share in their preparation was at first the determination of a plan to be followed

---

[1] Twenty-fourth Ann. Rept. U. S. Geol. Survey, 1903, 23-27.

[2] The name "Newark" in American stratigraphy. Journ. Geol., ii, 1894, 55-59.

[3] Amer. Geol., xxxiii, 1904, 137-142.

before the work was begun and the selection of authors for essays on each of the chief divisions of the geological time scale; later he had to read and to a certain extent revise the manuscripts submitted, and between the beginning and end he undertook a critical study of the principles that had been till then followed in establishing geological correlations and a careful inquiry into the principles that ought to be followed in the future. At all stages of the work he had much correspondence with authors and advisers, some of whom appear to have wished to follow plans of their own rather than to conform to the plan adopted. The following extracts from letters to Prof. O. C. Marsh may serve as samples of Gilbert's way of treating his problems. The first letter was written August 6, 1889.

> . . . The gentlemen who are considering the correlation of the Tertiaries, by means chiefly of invertebrate faunas, find practically no evidence connecting the classification of the Tertiaries of the great interior with the classification of the Tertiaries of the Atlantic and Gulf coasts, the difficulty arising from the fact that the interior Tertiaries are freshwater, while the coastal Tertiaries are marine, so that there is no overlapping of invertebrate faunas. I am informed by Mr. Ward that no light is thrown by the existing collections of plant remains, because the floras obtained from the marine Tertiary formations are very meager. I now write to inquire whether the vertebrates afford data for the discussion of the ages of the individual formations of the coast as compared with the ages of the individual formations of the interior. Between Cretaceous and Post-Pliocene (exclusive) are there common forms from the marine beds of one region and from the freshwater of the other, and if so, are these sufficiently numerous to form a satisfactory basis for discussing the comparative chronology of the stratigraphic columns?

In connection with his reply to this letter, Marsh appears to have made some unacceptable proposals regarding the introduction of new studies, which brought forth, on December 11, 1889, the following rejoinder from Gilbert:

> I regard it as altogether impracticable to include in such review any unpublished material, except such personal information as may serve to connect the description of species with data in regard to localities and formations omitted or concealed by the paleontologist. If unpublished material is included, there seems no place to stop, and the undertaking becomes interminable and impracticable. The question to be answered is not what *can be* shown by vertebrate fossils, but what *has been* shown, and what is the comparative value of the showing.

The words "omitted or concealed by the paleontologist" recall the extraordinary state of affairs that had existed in earlier years among rival hunters of fossil "big game" in the Tertiary formations of the West.

Official correspondence and reports exhibit one phase of Gilbert's work; personal letters exhibit another phase. When the correlation papers had reached an advanced stage in February, 1891, they combined with other duties to reduce the chief geologist, through the winter at least, to the position of an editorial critic. He wrote to a friend:

> My daily grind just now includes the reading of long manuscripts prepared for the printer. For years the Survey has been preparing a series of essays on the formations of the different periods and the means of correlating those of different districts and countries. The direction of this work has been in my hands and now the results are coming in. I have already read piles of MSS. with a total depth of 8 or ten inches and as much more is in sight with early prospect of still a foot or two more. To this I give my forenoons, with my feet comfortably stretched over a chair toward my sitting room fire, and then in the afternoon I sit at my desk, in front of a grinning ink-stained skull, and dictate letters and answer questions. Sunday is apparently a cross between the two occupations, for my feet are up and I am busy with letters at my desk.

The results of this "daily grind" on the part of the editor, following the labors of his associates, was a series of bulletins of great practical value to American geological science. The principles involved in correlation were summarized in a paper presented by Gilbert to the International Geological Congress at Washington in 1891,[4] which deserves careful reading by those who have to do with that branch of geological science.

THE SMALLER DUTIES OF A CHIEF GEOLOGIST

There can be no question that many of the plans and decisions regarding the geological work of the survey which Gilbert was called upon to make were of large importance in the progress of American science; and it may be well believed that, although his work was varied

[4] Congr. geol. internat., 1891, 151-155.

and difficult, he developed the plans and formulated the decisions justly and wisely in the great majority of cases, for he was preeminently a reasonable and fair-minded man. But he had also a vast number of triffling affairs to act upon, and it is grievous to think that any of his time should have been wasted on such small matters. Examples taken at random from his official correspondence include such details as the rearrangement of desks and chairs in the survey offices; a definition of the duties of disbursing agents; the specification of the form in which divisional geologists should write orders for the performance of field work by their assistants so as to meet the meticulous requirements of Treasury officials; a recommendation to the chief clerk that "the privileges of purchasing eggs and chickens, extended under certain conditions to the Appalachian division, be also extended to the Potomac division;" authorization for the transfer of a small unexpended balance from one division to meet a deficiency in another; an explanation to a field geologist that an "oath of office" must be taken by all his temporary scientific assistants, but not by his day laborers; advice whether a horse and buggy should be hired or a bicycle purchased for an assistant's field transportation; information as to how tents might be requisitioned from the Quartermaster General of the Army instead of bought in the open market; and the preparation of a reticle by means of which the total length of all the contour lines on a map should be estimated, as a guide in determining the cost of engraving. All these and many other small matters had of course to be settled by some one in authority, but that Gilbert should have been selected to make such settlements seems a wasteful use of an eminent man.

One reason that made some of the work that he had to do as chief geologist disagreeable to him was that, while he intensely disliked to render a decision on a case in which he felt he had not had time to gather and to examine all pertinent considerations, his administrative duties frequently called for prompt action. He wished to make deliberate rather than hurried judgments. Papers accumulated on his desk, because he would not or could not act upon them without a certain measure of consideration; and this is the more singular because in scientific matters his judgments were often formed quickly and with remarkable accuracy; observed facts were almost unhesitatingly classified in the groups where they belonged. Nevertheless, his decisions in administrative matters were often reached so slowly that the younger and more impulsive members of the survey, who saw chiefly their own side of the questions involved, were at times impatient under the many-sided deliberations of their chief. Indeed, it was not alone the younger members who were sometimes dissatisfied with the delay in the chief geologist's office in reaching decisions on their propositions; nevertheless, the chief geologist persisted in taking his time. He would surely have agreed that the captain of a sinking ship must adopt at once a fairly good plan for the rescue of his passengers, rather than wait a day or more for a better plan that might be reached after mature reflection; but he could not regard the survey as a sinking ship—although it unhappily came to be not altogether unlike one in 1892.

THE DISASTER OF 1892

Who can say how long Gilbert might have continued in administrative thraldom, had it not been for the disaster which overtook the survey in 1892. The year before, Congress had cut off irrigation at the behest of western landowners and cattle kings; that summer, Congress, acting very tardily, reduced the survey appropriation for the year ending June 30, 1893, to but little more than half of its previous amount. Many salaries were stopped entirely; others were seriously reduced; the survey was crippled.

Gilbert's relation to the disaster was peculiar. He had had nothing whatever to do with presenting the survey's needs to Congress in order to secure adequate appropriations for its work; his duty had been to see that a share of the money provided was wisely spent as far as geology was concerned. His small liking for even that sort of responsibility has been told; the responsibility of lobbying for appropriations would have been utterly distasteful to him, and he as well as others believed that he would have had little success in it. He had written to a friend the previous December:

> I have just spent four days in New York, New Haven, and Newport, trying to beg some money to bore a hole deeper [the deep boring at Wheeling?], and have only succeeded in confirming an impression I had before that I am a poor beggar. But the trip gave me much time for reading and I got through a *pile* of tracts.

If he had cared less for the scientific contents of the pile of tracts he might have made a better beggar; but his preference for science was so pronounced that even while the survey disaster was approaching he gave such time as he could take from his administrative duties to the prosecution of his own studies, not to the persuasion of the Congressmen.

It was indeed during the very period when the political attack upon the survey in Congress was at its height that Gilbert calmly spent many evenings at the Naval Observatory in Washington, studying our nearest neighbor. These evenings were about as far from congressional lobbying as can be imagined, although even there Gilbert might, had such been his disposition and habit, have deftly put in a good word for the survey when he met "obstructive" Congressmen, who apparently ranked above a mere geologist when it came to a seat at the great telescope for a sight of celestial scenery. Some of the Congressmen, however, seem to have been better tacticians than he was; for, as he afterwards wrote in high glee to an intimate correspondent, one of the national legislators, waxing eloquent in the course of his denunciations of geological depravity during a congressional debate, said:

> So useless has the survey become that one of its most distinguished members has no better way to employ his time than to sit up all night gaping at the moon.

It had been of course Powell's duty as director of the survey to meet congressional committees, and he had for years been extraordinarily successful in securing their support for appropriations sufficient to carry on all the work that he wished to undertake. But in the summer of 1892 his arm, partly amputated 30 years before after a wound received in the Civil War, gave him much pain, and the suffering thus caused made it difficult for him to present his case as effectively as before. He failed that year to overcome the opposition excited by the selfish interests of men who were rapidly growing rich by the unrestricted use of the public domain and the material resources of the West; and although the amount of money assigned for topographic surveys was not seriously cut down, the share of the appropriation available for geology was greatly reduced.

The reduction was all the more calamitous because congressional action regarding it was unduly delayed. Although the probability of reduction was foreseen late in July, the appropriation bill was not finally enacted till August 5, when field work, authorized some time before on the expectation that the usual amount of money would be available, was in active progress by a number of divisions of the geologic branch in all parts of the country. It therefore fell to Gilbert's lot immediately to countermand by telegrams and letters the orders that had previously been given. Field parties were directed to stop work at once and to prepare for record or publication such results as had been gathered. Indeed the manner in which the reduced appropriation was allotted by Congress to specified purposes apparently made it necessary to dispense with the services of a number of highly valued members of the survey, who must have been shocked indeed to receive communications, dated August 6, from the chief geologist, couched in the following terms:

> Letters have been to-day prepared notifying a large number of geologists and assistants that their resignations will be requested after an allowance of time to put their field material in shape for permanent record. I write unofficially to say for your information that such a request will be made of you, but at present the matter is undecided. Sincerely regretting that there should be any question of the ability of the Geological Survey to continue its agreeable and profitable relations with you, I remain . . .

Little wonder that the late summer of 1892 is recalled by the survey members of that period as a time of trouble and distress, even though the reduction of their numbers was not afterwards carried out in so drastic a manner as at first seemed necessary. Those who were in the field at the time suffered the pangs of uncertainty as to their future occupation and livelihood; the mental anxiety of the few in Washington was made the harder to bear by a canicular spell of excessive heat such as gives that city an unenviable reputation in the late summer. One of Gilbert's personal letters tells the story briefly:

> We are just through the hottest week the Weather Bureau has ever recorded for Washington. We are in mourning because Congress has voted so little money for geology that half our corps must be discharged before the year is through. The planning for readjustment is tiring in more senses than one.

Yet hot as was the weather and tiring as were the plans, the chief geologist did all in his power to make the best of a bad matter for his associates. Thus, on August 8, he wrote a long letter to the director of an important State geological survey, recommending for positions there certain members of the national survey who might have to be cut off. When one of the field geologists returned to Washington late Saturday evening a fortnight or so after the disaster in a state of great uncertainty as to his future, Gilbert called on him in person early Sunday morning with the comforting assurance that the director had placed his name on the list of those who were to be retained. To another he wrote in the following kindly spirit:

Perhaps it is not amiss to say at this time that your reputatiou at this office for careful economic administration is good, said reputation being based on your accounts, monthly reports, etc. Partly for this reason there lies on the desk of the Chief Clerk a letter from the Director to the Secretary of the Interior recommending an increase of your official salary. Unfortunately, our calamities will keep that and other letters of similar tenor from being sent forward to the Secretary, but it may give you some slight consolation to know that there was a spontaneous movement in that direction.

But there appears to have been another side to the calamity. Although Gilbert evidently felt almost as keenly as Powell himself the misfortune that enforced retrenchment would entail upon many of the survey members, he was philosopher enough to find consolation in certain of its aspects, as is indicated in a letter to a divisional geologist:

While the Congressional onslaught is disastrous to mauy individuals, and therefore grieves me greatly, it is not an unmixed evil for the Survey. It will have the effect of winding up shortly one or two investigations which have been too dilatory, and will give opportunity for reconstruction in one or two places where it has been desirable but difficult.

As to his own demotion from the rank of chief geologist to that of geologist, which took place by congressional enactment in August, 1892, Gilbert could have felt nothing but relief. Yet in the preceding June, when he might have enjoyed that relief and escaped from all administrative work on the survey by resigning from it to accept an offered professorship of geology at Cornell, he did not do so. His feeling at that time was expressed in a letter to a friend:

Your letter about Cornell versus Washington gives me great consolation in that it states the case very much as I see it. I have never consciously cared for the "renown that comes from running large affairs", and am disposed to think I have not really cared for it as I am perfectly conscious of caring for some other kinds of renown. At any rate I took my present office reluctantly as an accommodation, & my chief reluctance at the thought of leaving it is the difficulty to which you allude of filling the hole. . . . If I could name an entirely satisfactory successor—who could be prevailed on—the Major would let me swap back.

When his demotion came, if it lowered his official standing, it seems at least to have raised him to a higher floor of the Washington office; for about a month later he wrote a member of the survey who still addressed him in his former capacity, saying that the letter received called his attention to the fact that his change of status has not been officially announced, and added:

Don't trouble yourself as to any questions of etiquette which may be involved, for they make no trouble at this end. When business comes to me under the old title I send it down stairs without comment.

Faithfully as he had attended to administrative duties while they lasted, he was undoubtedly glad to be free of them.

Well may Powell have written in the thirteenth annual report, at the close of Gilbert's supervising service in this troublous time:

Special obligations are due to Mr. G. K. Gilbert for his arduous labors in administering with skill and success the business affairs of the geologic branch.

Gilbert's own report in the same volume touched on his personal affairs very briefly:

Duties in connection with the general work of the branch have left me little time for personal work in the field.

But he could not immediately escape from the office. A few days after his demotion, when he was impatient to leave Washington for a brief visit to friends in Ithaca, he wrote them: "Tonight I find myself right in the middle of a task that must be finished. Moreover my chief is used up by the strain on his mental resources and his sympathies. My own special work is nearly done but I see that by staying a few days longer I can relieve him from the necessity of staying to look after details after the problems have been solved. . . . Fortunately I am feeling

much fresher than when I put in my plea" for a leave of absence. Little wonder that he wrote about this time to Russell, who was exploring certain terraces of the Columbia Valley in the State of Washington, where the river had been formerly blocked by glaciers that descended from the mountains on the northwest: "I envy you the study." As a result of the postponed departure, his vacation was a short one. He was away from Washington during the fatiguing summer of 1892 only from August 11 to 27; the time was spent partly at Ithaca, partly at Rochester during a meeting of the American association. On his return to Washington he was freer than he had been for some years to pursue his own studies, the face of the moon among them. Yet, although the position of chief geologist no longer existed, because Congress had made no provision for it, Gilbert still continued at Powell's request to act in that capacity in many ways for over a year.

AFTER FIFTY YEARS

The disaster of 1892 bore much more heavily upon Powell as director of the entire survey than on Gilbert as chief geologist of one division. Indeed for several years previously the normal difficulties of the director's position, serious enough by reason of the extraordinary growth of the survey, had been aggravated by the antagonism of political enemies and the irritation of conflicts. The reduction of appropriations in 1892 to $430,000 was almost a breaking strain. It is true that the reduction was followed by an increase nearly to $500,000 in 1893, but this small gain did not nearly restore the scale of expenditure that had been permitted a few years before; and in spite of the better feeling that the gain indicated, the directorship became a fatiguing burden for Powell, all the more so because his half-amputated arm continued to give him much pain. He therefore determined in the winter of 1893–94 to withdraw from the survey in the following spring and to devote himself thenceforward to the Bureau of American Ethnology, of which he had been made chief before he became director of the survey. The Great Basin mess gave him a farewell lunch in May, at which 48 members of the survey were present; a photograph much prized by those represented in it shows Powell next to Gilbert at the head of the table. Then arose the important problem of securing a fitting successor to Powell as director.

The geological world naturally looked upon Gilbert as the man for the place, and rightly enough, for he was admittedly the most eminent geologist on the survey staff. It is gratifying to learn that Powell also had earlier looked upon Gilbert in the same light, for although no public mention was made of it at the time, it is known that the Major had wished to withdraw in favor of his chosen adviser several years before he actually resigned, and that the two had considered the matter together. The scientific opportunities of the position appear to have been in some respects tempting to Gilbert, but the administrative responsibilities had not attracted him. However, when the offer was definitely made to him he consulted an old-time friend in whom he had confidence, and the friend wisely advised him against it, pointing out that while no one who knew him could doubt for a moment the safety of the survey in his hands, the innumerable and often vexatious details which were unavoidably attached to the office of director and the annual necessity of a certain amount of political work would surely prove a heavy strain upon him, and that he ought to hesitate long before accepting the new responsibility in place of the freer position as an investigating geologist in which he had already gained so enviable a rank. Thus Gilbert's own judgment was confirmed, although perhaps to his disappointment; and the upshot of it all was that Powell continued as director several years longer. When at last Powell's resignation could be no longer delayed, Gilbert's experience as chief geologist appears to have convinced all concerned that he would not wish to be, and perhaps would not be successful as director; so the position went to another member of the staff upon whom its administrative cares rested easily. But it was still to his former chief geologist that Powell looked for personal support; it was Gilbert who went with him to the Johns Hopkins Hospital in Baltimore when a new operation on his arm became necessary in May, 1894.

A disinclination to part from his long-time chief was very probably the main cause that had led Gilbert to decline the offered professorship at Cornell in the summer of 1892, as told above, for he seems otherwise to have been much attracted by this opportunity. He had written regarding it to a professor in another college:

If C. should take me and give me a chance to develop a strong department I suppose I might attract some one who would otherwise get to you, but I imagine the chief result would be an increase of the output of geologists. Of late years there has been a fair demand for them and it ought to increase, but there is unquestionably danger of overstocking—especially if Chamberlin goes to Chicago and opens the big training school he talks about. Perhaps if I go to C. I would better limit my ambition to liberalize the education of future preachers, doctors, and engineers.

Thus Gilbert, almost 50 years old, remained on the survey as plain "geologist." His later relations were seldom more than those of an individual member, for he was not even placed in charge of a large division. Nearly all his studies of the following years were directed to the solution of special problems on which he worked by himself without assistants, as if in a state of semiretirement. In the earlier period of the survey's organization, his opinion and his voice as well as his personal example had been of great influence. His example continued to be an inspiration to many younger men, but the organization of the survey being well advanced, his opinion was less frequently consulted and his voice less often heard than before. Had it not been for the formation of the Geological Society of America shortly before the year of the survey's disaster, Gilbert's withdrawal from larger to smaller relations might have almost concealed him from a great number of young geologists who, then first coming forth, now stand in the front rank. Happily the meetings of that society made him even more widely known than before, inasmuch as while continuing his personal acquaintance with members of the national survey, he was brought into touch with many members of State surveys and many teachers in colleges who would otherwise have had no occasion of seeing him. Although he never assumed the authority that was his by right of scientific worth, he was one of the commanding figures at the society's meetings until, nearly 20 years later, loss of health forced him to withdraw from all active scientific associations. Fortunate indeed was it for the survey and for the society, and hence for American science, that they gave high position to a man of his personal character, in which the greatest scientific ability was combined, as it is not always, with sincere unselfishness and unimpeachable integrity.

Gilbert was in his prime at this turn in his life. His mind was never more alert and keen. His body was in good health, although his strength was naturally less than it had been 10 years before. His carriage was easy and erect; his bearing was so genial that his height of a little over 6 feet did not seem overpowering to men of shorter stature. His presence had the simple dignity well expressed in the portrait at the beginning of this memoir.[5] He wrote in 1892 to a Rochester classmate: "In the ten years of Western mountain work and camp life I built up my constitution so as to be a very vigorous man. Eight years of diminishing outdoor life and increasing desk work have lowered my tone somewhat and a sickness still more, but I am still insurable and fairly active;" and fairly active he continued to be for many years after. His great Bonneville monograph, published but a few years before, had confirmed all that was expected from his unusual geological ability. His relation to the geologists of the country was a most enviable one, for he had inspired their respect and admiration and had won their confidence and their affection to an exceptional degree. He was a welcome speaker at every meeting he attended, and what he said always carried weight; every hearer felt that his statement of facts was critically accurate, that his inferences were logical and trustworthy, and that his judgments were well founded and impartial. He held at one time or another offices of responsibility in nearly all the scientific societies of which he was a member; his associates seemed to delight in honoring him. Now, freed from the distractions of administrative work, he devoted 25 years to a succession of tasks, several of which were truly of great import; yet in reviewing them all one must regret that his rare ability was not concentrated on a few large problems instead of being distributed over many smaller ones.

That the decade from 1891 to 1900 should have witnessed Gilbert's withdrawal from office work was proper enough, for when a man of eminence reaches the age of 50 years he has earned the right to lay aside tasks that he dislikes in order to devote himself to what he can best accomplish. But that that same decade should have witnessed also a reduction in the continuity of his scientific investigations is regrettable. The summers of 1893, 1894, and 1895, spent on the plains of Colorado, seem in the light of later events to have involved chiefly a dissipation of energy that might have been conserved to advantage, had he then been assigned to a larger

[5] Another portrait, of about 10 years later date, is given in the Bulletin, Geol. Soc. America, xxxi, 1919, pl. 2.

problem, and to one that was more to his liking; such, for example, as the structure and origin of the basin ranges, which by good rights still belonged to him. When he did take up that fine problem seven years later, an unhappy accident caused him to turn from it after only a single season in the field.

It is true that the studies of Niagara and the Great Lakes, which Gilbert carried over from the previous decade into the nineties and later, were highly illuminating; and had they been advanced from personal to divisional rank and been carried to the point of preparing a complete report on the postglacial history of the St. Lawrence drainage, such a volume would have rivaled the Bonneville monograph; but unhappily his work never went so far as that, and his results, so far as they are published, remain subdivided among many short essays. It can not be doubted that, if he had had the aid of a small corps of assistants in tracing shore lines and lake outlets and if the volunteer workers in various parts of the Great Lakes region had been invited to submit their results to him for correlation, the Great Lakes problem would have been rapidly advanced and essentially completed. A comprehensive study of this kind would, to be sure, have assumed an international character which might have raised certain difficulties in the way of its execution, but paths around difficulties can usually be found when they are wanted; and a cooperative investigation of the St. Lawrence drainage basin by the United States and Canada would have been a fine contribution to international comity. But, as a matter of fact, Gilbert's field studies in the Great Lakes region were not only performed largely without assistance; they were also much interfered with by a variety of distractions.

Discontinuity of effort thus seems frequently to have characterized his later years, and one cause of the discontinuity appears to have been of his own liking, for it was entirely by his own choice that he undertook several courses of university lectures and various literary tasks, during the performance of which leave of absence was taken from the survey without pay. Interruptions of these kinds always consume more time than is allotted to them; they are like constrictions in a water conduit which diminish its discharge not only by locally decreasing the conduit diameter but also by exciting eddies, and thus diminishing the effective diameter of the conduit still further. However, in a later period, just before and after his seventieth year, Gilbert made a remarkable investigation of hydraulic-mining débris in California, the reports on which showed that he preserved even after a severe illness in 1909 and into his old age an extraordinary capacity in continuous quantitative work, and in linking together a long enchainment of causes and effects; but naturally enough this great study did not reach the dignity of the Bonneville investigation, all the observational and much of the theoretical work upon which was completed before his fortieth year. Many other subjects that he treated in later life were of comparatively short range; or if of longer range, his touch upon them was short lived even though penetrating.

It thus appears that when, on passing his fiftieth year, Gilbert was relieved from administrative work, he did not enter upon any one great task and carry it to completion. His later studies were directed to a variety of topics, each of which was treated admirably as far as it was followed, but none of which was investigated with monographic thoroughness. His work on Niagara and the Great Lakes was longest continued and carried nearest to completeness, but it was intermittently pursued and, as already noted, the results gained were not brought together in consecutive and harmonious form. Some of his most significant observations, such as those concerning the cross-spur channels cut by the proglacial lake discharge in central New York, and those concerning the Algonquin and Nipissing outlets across the Province of Ontario, remain for the most part in unpublished notebook records. Gilbert stood at this time unquestionably at the zenith of his scientific achievements; he enjoyed to the fullest extent the confidence and respect of American geologists, his relations with whom were, as already noted, happily extended through the meetings of the Geological Society of America, then newly organized. His influence on geological science continued for years to be most beneficent in the way of inculcating by example the very ideal of unprejudiced objective methods in scientific work; but his native genius was not employed to its fullest advantage in the discontinuous studies that he undertook. It was as if the countless distractions during his 10 years of administrative work had so interrupted his natural capacity of scientific study that he found difficulty in returning to large problems involving long-sustained mental effort.

CHAPTER XX

GENERAL SCIENTIFIC ACTIVITIES: 1891–1900

THE INTERNATIONAL GEOLOGICAL CONGRESS OF 1891

Gilbert was one of the large majority of American geologists who favored and brought about the change of meeting place for the Fifth International Geological Congress from Philadelphia, where it had been ineptly placed following the proposal of an unrepresentative American member at the London congress in 1888, to Washington, where it should have been placed from the first. He was vice president of the American committee on organization, and when the time for the meeting approached he took an active part in preparing the program for its sessions and in making arrangements for the western excursion that was to follow it. Chapters for the excursion guidebook on Niagara and certain districts in the Cordilleran region were written by him. During the meeting in Washington, he spoke on methods of correlating clastic formations, there reflecting his work on the correlation papers for the national survey; and at the final session he called attention to the fact that, as rules on various practical matters established by votes of earlier congresses had frequently been found unsatisfactory when the attempt was made to apply them under new conditions, the precedent of the London congress of 1888 not to establish rules by vote had been followed at Washington. He joined with Major Powell in giving a complimentary dinner to 12 European and 10 American members. The two hosts occupied the ends of the table, while foreign members alternated with Americans along the sides. A company that included among its members von Zittel, de Geer, Barrois, de Margerie, Rothpletz, and Credner, with Le Conte, Hall, Marsh, Chamberlin, Shaler, and King, and that was presided over by the director and the chief geologist of the national survey, may be considered truly distinguished.

Gilbert was one of a committee charged with the assignment of berths for the members of the western excursion party on the special train. "We shall," he wrote to a friend, "herd the Germans at one end and the French at the other and interpose a dining car in the middle of the train to put them still further apart." During the journey he was at all times a most helpful interpreter of American geology to the European members; he naturally acted as chief guide at Niagara and in the neighborhood of Salt Lake City. Although he had never been to the Yellowstone Park, he sacrificed the week that the excursion party spent there in order to go ahead of the special train to Utah and make every arrangement for the local excursions that he was to conduct along the Wasatch front and to certain Bonneville shore lines. After the party overtook him and spent three days there, the train ran to Denver, where a number of members continued eastward; Gilbert accompanied the others to Arizona, where Powell led them through an uncomfortable and indeed arduous experience, including a night in the rain with inadequate shelter, to the Colorado Canyon.

One of Gilbert's letters written after return to Washington refers briefly to an amusing illustration of skepticism on the part of the European geologists which was one of the most significant incidents of the entire excursion.

> My own part of the trip—about Salt Lake City—was peculiarly satisfactory. I skipped Yellowstone Park so as to spend a week in preparation in Utah. My friends in Salt Lake aided me most efficiently, so that I was able to carry out all my plans, and they added hospitality of their own. . . . . Lake Bonneville and the faults were examined with great interest and deliberation and much discussion. As to the faults especially the whole foreign contingent were skeptical at first, but they all capitulated before we were through.

Both skepticism and capitulation deserve fuller statement.

When the excursion train approached Salt Lake City in the afternoon of a hot and cindery day, most of the members were looking forward to a bath and a rest rather than to a field demonstration; but the train was stopped a short distance north of the city, where Gilbert met it with a procession of carriages, and the members, willing or not, were driven along a dusty road to

the base of the Wasatch Range to see a small alluvial fan in which a little gravel scarp, perhaps 10 feet high, was pointed out as indicating a very modern renewal of the great fracture on which the mass of the range had been upheaved. The foreign members promptly and almost unanimously refused to accept a trifling gravel bank as evidence of any such geological phenomenon. Gilbert placidly pointed out that the face of the little scarp ran like a chord across the curved front of the fan on the line of the foothill base, and added that other fans showed similar scarps in similar positions; but while the American members of the party, even to the youngest, felt no hesitation in accepting the explanation of their leader—for it illustrated what was to them a simple and familiar physiographic problem—the foreigners still remained incredulous. One of them, more outspoken than the rest, expressed the general dissent by saying that they were not used to such evidence for faults, and asked if they could not be shown an instance of displaced strata, such as they were accustomed to accept as proof of faulting. Gilbert answered that one such example was known to him, but it was inaccessible; and without trying to press his point further conducted the party to the city. But the "foreign contingent" did not let the matter drop there; they were openly incredulous in talking with each other through the evening, and said in effect: "Is this a sample of the sort of evidence on which Gilbert has based the theories in his great Bonneville monograph!" In the meanwhile Gilbert remained imperturbable, giving no sign of impatience, making no attempt whatever to help the visitors out of their difficulty; he simply bided his time, for he had something more than faulted fans in store.

The next day was given to a visit by train southwestward to the imposing Bonneville embankment known as the Stockton bar, illustrated in Plate IX of the Bonneville volume. It swings in a long curve, concave to the north, from the lake bluffs on the west side of the Oquirrh Range westward across the intermont depression known as Rush Valley; and here Gilbert's views were accepted without hesitation, if exception be made of the opinion expressed by an enthusiastic German glacialist, who was inclined to believe that the great embankment was the terminal moraine of a glacier that had come across the desert from the north.

The third day was spent south of Salt Lake City at the mouth of Little Cottonwood Canyon, one of the most interesting localities of the Wasatch front, as it exhibits, in addition to a whole series of faulted piedmont fans, the only moraines that descend from the mountains to the level of the Bonneville shore lines; and there, looking up from the line of fan scarps, the nearer one of the paired moraines was pointed out as having a narrow down-faulted notch in its crest. Even then the reluctant visitors demurred, saying that moraines were well known to have uneven profiles, and that a mere notch in a moraine crest would prove nothing. Not until the nearer moraine was ascended to the point on the upper side of its notch, whence the scarped fans to the north and south, a notch in the other moraine, and a little depression in the trough floor between the two moraines were all seen to be systematically aligned, was incredulity changed to belief. Then, to their credit be it said, the visitors' conversion was so complete that one of them exclaimed: "We must look for this sort of thing in the Alps."

This story, which has been called "the mutiny of the congress excursion" by some of the American members, is good enough without a moral, but the moral must not be overlooked. The story teaches that, even so late as 1891, the value of small surface forms, as analyzed by American geologists experienced in western exploration and applied by them to the interpretation of underground structures, was not known to their European colleagues. Since then progress in this physiographic branch of geology has been rapid; when the transcontinental excursion of the American Geographical Society crossed the United States to the Pacific coast in 1912 and visited Little Cottonwood Canyon on the return trip, the European geographers in the party betrayed no incredulity on hearing the explanation of fan scarps and moraine notches which Gilbert had given in 1891 repeated by an American who had then heard him; the skepticism shown by the geological party was not repeated.

THE ORIGIN OF COON BUTTE

In point of time the field study of Coon Butte, a curious circular rim around a basinlike cavity in Arizona, preceded and may have suggested the telescopic study of the moon, the beginning of which was alluded to in the preceding section, and the completion of which is told in the next section; but the theoretical discussion of Coon Butte was not published for three years after the appearance of the final essay on the "Moon's face." A possible origin of Coon Butte is indicated by the question-begging name, Meteor crater, by which the cavity within the butte rim had come to be known; and the idea that the cavity marked the place where a huge meteor has embedded itself in the earth's crust presumably had to do with the hypothesis of moonlets embedding themselves in the face of the moon, as the next section will explain. In any case, Gilbert took leave of absence from survey duties in November, 1891, and, with Marcus Baker as aid in the preparation of a topographic and magnetic survey, went to Arizona to examine the crateriform cavity. Several weeks were spent there, and a following fortnight was given to the study of unquestionable volcanic cones and craters in the neighborhood of the great dissected volcano of San Francisco Mountain. Gilbert took along on this excursion the manuscript of a bulky survey memoir on a subject not at all Gilbertian with the intention of giving bad-weather days to its revision; but fortunately the weather was persistently clear and his leave of absence was little interrupted by this office duty.

Before setting out from Washington, the object of the journey was told in a personal letter:

> The errand is a peculiar one; I am going to hunt a star. . . . Numerous fragments of meteoric iron have been found in a tract adjacent to a "crater," and the crater differs from others in that it is composed of sandstone and limestone and has no volcanic rock. Suspecting that it is the scar produced by the collision of the earth with a small star, I asked Mr. [W. D.] Johnson to take a look at it, and report. He has done so and his report confirms my suspicions. The scar is ⅝ of a mile across and the indications are that the missile is somewhere under the scar.

But in spite of the provisional acceptance of this meteoric theory, it was given up after the ground was examined. Another letter to the same correspondent tells the story:

> I didn't find the star of course, because she is not there—but like the peasant who subsoiled for gold I reaped a crop that is worth having. Thirty days in camp gave a fine foundation for winter work, and then even though the hole is starless it is probably unique. The theory at present in favor is that hot lava was injected 1200 or 1500 feet below the surface of the plain, superheating some water it found there and causing a big blast of steam power. If that theory is good, the shower of meteoric iron was subsequent, and its coincidence in place was fortuitous. . . . Our wagon carried three barrels of water and only one camp was made near a spring. We were free to stop where we pleased, and we always stopped where there was plenty of fuel. So we didn't mind if we had to comb ice from our hair after morning ablutions.

The same subterranean cause of the cavity was given preference two months later, after all other hypotheses had been eliminated; the opinion then expressed was that all the features of the cavity confirmed the view that it was "not produced by the penetration of a large meteor. It is probably due to the explosion of steam caused by the [subterranean] injection of volcanic rocks." As such it was regarded as a first phase in the formation of a volcano; a phase that is ordinarily concealed by the eruptions of the later phases, but which is here visible because no later phases occurred.

The problem of Coon Butte, as Gilbert called it, was repeatedly the subject of communications at scientific meetings or of lectures before more popular gatherings for several years. It was presented to the National Geographic Society of Washington in March, to the National Academy of Sciences also in Washington in April, and to the American Association at Rochester in August, all in 1892; and in 1895 at Kansas, Chicago, and Cornell Universities; but in the present connection the problem is chiefly interesting as the subject of a presidential address before the Geological Society of Washington in December, 1895, entitled "The origin of hypotheses," [1] in which philosophical impartiality in the presentation of alternative hypotheses is illustrated to perfection. A reader of this notable deliverance might indeed contend with good reason that its author seemed more interested in the abstract discussion of his problem than in

[1] The origin of hypotheses, illustrated by the study of a topographic problem. Published by the Geological Society of Washington, 1896; also in Science, III, 1896, 1-13.

any concrete result to which the discussion led. The treatment recalls that of the address on the "Inculcation of scientific method by example," delivered 10 years before, but differs in placing greater emphasis on certain steps in the orderly progress of a research. Its outline is as follows:

The first step in an investigation, after observation has been somewhat advanced, is the invention of explanatory hypotheses. Hypotheses ordinarily "flash into consciousness without premonition," and they appear always to be suggested through analogy.

The unexplained phenomenon on which the student fixes his attention resembles in some of its features another phenomenon of which the explanation is known. Analogic reasoning suggests that the desired explanation is similar in character to the known, and this suggestion constitutes the production of a hypothesis.

Gilbert said that, in order to test this view, he had for some years endeavored to analyze the methods employed by himself and some of his associates in geologic research.

Next, after an investigator has framed or invented an hypothesis, he proceeds to test it. If the phenomenon was really produced in the hypothetic manner, then it should possess, in addition to the features already observed, certain other specific features, and the discovery of these will serve to verify the hypothesis. Resuming its examination, he searches for the particular features. If they are found the theory is supported; and in case the features thus predicted and discovered are numerous and varied, the theory is accepted as satisfactory; but if the reexamination reveals features inconsistent with the tentative theory, the theory is thereby discredited, and the investigator proceeds to frame and test a new one. The investigator therefore makes two steps: He invents hypotheses and he invents tests for them, and it was to the intellectual character of these two kinds of invention that attention was directed. The comment may be here introduced that tests are usually prepared by the largely conscious process of deduction, and thus differ from hypotheses, which are more truly inventions.

The treatment then turns more especially to Coon Butte and considers particularly the two hypotheses of meteoric impact and subterranean explosion, appropriate tests being developed for each. The meteoric hypothesis is first examined.

If the crater were produced by explosion the material contained in the rim, being identical with that removed from the hollow, is of equal amount; but if a star entered the hole, the hole was partly filled thereby, and the remaining hollow must be less in volume than the rim.

The excess of rim matter required by the theory of a "buried star" was not found by observation; nor were any magnetic deviations detected, such as a large meteor should produce. It was calculated that if the meteor were 500 feet in diameter, it must be buried 10 miles below the surface in order not to affect the magnetic needle perceptibly; and such a depth of burial is regarded as so improbable that the meteoric hypothesis is thereby negatived and excluded. The hypothesis of a subterranean explosion, caused by an ascending volcanic intrusion nearly to the surface, is next examined and found to be more plausible; but in spite of the opinion earlier expressed in favor of this hypothesis, little preference is expressed for it over several other hypotheses which are patiently examined. In the end, the origin of Coon Butte is left uncertain, as if both the hearers and the readers of this address were urged to practice the suspension of judgment until some more convincing test than any that Gilbert had devised could be tried and satisfied. Instead of emphasizing one or another explanation of the curious topographic feature to which so much careful observation and reflection had been given, emphasis was given to the conditions under which an investigator may best invent hypotheses. It was concluded that if the ideas set forth concerning the origin of hypotheses are correct, "then fertility of invention implies a wide and varied knowledge of the causes of things, and the understanding of nature in many of her varied aspects is an essential part of the intellectual equipment of the investigator."

Gilbert was undoubtedly right in his estimate of the more and the less important aspects of his address. Coon Butte is a relatively trifling and local affair, while fertility in the invention of hypotheses and of tests by which the hypotheses can be proved or disproved is a matter of the broadest scientific importance. But if, in the judgment of a man in Gilbert's position, it was desirable in 1895 to expound to a representative body of American specialists in the hightly

speculative science of geology the method of inventing hypotheses and the manner of devising and applying tests for them, the implication thus given as to the previous education of our geologists is not flattering. It may be added that the uncertainty in which the problem of Coon Butte was left is characteristic of, not to say inherent in, the discussion of a single phenomenon, much of which lies beneath the earth's sufrace and is therefore beyond the reach of observation. The probability that the cavity was produced by a subterranean expolosion—such being the explanation which Gilbert appeared to prefer, as fas as he showed any preference—is increased when it is noted that a number of other cavities resembling the hollow of Coon Butte more or less closely are known to exist farther south in the United States and in Mexico.[2] The probability that the cavity was produced by the impact of a meteor—such being the explanation which many other observers of the cavity prefer—is increased by the presence of meteoric fragments near by.

THE MOON'S FACE

Reference has already been made to Gilbert's telescopic study of the moon at the Naval Observatory in Washington during the summer of 1892, when he found Congressmen to interfere about as much with his purpose as clouds. He wrote to a friend at that time:

> I am a little daft on the subject of the moon, being troubled by a new idea as to its craters, and I have haunted the observatory for three evenings in which I have netted but one hour of observation. Clouds and congressmen are about equally obstructive. Tonight I go with a stenographer so as to record what I see while it is before my eye.

Possibly it was the expense of this stenographer that was represented by an entry which recurred a number of times in the pocket diary of this period: "Astronomy . . . $2.00." The "new idea"—not altogether new—was that lunar craters are due to the impact of external bodies and not to volcanic eruptions; the idea was very probably suggested to him by the study of Coon Butte. Gilbert gave much time and more thought to the study of this highly speculative problem. His "observations were practically limited to two lunations in August, September, and October, 1892, a period affording 18 nights available for work"; and a period in which work of this wholly nonpolitical sort may have afforded welcome distraction from the disaster in which politics had involved the survey. The instrument used was a 26½-inch refractor, and the magnifying power found most serviceable was 400. So persistent were his visits to the observatory that he eventually traced the "terminator," or line of sunrise and sunset, all across the face of the moon, and thus saw every part of its earth-turned half very obliquely illuminated, so as to bring out its relief to best advantage.

In the following winter, when Gilbert was absent on leave from the survey from January to April to deliver a course of lectures at the Columbia School of Mines in New York City, his inferences based on telescopic observation were tested by the experimental production of crater-like forms in materials of various kinds, a laboratory at Columbia being made available to him for this purpose. He dropped marbles into porridge and lumps of mud into a bed of mud; he rigged up a strong sling-shot apparatus with heavy elastic bands, and thus threw pellets of clay with about one-sixth of a pistol bullet's velocity into a bed of clay, varying the size and velocity of the pellets and the viscosity of the clay bed; he used a large-bore musket to fire balls of various materials into beds of the same materials: "A leaden ball fired into a flat sheet of lead produced a cup-like crater, lined throughout with the material of the ball." The results of this experimental work, which the experimenter referred to in his letters as his "knitting," supported his idea that the lunar craters are due to impact; but before the experiments were made, he had worked out what he called the "moonlet theory" to account for the craters and presented it as a most alluring address when retiring from the presidency of the Philosophical Society of Washington in December, 1892.[3]

It has been remarked that the majority of astronomers explain the craters of the moon by volcanic eruption—that is, by an essentially geological process—while a considerable number

---

[2] N. H. Darton. Explosion craters. Sci. Monthly, iii, 1916, 417–430. [It is reported that a boring in 1925 has discovered a body of meteoric iron at a depth of over 1,000 feet below the rim of Coon Butte.]

[3] The moon's face, A study of the origin of its features. Bull. Phil. Soc., Washington, xii, 1893, 241–292.

of geologists are inclined to explain them by the impact of bodies falling upon the moon—that is, by an essentially astronomical process. This suggests that each group of scientists find the craters so difficult to explain by processes with which they are professionally familiar that they have recourse to a process belonging in another field than their own, with which they are probably imperfectly acquainted, and with which they therefore feel freer to take liberties. However that may be, a perusal of Gilbert's address leaves no doubt that his moonlet theory was, in the first place, invented by an observer who knew full well what terrestrial volcanoes are; that it was, in the second place, based on a critical study of the moon's face; and that it was, in the third place, circumspectly extended into the realms of astronomy in accordance with reasonable physical laws. His summary of the topographic features of the moon is an admirable piece of inductive work. There are a score of dark plains or maria, a score of rugged mountain chains, and a few linear troughlike depressions, but of craters there are many thousands, with diameters usually between 10 and 100 miles; yet "through the expression of every feature the lunar crater emphatically denies kinship with the ordinary volcanoes of the earth," hence, as to their origin, differences of form "effectively bar from consideration all volcanic action involving the extensive eruption of lavas," by which terrestrial volcanoes are almost universally characterized. Explosive eruptions, such as are believed to produce the occasional "maare" about in terrestrial volcanic regions, might explain the small lunar pit craters which make one-half of the total number, but not the other half, for maare "lack the wreath [rim], the inner terraces, the inner plain, and the central hill" by which the larger lunar craters are characterized. An origin by the impact of external bodies is therefore considered.

The meteoric theory of lunar craters is rejected, because such bodies approaching the moon in any direction would make a large number of oblique impacts, their predominant angle of incidence being 45°, and would therefore produce a large proportion of oval craters; but the lunar craters are nearly circular; twenty-nine-thirtieths of them have less than 0.3 ellipticity. The meteoric theory is therefore replaced by the moonlet theory, and this was Gilbert's original and personal contribution to the problem; he set it forth with some fullness. It assumes that the moon has been formed by the gradual gathering together of a vast number of small bodies, or moonlets, which originally formed a ring around the earth, similar to the ring around Saturn; that successive impacts of the moonlets on the growing moon were so far separated in time that, except when heated locally and temporarily by an impact, the moon was always a cold planet; and that even after the moon had grown to about its present size, the surviving moonlets still moving in the plane of the original ring would therefore strike the moon in that plane, instead of at all parts of its surface. Under such conditions, at least half the moonlets would fall upon the moon with an angle of incidence of less than 30°; if the moon's attraction be considered, nearly three-fourths of the impacts would have a less angle of incidence than 30°; and if the effect of moonlets in influencing the moon's rotation is considered, the number of nearly vertical impacts would be still further increased. The objection that, under this theory, lunar craters should lie chiefly near the lunar equator is met by saying that as the moonlets did not move precisely in the plane of the moon's equator, "their collisions would disturb its axis of rotation . . . . Under successive impulses thus given the moon's equator may have occupied successively all parts of its surface, without ever departing widely from the plane of the moon's orbit."

This theory is manifestly ingenious, but its acceptance is hindered by three difficulties that Gilbert's essay does not overcome. One is the absence of at least a few very elliptical craters, which even under the moonlet theory ought to make no insignificant fraction of the total. A second difficulty is the unsystematic distribution of the circular craters; for under the moonlet theory an unsystematic distribution involves the necessity of supposing that, even after the moon had reached essentially its present size, the off-equator impacts of the remaining moonlets, some of them to the north, others to the south, were so far from being compensatory that they tilted the moon into various positions, and this in spite of the earth's action in holding the supposedly prolate moon with its longer axis directed earthward. It would seem, on the contrary, that the last few thousand moonlets would be unable to tilt the moon appreciably, and that they ought therefore to have produced craters chiefly around an equatorial great circle, or that if they

departed from the equator as widely as the few thousand last-formed craters do, the impacts of those falling in high latitudes, north or south, would be so oblique that the resulting craters would be plainly oval instead of nearly circular. A third difficulty is that the supposed influence of moonlet impacts on the moon's rotation, by which the incidence of impacts would be made more nearly vertical, would be greatly weakened if not wholly overcome in the late period when the last few thousand impacts occurred, by the earth's action on the supposedly prolate moon above referred to.[4]

But apart from these difficulties, there are certain supposedly "sculptural" features of lunar scenery which do not appear to have been recognized by others than Gilbert, and which give independent support to certain aspects of the moonlet theory. These are, on the one hand, the grooves which furrow the rims of certain craters or score the tracts between the craters; and, on the other hand, the ridges or oval hills which appear to have been added to the surface. They have locally accordant directions, so that the grooves in crater rims are arranged like parts of a system of parallel chords across the rim circle; but when platted on a chart of the moon, the directions diverge from "a point near the middle of the plain called Mare Imbrium [northwest of the center of the moon's visible hemisphere], although none of them enter that plain." This sculptural system therefore has nothing to do with the "white streaks" which diverge in radial systems from certain craters, especially from Tycho Brahe in the southern part of the moon's face, and which, following Würdemann, are regarded as great splashes or spatterings of whitish matter caused by impacts. Furthermore, "associated with the sculpture lines is a peculiar softening of the minute surface configuration, as though a layer of semiliquid matter had been overspread, and . . . . obliterated the smaller craters and partially filled some of the larger"; and such Gilbert believed to be the fact. Hence, he thought that—

> a collision of exceptional importance occurred in the Mare Imbrium, and that one of its results was the violent dispersion in all directions of a deluge of material—solid, pasty, and liquid. Toward the southwest the deluge reached nearly . . . . a distance of 900 or 1,000 miles . . . Northward and northeastward it probably extended to the limb. Thus . . . were introduced the elements necessary to a broad classification of the lunar surface. A part was buried by liquid matter whose congelation produced smooth plains [maria]. Another part was overrun by a flood of solid and pasty matter which sculptured and disguised its former details. The remainder was untouched, and probably represents the general condition of the surface previous to the Imbrian event.

Gilbert's confidence in this catastrophe is further indicated by the association with it of certain great furrows, "comparable in magnitude with the Grand Canyon of the Colorado," and believed to result from a terrific plowing of the lunar surface by fragments of the Imbrian moonlet, after they had described a trajectory of over 1,000 miles. His confidence is also illustrated by a description of certain "rills," which he imagines to have been "yawning chasms three-fourths of a mile wide at top and several miles in depth" before the Imbrian deluge; when "the swift tide rushed over them a small portion may have been arrested and engulfed, but the chasms were not filled till the torrent stopped. Then that which spanned them sank down, coming to rest a short distance below the edges and so forming the visible floors."

The coherence of these explanations is attractive, but in the present connection the most remarkable things about them are, first, the amount of time that was found, during the most distressing period which the Geological Survey ever passed through, for making late at night the observations on which the explanations were based; and, next, the distance to which the explanations that were framed to account for the observations led their cautious inventor into an extremely speculative field. In so far as his ventures concerned the explanation of existing features of the moon's surface, it may be believed that Gilbert was exceptionally well equipped by his minute and appreciative study of existing features on the earth's surface; in this respect he had a great advantage over astromomers; and in so far as his ventures touched concepts of an astronomical nature in connection with his theory of moonlets, he was exceptionally well prepared by his unusual capacity for mathematical and physical discussion; in this respect he had advantage over most if not all geologists. One who was well acquainted with this lunar excur-

[4] It has lately been suggested that the circular form of lunar craters might be determined by the explosion of material vaporized by meteoric impact, whatever the direction of a meteor's approach.

sion of Gilbert's and who was especially competent to judge the quality of his astronomical discussions regards them as indicating a remarkable capacity in the vizualization and the statement of mathematical conditions and relations; for although the excursionist was little acquainted with the formal machinery of higher mathematics, he was surprisingly familiar with the concepts essential to their application to physical questions; he could state his problems in words with remarkable facility and clarity, even though they might be difficult to express in symbols and yet more difficult to work out. Nevertheless, it still seems that the necessity of tipping the moon this way and that after it had essentially reached its present size, in order to explain the systemless distribution of craters under the moonlet theory, is a serious embarrassment in a beautifully conceived process.

SCIENTIFIC SOCIETIES IN WASHINGTON

Gilbert's relations to scientific societies in the decade 1881–1890 have already been outlined. The expansion of those relations a decade later is here sketched. His five-year service as vice president of the Philosophical Society of Washington was succeeded by a one-year term as its president, at the close of which he read the noteworthy address on the "Moon's face," above noticed. He spoke before the same society in 1893 on the "Temperature of the earth's nucleus," and in 1894 on "Sedimentary measurement of Cretaceous time." A discussion of "Gravity determinations" in 1895 will be referred to later, under the heading of "Isostacy." All these subjects were eminently appropriate to the audiences before which they were presented, as they were concerned with the broadest aspects of geologic philosophy. The sedimentary measurement of Cretaceous time [5] was suggested by field work in Colorado, where he had found sections exhibiting regular alternations of limestone and shale involving 20 or more repetitions at several different levels; from this he inferred that some regular and periodic change of conditions must have determined the periodic changes of deposition; and he suggested that the change of conditions represented the cycle of the precession of the equinoxes. The change was supposed to be chiefly climatic and to affect the area whence the sediments found in the plains strata were derived.

> If the climate of a broad peneplain were by precession made alternately moist and dry, then during moist epochs it would be densely clothed with vegetation, subterranean waters would be highly charged with organic acids, so as to dissolve much lime carbonate, and mechanical degradation would be impeded by the vegetal mat. During dry epochs vegetation would be sparse, water would have little power of solution, and relatively rapid mechanical degradation would cause the residual clays to be transported to the ocean.

If, then, a deposit 4 feet in thickness represented one precessional cycle of 21,000 years, the total thickness of the strata concerned, 3,900 feet, would require 20,000,000 years for their deposition; an estimate which, it is noted, may be half or double the truth. The value of the precessional cycle as a measure of geological time was elaborated six years later in his presidential address before the American association, mentioned below; and at about the same time the use of the precessional period in the measurement of a recent time period was proposed in a review [6] of a paper on the latest epoch of the glacial period, in which 17 terminal moraines were said to occur between the farthest advance of the ice sheet at Cincinnati and its retreat to the Ontario basin:

> Postulating the astromical cycle of the precession of the equinoxes as the cause of the morainic cycle, the approximate time covered by the morainic history is computed . . . at 315,000 years.

As the time since the ice margin lay in the Ontario basin is measured by the age of the Niagara gorge, the total time since the last ice maximum was placed at 300,000 or 400,000 years. Both of these studies represent the strong interest that their author so often manifested in the quantitative aspect of geological problems.

Gilbert was a member of the Biological Society of Washington, but not a frequent speaker at its gatherings. On one occasion when he was present the meeting seems to have proved less entertaining than he had wished; he wrote about it to a friend as follows:

---

[5] Journ. Geol., iii, 1895, 121–127.

[6] Journ. Geol., vii, 1899, 621–623.

Last evening I attended a meeting of the Biological Society. Prof. Osborn of Princeton [later of the American Museum of Natural History in New York City] presented a review of Marsh's recent papers on the discovery of Cretaceous mammals. Merriam, president of the Society, had sent a notice of the meeting to Prof. Marsh and he was there to hear himself reviewed. Osborn undertook to show that Marsh had made seven species, five genera, and four families out of the teeth of one species—and more of the same sort. Marsh in reply said that when he wrote he had about 100 specimens, but that the work of collection had not stopped and now he had more than 1,000, including some jaws with the teeth in place. This later material was not fully studied but he was able to say that Osborn was partly right and partly wrong—which I thought, considering Marsh's peculiarities, a remarkably good response. So all our fun was quiet, and the proceedings of the Society on the Stanislaus were not repeated.

No explanatory notes will be needed by any reader of this memoir as to the biologists above named; but as some of the younger readers may be puzzled by the mention of the "Society on the Stanislaus," it may be well to state that its publications are not listed in the Royal Society's catalogue.

The allusion here made to Marsh may introduce a reference to the second of the few papers that Gilbert wrote on what seemed from its title, "Age of the Potomac formation," [7] to have to do with historical geology or with paleontology, but which proves on examination to be concerned chiefly with those aspects of physical geology that Gilbert habitually considered and, indeed, rather with the philosophy of that division of geology than with its facts. It appears that Marsh had assigned the so-called Potomac formation—a nonmarine deposit at the base of the Atlantic coastal plain series—to the Jurassic; and Gilbert asked for the "character of the evidence and the course of reasoning" on which the assignment was based. He was prompted to do so because in the absence of marine invertebrate remains, Marsh had been guided by vertebrate fossils, and the method by which such fossils are interpreted as time markers "must differ in an important way from the method ordinarily used by students of invertebrate fossils and fossil plants. As he [Marsh] has pointed out, land vertebrates are peculiarily sensitive to climatic and other physical conditions, and the evolution of new forms is consequently rapid. The life of a species is short and its value for purposes of correlation . . . correspondingly high. . . . But," Gilbert adds, "it appears to me *a priori* that this quality of rapid evolution is a two-edged sword," because it prevents correlation by widespread identity or close similarity of forms. Moreover, in point of fact the Potomac beds in New Jersey lie unconformably on Triassic strata below and pass insensibly into marine Cretaceous above; and Marsh seems to have interpreted these structural relations as indicating an intermediate time relation between Triassic and Cretaceous.

The geologist, however, infers that the unconformity beneath the Potomac represents a time interval, and consideration of the extensive dislocation and deformation of the Newark [Triassic] beds and of the enormous degradation they suffered before the deposition of the Potomac gives the impression that the time interval was very long as compared to the time represented by the Potomac beds themselves.

In closing, Gilbert remarked that he had "no intention to controvert Prof. Marsh's view, but merely to show how desirable it is that he set forth the reasons therefor."

A later communication was made to the Biological Society of Washington in 1899, regarding the effects of a landslide by which the Columbia River had been dammed at the Cascades, "not less than 350 years ago"; the observations on which this study was based were made on the return from the Harriman Alaska expedition, and the results were presented to a biological society because they had to do with the explanation of a submerged forest.[8]

The National Geographic Society had Gilbert's services on its board of managers from 1891 to 1900, except when he acted as vice president in 1896 and 1897. He addressed its meetings on various occasions, his subjects being "Coon Butte," in 1892; the "Great Basin," in 1898; and the "Glaciers of Alaska," in 1899. He also gave, in January, 1894, under the auspices of this society six lectures on the "Shaping of the earth's surface," apparently repeated from a similar course for Washington teachers in the last months of 1892. He contributed a

[7] Science, iv, 1896, 875–877.
[8] Science, xi, 1900, 99, 100.

popular article on the "Origin of the surface features of the United States" to the society's Magazine in 1898, and he was of course selected to prepare an account of Niagara Falls in the society's short-lived "National Geographic monographs," published in 1895. This excellent account is referred to in another section; the previous article is of a much more popular nature; diastrophic processes of telluric origin are here called Plutonic, and erosional forces largely of solar origin, Apollonic. The following extracts will illustrate the style of presentation here employed:

The Central Plain, comprising half of all the land [of the United States], has been shaped by Apollonic forces. The geologist tells us of many uplifts, dislocations and flexures of the crust, but all these have been reduced to approximate evenness by the cooperative work of rain, frost and rivers. Where hollows were made they have been filled; where hills and mountains had grown they have been pared away, so that only their roots, with a few low stumps, remain. . . . [In the Great Basin] the ranges are definitely Plutonic, each one having been caused by a distinct local uplift. . . . Through extensive districts [of the same region] the alluvial waste from the erosion and sculpture of the ranges is gathered in the intervening valleys, making of each one a shallow basin or gently concave plain, where roads may run at will. Here and there some of the ranges are almost buried by the alluvial filling, so that their summits project as craggy islands above a sea of rock waste.[9]

Gilbert served as a manager of the Cosmos Club in Washington from 1890 to 1892, as its vice president in 1893, and president in 1894. This appears to be the only instance in which he accepted office in a nonscientific organization, if the Cosmos Club may be so called. There is no record of his having taken part, even as ordinary member, in any political organization or in any society for public welfare; or of his having served on any paid or unpaid commission, civic, State, or national; not that he was indifferent to good government and a better social order, but that he did not directly concern himself in such matters. He was as nearly as possible exclusively scientific in his activities.

THE GEOLOGICAL SOCIETY OF AMERICA

On the formation of the Geological Society of America, an organization which has come to be second only to the national survey in its influence in American geology, Gilbert was listed among its original members. He urged that the first council of the society should be selected with care, in order to secure a proper regulation of its affairs from the outset:

The Society's publications should not be suffered to afford an asylum for inferior papers unable to secure publication elsewhere.

At the first winter meeting, held in New York City in 1889 under the presidency of the venerable James Hall, he spoke on the "Strength of the earth's crust," a subject that is here further discussed in a later section on "Isostacy"; he was subsequently one of the most regular and most highly valued attendants at the society's meetings for a number of years. An incident thoroughly characteristic of him is recalled from the New York meeting. Russell, reporting on a journey in Alaska, called attention to the prevalence of high and steep bluffs along the right bank of the Yukon; whereupon another member suggested that such bluffs might be plausibly explained by the lateral erosion of the river under the influence of the earth's rotation, which tended to deflect rivers to the right in the Northern Hemisphere with a force that increased with the sine of the latitude and which should therefore be unusually effective in a river in the high latitude of Alaska. Gilbert rose at once to make a correction, saying he desired to point out that the deflective force did not increase with the sine of the latitude as the other member had just asserted, but with the sine of twice the latitude, and that its maximum would therefore be, not at the pole, but in latitude 45°. This statement caused consternation to the previous speaker, who although confident of having correctly quoted the formula for the deflective force, was at the same time fully aware of Gilbert's competence in the subject and who had indeed had occasion previously to admire that competence as exhibited in his article above noted on the "Sufficiency of terrestrial rotation for the deflection of streams"; and he therefore, on catching a glance from Gilbert as he sat down, expressed his consternation by a significant movement of his closed hand, but said nothing further. Gilbert also remained silent until after another paper had been presented, when he rose, saying:

[9] Nat. Geogr. Mag., iv, ix, 1898, 308-317; see pp. 312, 311.

Mr. President; as I sat down after the remarks I made a few minutes ago, I noticed that the speaker who had preceded me and whose statement I attempted to correct, made a threatening gesture in my direction. That caused me to reflect on what I had said; and upon reflecting I discovered that I was wrong. I therefore wish to withdraw my remarks.

As the two disputants were walking to lunch together in the noon hour, the one who had made the "threatening gesture" expressed his surprise that the other should take the trouble to announce his mistake before the whole meeting; to which the senior and wiser disputant replied:

If you have made a mistake, the very best thing you can do is to correct it yourself as soon as possible, so that no one else shall correct it for you.

The nominating committee of the Geological Society wisely suggested that as the society had had James Hall, James D. Dana, and Alexander H. Winchell for its first three presidents, Gilbert should be its fourth and preside at the meeting to be held at the end of 1892 in Ottawa, or as he himself phrased it in his presidential address, in "the land of the sable and the beaver" after a "three-year sojourn in the land of the raccoon and the opossum." The address was on the "Problems of the continents," and its avowed purpose was not either to enlarge upon recent contributions or to summarize them, but to state "some of the great unsolved problems which the continents proposed to the coming international congress of geologists" at the World's Fair in Chicago the following summer. It was a thought-provoking presentation, but less satisfying to passive hearers, who might wish chiefly to learn conclusions, than to active minds, who were themselves pressing forward toward the unknown in geological science. He doubted the persistence of continental growth, and advised that the inquirer, if he would reach the truth, should "go behind the postulates" on which that widely accepted view was based; he considered the relations of crustal rigidity and isostacy as conditions of continental maintenance; and he was, as the lamented Barrell later put it, "twenty years in advance of his time in the appreciation of the large significance of unconformities" as indices of continental movements. The address was characterized by a forward-looking comprehensiveness. Its closing passage is as follows:

The subject of the continents affords no less than half a dozen great problems, whose complete solution belongs to the future. It is not altogether pleasant to deal with a subject in regard to which the domain of our ignorance is so broad; but if we are optimists we may be comforted by the reflection that the geologists of this generation at least will have no occasion, like Alexander, to lament a dearth of worlds to conquer.

The printed proceedings of this well conducted society duly record, regarding the winter meeting at Ottawa, that on Wednesday, December 28, 1892—

at 10.20 a m the President called the Society to order and after a word of salutation introduced His Excellency, the Governor General of Canada . . . who extended a hearty welcome to the Fellows of the Society. Science, he said was cosmopolitan and did not admit of distinctions of race, creed or national boundary; as far as science was concerned, all were one brotherhood. He assured the visitors that they would be shown every hospitality while in the city. The President made reply to the welcome of His Excellency, referring in complimentary terms to Canadian hospitality.

But how pale is this formal record compared to the informal narrative of the event in one of Gilbert's letters:

We had a good meeting and lots of fun "on the side." They were disposed to make much of us and we were not indisposed. His Excellency, the Governor General of Canada, gave us an address of welcome and afterward a reception, and not only he but the Premier and the Minister of the Interior attended our dinner and listened to some of our proceedings. . . . The Local Committee was in a fever of anxiety lest we Americans would fail to show proper deference to His Excellency, and the manifestations thereof added to our entertainment. On the day of our opening session, H. E. repaired to his office and (doubtless said his little speech over while he) waited to be summoned by messenger. The L. C. proposed that we call the meeting to order and mark time while H. E. was summoned, and I compromised by agreeing to call to order when H. E. was seen actually on his way across the ground. He was met at the door of the building by the President of the Royal Society of Canada and the Director of the Geological Survey who escorted him upstairs. I met him at the door of the room and escorted him to his chair, while the G. S. A. and the L. C. rose and cheered. H. E. was followed by an A. D. C. (aid-de-camp) in gaudy uniform with a sword altogether too large for him, but H. E. himself wore the ordinary togs of a gentleman. Then he made his speech and I made mine, and H. E. and the A. D. C. went out.

How much more interesting history would be if it included all the little personal items that ought not to be publicly told as well as those that ought to be! If further evidence in support of that generalization is wanted, let another passage be quoted from the above letter, which shows that, genuine as Gilbert's pleasure surely was when the Geological Society elected him to its presidency, he was glad to relinquish the office after faithfully filling it. "A presidency," he said, "is one of the things for which one is twice glad. I am rejoicing just now that my term of office has expired in two societies." The other one was the Philosophical Society of Washington, to which reference has already been made. He was evidently not of the kind of officer who not only holds office but holds on to office.

Gilbert spoke on "Chemical equivalence of crystalline and sedimentary rocks" at the Boston meeting of the Geological Society in 1893; and on "Sedimentary measurement of Cretaceous time" and "Lake basins created by wind erosion" at the Baltimore meeting of 1894, the last two topics having been suggested by his field work in Colorado. The first of the two was also presented before the Philosophical Society of Washington, in connection with which a note upon it has been given above. The second was one of the many smaller subjects to which Gilbert so often gave illuminating attention. Three kinds of lakes were found on the plains: Those of one kind occupy hollows in deposits of glacial drift; those of the second are barred by sand dunes; those of the third appear to occupy basins excavated by the wind. These lakes "are so shallow that one may wade across them in any direction. They have no outlet and no persistent inlets. Their catchment basins are small. Ordinarily their basins interrupt divides between stream valleys, and they often rest upon the highest tables of their vicinity. They are not permanent, but appear and disappear as storm and drought alternately prevail. Some basins are ordinarily dry, holding water only for a few days or weeks after a thunderstorm. The lakes of others are apparently perpetual, disappearing only after a succession of dry seasons." [10] Of all the features noted in this concise induction, the location of the lakes near the divides where water action must be weakest appears to be most conclusive in pointing to the origin of their basins by wind action.

At the Washington meeting of December, 1896, report was made on "Old tracks of Erian waters in western New York," a subject in which he had made some remarkable discoveries during the preceding half year, as will be told in a later chapter on Niagara, which will also refer to a paper on "Glacial sculpture in western New York," presented at the December meeting in New York City in December, 1898. A year later in Washington he described a submerged forest in the valley of the Columbia River, which he had examined during return from the Harriman expedition to Alaska. It thus appears that the Geological Society of America admirably served its purpose, not only in the scientific way of promoting the early announcement of original studies by its members, but also in the personal way of bringing together its members, old and young, so that they should come to know each other. Gilbert in particular was one of the seniors whose presence gave as much pleasure and profit to the juniors as his precepts.

THE NATIONAL ACADEMY OF SCIENCES

The spring meetings of the National Academy of Sciences usually found Gilbert in attendance, but he spoke rarely. A paper on "An American maar," giving an account of Coon Butte, was announced for April, 1892, but this subject was treated much more fully before the Geological Society of Washington at the end of the same year. He seldom attended the autumn meetings of the academy in other cities, but an exception was made in favor of Baltimore in 1892, when he spoke on the "Evolution of the moon," this subject also receiving much fuller treatment in the Philosophical Society of Washington, as noted above. The only other part that he took in the academy's activities was to present by title at the spring meeting of 1897 a memoir of G. H. Cook, State geologist of New Jersey. From that memoir, an earlier number in the series to which the present memoir belongs, a passage may be disinterred for reinterment here, a passage that is peculiar not only in the care taken to define a group of

[10] Journ. Geol., iii, 1895, 47–49.

categories before placing the subject of the memoir in the group to which he belonged, but also in the wholly unconventional Powellian style employed in the definitions. Gilbert first points out that besides pure and applied science there is also practical science, namely, "research which is prosecuted for the sake of, and with constant reference to, definite utilitarian ends, so that the discovery and the application of natural laws are parts of one process"; then the three kinds of science are further defined by analogy:

The love of pure science is the blind instinct of civilization. It loves to lay eggs—the more, the better—but gives no thought nor care to their hatching. Applied science is a working bee who builds cells of utility, and in them rears to maturity the larvæ hatched from her sister's eggs. Practical science may rather be compared to intelligent parentage, which not only conceives and bears, but nourishes and rears its progeny, foreseeing the end from the beginning.

Having thus made his categories clear, he places Cook in the third; for he "devoted his life to practical science. A study of his works shows that every research was for a practical end, and that end was steadily kept in view."

Curiously enough, at a meeting held in memory of Powell by the Washington Academy of Sciences in 1904, Gilbert followed even more rigidly the same categorical method of treatment in paying tribute to his chief: "Those who labor for science do three things: They observe the facts of nature, taking pains to observe them accurately; they arrange the observed facts in groups, or classify them; and they discover the relations of cause and effect, or explain them;" and a fourth thing is done by others who apply science. "Powell's work in geology included observation, classification, explanation, and application to welfare." This is as if, with the memory of the man, came an imitation of his style of expression. The rest of the tribute has more of feeling, less of logic.

OTHER SCIENTIFIC SOCIETIES

With the organization of the Geological Society of Washington in 1893 Gilbert was placed on the council for the first year and elected vice president for the second year, when he spoke on "Le Conte's physical theory" and on "Pig-backs of Dry Creek [Colorado]," the latter unconventional title probably serving as a diminutive of hogbacks or monoclinical ridges. He was elected president for 1895 and in that year spoke on "New light on isostacy," and gave his remarkable address on "The origin of hypotheses," a discussion of the origin of Coon Butte, analyzed on another page. He described some "Laccolites in southeastern Colorado" in 1896, a subject already referred to in the chapter on the Henry Mountains; and gave accounts of "Niagara gorge" and "Niagara whirlpool," in 1897; these topics will be here touched upon again in a later chapter on Niagara and the Great Lakes. In 1898 he described "An American boulder pavement," "An anticlinal ridge in an alluvial terrace," and "Ripplemarks and crossbedding." In 1899 he discussed the "Origin of gravels." The range of interest thus indicated is broad, but the topics treated were relatively restricted. It may be presumed that the younger members of the national survey, who constitute a majority in this society, not infrequently found the manner of Gilbert's treatment of greater value than the subject treated. It must surely have been of great profit to them to see that a geologist of the highest rank still found facts of relatively small dimensions worthy of his careful consideration; and yet a greater profit to perceive the purely intellectual quality of the consideration that he gave them. He was always so sane, so calm and dispassionate, so free from controversy and personal bias. His observations of facts were accurately reported, his explanatory analyses were carefully thought out, his presentation was delightfully clear, and withal his judgment of conclusions was cautiously restrained.

When the Washington Academy of Sciences was founded in 1898 Gilbert was elected its first secretary, and at the close of his year's service he put on record a history of the origin of the academy, a matter in which he might well be imitated by the secretaries of all other academies. In the following year, when he served as vice president, he spoke on the "Glaciers of Alaska," shortly after he had returned from the Harriman expedition to that wonderfully glaciated coast; and in 1900 he discussed "Photography as an aid to physiography." Except his memoir of Powell, referred to below, he presented no communications thereafter, although he was a frequent attendant at the meetings of the academy until 1905; after that date the indices of

successive volumes of proceedings do not contain his name, although they make frequent references to "Gilberti" as attached to new species of plants and animals described by his admirers.

After his famous Niagara lecture at the Toronto meeting of the American Association for the Advancement of Science in 1889, Gilbert's contributions to its gatherings were of minor importance for several years; but when the association met again at Buffalo in 1896, 10 years after he had presented his first essay there on the age of Niagara, he reported upon new studies of that river, as well as on the temporary "Algonquin River," by which the upper lakes were for a time drained eastward across the Province of Ontario without passing through Lake Erie; and the following year, at Detroit, he discussed "Earth movements in the Great Lakes region;" both these contributions are elsewhere summarized. In the autumn of 1899 the death of Edward Orton, who had been elected to the presidency of the association the previous summer, left that office vacant; Gilbert was chosen to fill it and was thus given one of the highest marks of scientific approval that an American scientist can receive. He presided at the meeting in New York in 1900 and there read an address on "Rhythm and geological time," which proved so acceptable that it was printed in full in three independent publications,[11] an honor rarely accorded to any similar essay, and the more remarkable since no definite or striking results were announced in it. The treatment of the subject was suggestive rather than conclusive; its presentation was, however, developed in a highly characteristic manner, beginning with simple and familiar examples of short-period rhythms, and ending with the precession of the equinoxes. The leading suggestion was that apparently periodic alternations of sedimentary deposits, such as he had seen in Colorado eight years before and had briefly described before the Philosophical Society of Washington and the Geological Society of America in 1894, may be due to some long-period rhythm in the physical processes that control erosion and deposition; and inasmuch as recorded descriptions of stratified deposits do not suffice to detect the possible consequences of such rhythms, he urged an outdoor "search for records of the ticks of the precessional clock."

Gilbert's pronounced liking for the mathematical treatment of problems may be perceived in a number of his papers, especially in his account of the moon's face, already analyzed, and in his several discussions of isostacy, to be described later. Still another illustration of the same mental habit is indicated in one of his reports to Powell, dated March 12, 1894: "I have been occupied largely during the month with the continued consideration of a plan for a machine to solve problems in least squares," apparently in connection with the adjustment of observations in topographic triangulation; but nothing seems to have come of this effort.

[11] Proc. A. A. A. S., xlix, 1900, 1-19; Science, xi, 1900, 1001-1012; Pop. Sci. Monthly, lviii, 1900, 339-353.

CHAPTER XXI

PERSONAL RELATIONS: 1891–1900

COLLEGE LECTURES

Gilbert's career as a college lecturer began in the eighties and culminated in the nineties. His first opportunity was at Cornell University in 1887, when he spoke twice on "Geological field work." Five years later it was proposed, as has already been told, to secure his appointment there as professor of geology, but he decided not to leave the survey. While he was considering this offer, which evidently attracted him, he seemed to have some fear that the geological market might be overstocked with young graduates, and wrote to one of those whom he had consulted in the matter:

> Perhaps if I go to C. I would better limit my ambition to liberalizing the education of future preachers, doctors and engineers.

After the decision to remain in Washington had been reached, he wrote again to the same correspondent:

> On the whole I am glad the talk of a professorship has talked itself out. The pros did not weaken with a closer view but the cons grew stronger. Several geological friends gave me Punch's advice and only one advised me to swap horses. The discussion has brought out the strong points of the horse now under the saddle and has made them actually a little stronger.

This was written about two months before the disaster of 1892; had the offer from Cornell been made half a year later, who can say what the outcome might have been!

It will be recalled that Gilbert had been offered and had declined a professorship at Brown University nearly 20 years before; and it may be at once added that he was asked to take a temporary appointment at Harvard University for the year 1898–99, during the absence of one of the staff in Europe; but he declined this invitation also, chiefly on the ground that the labor of removal from Washington for so short a time and the preparation of the many lectures called for would be too heavy a burden. How many other similar offers he received is not known; but the one from Cornell evidently was the most tempting to him. During the last 30 years of his life he repeatedly advanced small sums of money, chiefly on the advice of his friends in Ithaca, to needy Cornell students on the condition that the advance should be repaid without interest as soon as the recipient became self-supporting. He held to this condition not only because the money could then be used over again, but also because he felt it better for the recipient to feel a responsibility for repayment, an opinion that is shared by many educators. On his death he left a thousand dollars to be personally administered as a revolving fund for Cornell students under similar conditions.

To return to college lectures: In 1891 Gilbert spoke at Johns Hopkins University on Niagara; in the following year he addressed the Peabody Institute of Baltimore on the same subject and on the moon. It was in the closing months of 1892, just after the survey disaster, that he gave a course of lectures to the teachers of Washington on "Making and remaking the surface of the earth"; and this course appears to have been repeated, as already told, in January, 1894, under the auspices of the National Geographic Society, with the title, "The shaping of the earth's surface." Near the end of the same year he spoke at Cornell University on the Ontario basin.

His first long educational course consisted of 12 lectures on physical geography at the School of Mines of Columbia University in January and February, 1893, and during that time he had leave of absence from survey duties. It may interest future generations of economists as well as of geologists to know that he received $1,000 for this effort. It may also interest future generations of New Yorkers to know that he wrote to a friend at the conclusion of the course: "I enjoyed my lectures and would gladly have given a few hours more because the time was short for my subject—or subjects. Not being much acquainted in New York and

being under no necessity of becoming so I took my knitting with me and made nearly as good progress as I should have here," in Washington. It ought to cause merited remorse to any of the more liberal-minded future dwellers in Manhattan who cast a glance at that quotation to think that in their grandfathers' time commercialism was so prevalent in the metropolitan city that a man as distinguished in science and as agreeable in person as Gilbert remained unknown and neglected in his room, occupying himself with his "knitting" evening after evening, when he should have been invited to social gatherings. But those few New Yorkers of the nineties whom Gilbert did know and who prized his company while he was in their city will remember that what he called his "knitting" was the experimental part of his research on the origin of Coon Butte; namely, the firing of pellets of clay and bullets of lead into surfaces of the same material, in order to discover what sort of cavities would result. This research was performed chiefly in a laboratory of Columbia University and not in the solitude of his room. All the same, he did ply his needle there, as he confessed to an intimate correspondent:

My overcoat sleeve lining misbehaved and I set it right. Then while I was about it I inserted various other stitches in various garments, each time saving 800% profit.

There is no reason to think that he was unduly lonely, but he did miss the opportunity for congenial intercourse that Washington offered:

I manage to enjoy myself pretty well, but I don't see how those Columbians get along without a Cosmos Club.

However, he seems to have found lecturing an agreeable task, for in January and February, 1895, he gave a course of 10 lectures on physiographic geology at Johns Hopkins University, and with such acceptance that he treated the same subject in a course of doubled length during the first three months of the following year. The payment for the doubled course was $400; and by comparing this sum with that previously received from the School of Mines for 12 lectures, the value of lectures in Baltimore may be inferred, on the educational principle that the lecturer is worthy of not more than his hire, to be only about a quarter as great as in New York.

The kind of teacher that Gilbert would have made had he accepted any one of the professorships offered to him may be inferred on reading the dissent he expressed from the opinion of a reviewer of a geological textbook in 1896. The reviewer urged that "a textbook should be the exposition of a doctrine," and that it should be so planned that the student, "in his intellectual processes, acquires that habit of decision so essential to practical success." One might know that Gilbert, with his habit of deliberation and his unusual capacity for maintaining a suspended judgment, could not accept such a view. He wrote in reply: "It appears to me"—and in thus unconsciously adopting the opening phrase with which so many professors preface their remarks in faculty meetings he showed himself to that extent at least qualified to join their ranks—

It appears to me that something is to be said in favor of occasionally submitting to students alternative opinions regarding an unsettled question. The scientific text book which presents only facts and accepted principles, or gives only the author's opinion in open questions, must tend to leave the student with the impression that scientific knowledge is complete. The statement and discussion of rival hypotheses not only exhibits the actual incompleteness of knowledge, but illustrates the method of progress, and it appears to me quite as important to the world's future that the rising generatiou shall learn the method of research as that it become acquainted with the results of research. It may also be questioned whether the habit of decision inspired by the exclusive assimilation of positive ideas will usually lead to the best results when applied to the practical affairs of life. Problems of affairs resemble, in the complexity of their factors, the problems of such a science as geology; and the mind which habitually suspends judgments until various points of view have been considered may gain, through the wisdom of its decisions, as much as it loses through delay.[1]

In so far as this passage reveals the method that Gilbert would have employed with college classes, it indicates that his success would have been greater with advanced students than with beginners; but if one considers the practical good sense that he showed in many relations as well as the opinion that he here expressed, it can not be doubted that he could have soon

[1] Science, iv, 1896, 877.

learned to feed beginners chiefly with an abundance of well-described facts and of well-ascertained explanations, and not to expect them to show as a rule either appetite for alternative hypotheses or capacity for suspended judgment. As to tho "exposition of a doctrine" he would have without question strongly emphasized the modern geological doctrine of evolutionary uniformitarianism, and would have supported it by all manner of apt illustrations; it would have been rather with regard to mooted geological problems, such as isostasy and mountain building which are best treated before more advanced students, that he would have presented multiple hypotheses; but, although it has been venturesomely suggested as worth a trial, perhaps he would not have gone so far as intentionally to lead his advanced students to an erroneous conclusion—for even advanced students will swallow such a conclusion willingly enough if it is offered with a sugar-coated argument—in order to give them practice afterwards in the difficult art of changing their minds. Surely, had Cornell secured him in 1892 he would have made New York State as classic ground in physiography in tho next 20 years as James Hall had made it in paleontology half a century earlier.

In the same year with the doubled course at Johns Hopkins, the Brooklyn Institute heard Gilbert on volcanoes, the moon, and wind work; tho State university at Lawrence, Kans., heard him twice, on Coon Butte and volcanoes; and Chicago and Cornell Universities once each on Coon Butte. It may be added that the popularity of the last-named lecture was due much more to the philosophical manner in which its subject was treated than to the inherent importance of the little geographical feature on which it was based. In some cases, however, the lecturer appears to have carried the philosophical analysis of his subjects over far; as when he explained to a popular audience the 17 independent factors which had to be considered in studying the history of Niagara River and in determining the age of the Falls, for that was farther than such an audience cared to follow. Nevertheless, his lecture on this subject was repeatedly asked for; as at Cornell and at the city of Niagara Falls in 1897, at Vassar College in 1898, and at Teachers College, New York, in 1899. In the last year Gilbert spoke also at Ohio State University on "Edward Orton, geologist," this address probably being the substance of the memoir that he had prepared on Orton, his senior on the Ohio Geological Survey 30 years before, for the Geological Society of America.

LITERARY WORK

After Gilbert's withdrawal from administrative duties he accepted several invitations from publishers to take part in what is here called, perhaps inappropriately, literary work. Thus from 1893 to 1895 he prepared for a new edition of Johnson's Universal Encyclopedia—to which ho had previously contributed a few paragraphs in 1878—about 130 brief articles on geologists and on geological and geographical subjects, through which he plodded in alphabetical order. Just how and why he was beguiled into accepting a labor of this sort does not appear; the compensation received for it, which was fairly liberal as such compensations go, may have been an inducement, for his salary as geologist on the national survey was not lavish; and the authoritative quality of his articles may be somo compensation to the more studious fraction of the reading public for the distraction of the article-writer from studies of a higher order; but in the present connection this patchwork task is chiefly significant as illustrating the discontinuity of mental effort that seems, as already mentioned, to have characterized Gilbert's undertakings in the later years of his life. His contributions are good examples of concise treatment, the attainment of which cost him a considerable amount of effort. Among the articles is a very colorless biographical sketch of himself. The following extract from a paragraph on the Adirondacks may be taken as a sample of many others:

> The mountains consist of crystalline rocks, and about their flanks these are overlapped by Cambrian and Silurian sediments. Their surface characters were greatly modified by the Pleistocene ice sheet, which traversed them from N to S, scouring the soil from their summits and depositing the material in an irregular way in the mountain valleys, so as to obstruct the drainage and produce a great number of lakes.

The absence of reference to the bearing of the overlapping Paleozoic strata on the early history of the region and the exclusion of glacial erosion from a share in the production of the lake basins are noteworthy.

This encyclopedia work appears to have been done "between times," as his diaries do not show that he took time off from the survey for it; but in 1897 he had leave of absence without pay for November and December when he was engaged on the preparation, jointly with A. P. Brigham, of a school textbook on physical geography, the junior author preparing the first copy and the senior revising it. The first two months of 1898 and 1899 were similarly withdrawn from survey work to prepare definitions of geological and geographical words for a supplement to Webster's Dictionary. In 1906 he wrote the article on Niagara for the Encyclopedia Brittanica.

Reference has already been made to the lucidity of Gilbert's style. It may be here recalled that in his early years in Washington he found difficulty in writing his reports, a difficulty that was largely overcome a few years later; but it may be added that even to the end of his life he did not succeed in writing final "copy" in a first attempt; he habitually revised his manuscripts, making many corrections and additions, and their printed form was sometimes a second or third revision of their first. His style was not only lucid, it was genial and gracious also, especially in articles written to express his dissent from the views or conclusions of some one else. The rarity of exceptions to this rule make it the truer. One early exception in 1883 has been noted; another occurred about 15 years later, when he wrote in a short review:

> The paper is emphatic, not to say eloquent, in its characterization of the fatuity of opinions it contradicts, but the dangers which lurk in rhetoric are minimized by the suppression of names.

Those who knew Gilbert personally will feel sure that he laughed rather than scowled in phrasing that statement.

Unlike Powell, he had no noticeable mannerisms, and a careful reading of nearly all his reports and essays discovers very few words to which a purist might object. He made consistent use of "whose" as a possessive for nonpersonal nouns, a use that is provoked by the lack of a word for "which's"; he occasionally employed "transpire" not in its original sense of "come to be known" but in the sense of "take place," a sense that is widely warranted by American usage; and it may be believed that he liked American usage well enough to adopt it intentionally, for he was quick to recognize it; he once called attention to "sheepherders" as "occidental for shepherds." He was as a rule critically careful in his choice and use of words, his lively interest in their correct use being indicated by a brief article on "Interesting and important facts,"[2] in which he elaborated Powell's dictum that "a property is an essential characteristic considered in itself; a quality is a characteristic considered in relation to man." In his early Wheeler report he at least once used the phrase "the feet of a dozen ranges," although good argument may be advanced in favor of using mountain "foot" unchanged in the plural.

In that early report (1875) he did not use the shorter "-ic" instead of the longer "-ical" termination for certain adjectives, but wrote "geological" exploration, atlas, history, and information; "geographical" definition; "topographical" relief. His essay on the Colorado plateaus described them as a field for "geological" study (1876), and told of "geological" exploration, students, progress, map, and section. In the Henry Mountains report he wrote "geographic," but "geological." Whether it was Powell or McGee who led the national survey to adopt "geologic" instead of "geological"—except for the name of the survey itself, which is spelled according to congressional enactment—Gilbert followed suit, and thus contributed to obliterating in our scientific literature the shades of meaning suggested by such a phrase as "The geologic record is mapped in geological folios," although such shades of meaning are still advantageously expressed in separating historic events from historical publications, microscopic objects from microscopical societies, and economic geologists from economical geologists. While his essay on Bonneville in the second annual report of the national survey includes the phrase "geological" party, it includes also "geologic material," science, evidence, time, and period.

A few years later he wrote on the "Capitalization of names of formations,"[3] apparently favoring the third of three diverse usages; the first capitalized Potsdam and Carboniferous

---

[2] Science, xxi, 1905, 68.
[3] Science, iii, 1884, 59-60.

as nouns, but wrote them with small letters when used as adjectives, as in potsdam formation and carboniferous strata; the second capitalized both nouns and adjectives if derived from names of places, as Potsdam and Potsdam strata, but wrote descriptive nouns and adjectives with small letters, as the carboniferous and carboniferous strata; the third capitalized both substantive and adjective forms. Although the first and the third usages both have certain advantages, the third seems to have been preferred:

> The capitalization of all formation names has the manifest advantage that it enables one to say that the Carboniferous rocks are not the only carboniferous rocks.

The extracts already given from Gilbert's personal letters suffice to show that their style was informal and often jocose. They not infrequently exhibit a preference for a turn of phrasing that by its graceful indirectness leaves the interpretation of the intended meaning to the reader. Thus on arriving at a gathering in a distant city, he wrote to a friend: "I was gratified to find I did not have to introduce myself." A similar example has already been quoted from his letter about the attentions showered upon him by the city of Bath during his visit to England in 1888. He enjoyed certain western expressions that partook of this kind of indirectness. At a certain geological meeting a speaker had to confess his inability to answer a question that was raised in the discussion of his paper; Gilbert whispered to a neighbor: "You can't prove it by him."

It was during the period here considered that Gilbert adopted the principles and to an increasing extent the practice of "simplified spelling." The first announcement of this change in his ways was in a brief article in "Science," in which he requests that the compositor and proof reader shall not be allowed to nullify his attempt at reform in a review that he sent for publication; a very gentle attempt, as it involved only the words "groupt" and "addrest." He added that he made no criticism of the editorial policy of "Science" for "not joining in the spelling-reform movement, as it would be unwise for a journal with its own battles to fight to incur the odium which is attached to rational spelling. The prejudices in favor of irrational spelling are so strong and prevalent that they can not be opposed without a certain measure of sacrifice on the part of the opponent."[4] His officially published works and his dictated and typewritten letters are of course in orthodox spelling. In his own manuscripts he was more successful than many advocates of simplified spelling in changing the habit of a lifetime, for in addition to the phonetic use of "d" or "t" for the "-ed" termination of past participles, as in askt and labeld, he cut out unnecessary mid-word vowels, and omitted various silent letters, as in activ, bild, deth, dout, erly, evry, hav, hed, lact, lern, strait (for straight) topografic, wether. As already noted, his adoption of these simplifications resulted from his unconventional temperament, and was not a mark of either learning or ignorance.

The card indexes to American geological literature that were prepared under his direction when he was in charge of the Appalachians, as noted in an earlier section, led him further into bibliographic work than he had intended to go when the indexes were undertaken. He wrote in 1889:

> For more than a year past there has been a snarl in the Survey on the subject of bibliography. A number of geologists are preparing indexes to subjects or lists of papers or other aids to literary research, and their work before publication is checked up and criticized by the editorial division and the librarian, who have very different ideas as to what a bibliography should be. The discrepancies are so great and the differences are so difficult to adjust that the Director found he could not give the necessary time to their consideration and long ago he turned the matter over to me. But he has never till now allowed me the time to attend to it. I am systematically investigating the dozen or so bibliographic works in hand or awaiting publication and find myself getting much interested in the subject.

Hence in this problem, as in so many others, when wise action was needed to reduce confusion to order, Gilbert was appealed to. It may therefore be supposed that the bibliographic bulletins of the survey issued in later years—models in the way of thoroughness and completeness—have been prepared on a form in the standardization of which Gilbert had had a hand, and probably a guiding hand. With this experience behind him he was naturally chosen

[4] Science, v, 1896, 185, 186.

to represent the survey as a member of a committee, appointed by the International Geological Congress of 1891, to prepare a "Bibliography of geological bibliographies," his share being North America. After the completion of this compilation he wrote of it:

When a cooperative work of such magnitude is carried to a successful conclusion there is usually some one individual to whose skill and energy the success is due, and in this instance that person was M. Emm. de Margerie of Paris, the secretary of the committee.

It must have been, in part at least, because of Gilbert's repeated contacts with the art of bookmaking that he came to be critically appreciative of what makes a good book. His comments were of the pointed kind that give great pleasure to the author or the artist whose painstaking work is so often taken as a matter of course by the casual reader. Thus he wrote of one book:

The cuts are not only good, but uniformly and harmoniously good.

He also came to have definite views as to how certain parts of the bookmaking art should be practised; for he was one of those who believed that a book should be so constructed that its contents shall be clearly indicated, not only on its cover and in its table of contents, but also by its page headings. He stated his opinion on this subject clearly in 1892, when acknowledging the receipt of a large volume from one of his correspondents:

I received the ——— Society's quarto report day before yesterday and came near laying it on the wrong pile under the impression that it contained routine matter only. . . . The book has the same difficulty as the ——— only more so. It has a series of distinct essays with no change of page heading, and it has no table of contents. It thus requires the possible reader to turn the pages and dip into them in order to learn the scope of the book. It is a shame to find fault with a work in which I expect to find a feast when I get at it—but you see the defects of the husk came near keeping me from the kernel.

If he had had as much experience with European scientific publications as with American, the antiquated methods, long maintained by some overseas societies for concealing the contents of their proceedings, might have led him to even more emphatic expression of discontent.

A GENEROUS TRAIT OF GILBERT'S CHARACTER

The preceding notes on Gilbert's own literary work may serve to introduce mention of a generous trait in his nature which led him to express interest in the literary work of others, especially in that of his juniors, by writing them letters showing his appreciation of their product. Some of those juniors, now seniors themselves, vividly recall the pleasure and encouragement that such letters gave them. Thus, after a first effort in scientific reviewing, a young teacher received from Gilbert in 1891 and still treasures the following hearty recognition of his work:

I write to express my appreciation and thanks for your abstract of the climatic portion of my Bonneville report. It is but rarely that an author's work receives so appreciative a notice and I congratulate myself especially on the good hands in which I fell. Permit me to congratulate you also on the skill with which you have set forth the salient features. In case you have not a copy of the volume it will give me great pleasure to send you one.

Similarly, a letter written in 1893 to the young author of an essay on the physical features of a Southern State closed as follows:

I beg to congratulate you on the clearness and skill with which you have presented the matter, on the care with which you have assigned relative weights to your tentative and more mature conclusions, and on the eminent courtesy with which you have treated the work of others in a district where controversy seems to be spontaneous.

To correspondents with whom Gilbert was on more familiar terms, he wrote in a less formal but equally acceptable manner; thus the author of a report on which much study had been compressed into small compass received a terse message on a single page of note paper:

You have done a good work well. Nothing could better conduce to the making of geology interesting, or to the expansion of good governmental work. You're a brick.

One who did not know Gilbert might think that he was seeking advantage or favor in thus according praise; but such was not the case in the least. His frank expressions were doubtless

prompted in part by the knowledge that authors are naturally much interested in the reception of their essays by competent readers, and that their interest is best satisfied by the direct communication of their readers' opinions; but whatever he said in this way did not go beyond his genuine feeling, and his feeling was not guided by any ulterior motive whatever. Indeed his letters of commendation are, in their frankness and directness, of a piece with the comments, favorable and unfavorable alike, which he, when acting as censor for the Geological Society of America, habitually sent to the author of a censored manuscript as well as to the publication committee.

It was not only during his more active years that Gilbert pursued the genial habit of writing letters of encouragement as occasion offered; the habit was continued to the end of his life. He wrote in 1916 to a younger physiographer from whom he had received the notes of a hurried visit to a locality that he himself proposed to examine more at leisure:

> You certainly have a rapid-fire brain and note book, to get so much during a rapid transit. I am specially appreciative of such qualities because my own "reaction time" is lengthening.

And in the same year a geologist, who during his college days had studied Gilbert's reports as models of what one should aspire to produce, received from the master whom he esteemed so highly the following comment on a report of his own:

> I take the liberty, as your senior by at least a generation, to congratulate you not only on the importance of your contribution but on the effectiveness of its presentation.

The younger man might have, with good right, regarded that statement as an accolade. It may not be given to anyone to equal Gilbert as an investigator, but many whose best efforts fall short of his may to their own greater happiness try to practice his gentle art of giving direct expression to their pleasure in the work of others.

LOST IN PHILADELPHIA

It may surprise the reader to learn that a man as experienced as Gilbert in finding his way about in an unknown country should have once been lost in the city of brotherly rectangularitude, yet such was the case; and he enjoyed telling how it happened as an illustration of the mental process by which a traveler keeps his orientation in new surroundings. It was on the occasion of his coming by train from New York to Philadelphia late one afternoon, when perhaps from falling asleep on the way he did not notice that the Delaware was crossed at Trenton; but saw on approaching Philadelphia after dark half an hour later that a river was then crossed as the city was entered. Assuming that this river was the Delaware and that he was therefore crossing westward from New Jersey into Pennsylvania—although as a matter of fact he was crossing the Schuylkill eastward—he set his mental compass accordingly, kept his bearings as he turned a few street corners on the way to the hotel and a few corridor corners on his way to a room, and turned in for the night. The next morning, which it is to be presumed was cloudy, he set out, still guided by his mental compass and asking no question, although he was new to the place, to have a look at the Delaware; and reversing his course of the evening before walked as he supposed eastward, but really westward, until a river was reached. On arriving there, with the supposed north or upstream part of the watercourse on his left and the supposed south or downstream part on his right, he saw to his utter astonishment that all the shipping was on the upstream side of a bridge that had no draw, and that no vessels were to be seen on the other or oceanward side; and for a few moments he was literally "lost." It was only after inquiry that he could locate himself on the Schuylkill instead of on the Delaware, and much conscious effort was then needed to find himself by rotating his system of coordinates 180°.

HOME AFFAIRS

How far the unsettlement of Gilbert's home life distracted him from larger scientific achievement can not be told, but the continued illness and increasing inability of his wife must have made exhausting drafts on his sympathies and continually disturbed his peace of mind; for from the time of her loss of health in 1881 until her death 18 years later his care and protection of her never diminished. In the meantime the two sons had, after their mother became unable to care

for them, spent most of their growing years at boarding schools, and the separation from them grieved their father. He wrote of this to a friend in the summer of 1894:

> Archie has beeu home now for a month, and I am better acquainted with him than for years. One of the disadvantages of his absence has been that I have largely dropped out of his life except as a source of supplies and a stickler for accountability. I find him more of a man than I had expected and am glad to gratify his desires for a college course.

The elder son was therefore sent to Cornell University, where he was near his father's especial friends, the Comstocks, and where, as he seemed to possess some of his father's capacity in mathematics, he studied engineering. He later established himself in that profession in the West, much to his father's satisfaction. The younger son, Roy, did not carry his studies beyond school years; but he had the satisfaction of aiding his father occasionally by acting as carriage driver during field excursions in western New York and by typewriting some of his longer manuscripts.

When the boys were at home, their father enjoyed bicycle rides with them, and used to take advantage of such occasions to encourage the observation of outdoor objects. One of the boys recalls an incident which illustrates the extreme patience that Gilbert showed in his home. It was at the time when the study of the moon absorbed the father's attention, and when he used to resort to a room on the third floor in order to find quiet for his work. One day when he was thus secluded, one of the boys, feeling a wish for company, followed him there and, boylike, stood around whistling. The studious father bore the distraction silently as long as he could, and then, still silent, quietly carried pen, ink, and papers into an adjoining room. The boy followed him, still whistling. After a time the father went back to the first room; and the boy, not catching the point, again accompanied him. At last, unable to pursue his thoughts with the whistling boy so near, the father said in the most gentle voice:

> My boy, I can't work so well with you here whistling; don't you think you could go somewhere else?

The mother of the boys died on March 17, 1899, in Florida, where she had gone with a valued friend and where her husband followed on receiving word of her approaching end. The Corcoran Street house was thereupon given up, and the widower established himself temporarily in an apartment. In April of that year he had the fortunate distraction of going, with C. E. Dutton, geologist of the high plateaus of Utah and the Colorado Canyon, and W. H. Holmes, whom geology had lost to the profit of anthropology, to Mexico, as the guests and in the private car of G. W. Breckinridge, of San Antonio. Gilbert's attention was given primarily to the "phenomena of land sculpture and the relation of types of sculpture to climate." Among many features noted was the little cleftlike gorge, known as the "Infernillo," which lies along the side of a broad valley in the eastern margin of the central plateau and is followed for a short distance above the town of Orizaba by the railway between Vera Cruz and Mexico City. Gilbert recognized the gorge to result from the displacement of a valley stream from its former mid-valley course by a relatively recent lava flow, although his only opportunity for observation was from the train window.

In February of the same year Gilbert proposed to the director of the survey that "the new methods of geologic interpretation which have been rendered possible by the genetic study of topographic forms," and which "have been successfully applied in the Appalachian province," should be carried to the Cordilleran region; and that the work should begin at the north end of the Rocky Mountains and advance southward, or at the south end of the Coast Range and advance northward; but in case the expense involved seemed too great, an alternative plan of areal work in western New York was added. The larger proposal is, like his flying studies in Mexico, of interest in showing Gilbert's continued preference for physiographic geology. However, he did not carry out either plan, but went instead to Alaska as a member of the famous Harriman expedition. For a time after the death of his wife, Gilbert had an unsettled residence in Washington, but after returning from Alaska he was invited by Dr. C. Hart Merriam to become a member of his household at 1919 Sixteenth Street, and there he resided for a good part of all the remaining years of his life. The arrangement was one that gave happiness to all concerned.

GILBERT'S RELIGIOUS VIEWS

It has often been remarked that thoughtful men say little about their innermost feelings on religious subjects, and this was eminently true of Gilbert. Most of his friends never heard him mention such matters, but his silence did not mean that he had nothing to say. His convictions were as deep and as sincere as if they had been in every respect instead of in no respect orthodox. If it is here attempted to bring together from various sources some indications of his beliefs, the attempt is not made with the idea of prying into his private affairs, but with the wish to show again, as has so often been shown before, that a man may be pure in life and cheerful in living even though he rejects the dogmas which are supposed by those who still hold them to be essential to goodness and happiness.

Gilbert was essentially and consistently a rationalist. He stood by himself, thoughtful and independent, in all religious matters. He was a member of no sect; he rarely went to any church, and when he did go it was to listen to the preacher as he would listen to a lecturer. His beliefs were the product of his reason, not of his emotions; they were essentially ethical rather than theological. He saw clearly that man's advance in natural knowledge, based on observable evidence and logical inference, has long been accompanied by a decrease in his supernatural beliefs, derived from revelation or inspiration, so-called. He therefore trusted wholly to natural knowledge, and let the decrease of the supernatural take its course. He was much interested in the history of the successive steps in this decrease and knew that dangers were imputed to each step by those unwilling to take them; but he knew also that others who have taken some of the successive steps have found the imputed dangers to be only imagined; and he was therefore not dissuaded from taking his own further steps. His intellect was his guide, and he abandoned all views for which it gave no support.

Men like Gilbert would of course be called "disbelievers" by the modern conservative who, apparently unaware that the supernatural elements in his ancestors' faith during the Dark Ages were many as compared to the few that he accepts to-day, regards his own belief as a standard that has been fixed, permanent and constant through the Christian centuries; and who, ignorant of the great share that the intellect of great men has had in dispelling the superstitions of earlier times, looks upon any intellect that may lead to a less belief than the one he holds as a cold and self-willed guide. Yet Gilbert's nature was not cold, nor was his life selfish. On the contrary, he was warm-hearted; his sympathy was easily aroused, and his generosity was always responsive; his disposition was gentle, kindly, loyal, and helpful. Religiously independent as he was, he never sought to disturb the religious dependence of his friends. If his judgments were sometimes stern, they were always sincere; and they were applied with much more severity to himself than to anyone else. His will was strong, but it was controlled by a tender conscience. "Virtue is its own reward" was to him no empty platitude; it was the rule of his life; further reward he neither asked nor expected. He was self-trained in righteous living to a degree that is reached by few men. It would have been as unnatural for him to do anything that he felt was wrong, whatever the temptations to such a course, as it was natural for him to do anything that he felt was right, whatever the difficulties in the way. Those who knew him best know that, although at certain stages of his life the difficulties in the way of right doing were greater than can be here told, he never swerved from the path of duty as he earnestly conceived it. It is a remarkable tribute to this earnest disbeliever that one of his devoutly orthodox associates should say of him:

> Gilbert was one of the most Christlike men I ever knew.

Something of what Gilbert would have said, had he expressed himself on religious matters, may be inferred from certain passages in his memoir written in 1899, on that excellent and truly venerable geologist, Edward Orton,[5] with whom he had been associated during service on the Ohio survey 30 years before. It appears that Orton as a young man about 1850, experienced a "change of faith," when he replaced the Calvinistic creed in which he had been brought up for "the shorter statement of Unitarianism." It is explained that this change cost Orton "not only

[5] Bull. Geol. Soc. Amer., xi, 1900, 542-546.

loss of status as a Presbyterian minister, but the alienation of friends, and then followed a period of such unhappy remembrance that he afterward shrank from the mention of it, even to the members of his family." A comment on this is clearly indicative of Gilbert's own liberal attitude:

> It is to the credit of advancing civilization that here at the end of the 19th century our community finds difficulty in realizing how hard was the temporal way of the apostate at the century's middle. . . . It is proper to add that while his [Orton's] belief in the fundamental doctrines of Christianity survived the perils sometimes attributed to scientific training, his science suffered nothing from theological bias. His writings ascribe the phenomena of nature to natural causes, and his hypotheses seek verification by appeal to visible and tangible facts.

If one recalls in this connection the experience that Gilbert's father had on leaving the Presbyterian church in Rochester a few years before Orton's "apostacy," the above extract gains an almost autobiographic flavor.

Another memoir written at about the same time also gives expression to liberal feelings characteristic of the end of the nineteenth century rather than of its beginning or its middle. The subject was Joseph H. James,[6] and it is told that he lost his position in an Ohio college because of the "disruption of the faculty arising from religious prejudices. . . . When religious beliefs were under fire . . . professor James was accused of being an agnostic and defended as being essentially a Unitarian. So far as I knew it," Gilbert writes, "his religion was an unswerving devotion to science. Science gave him only a modicum of that fame which is dear even to the least selfish of her votaries, and she utterly failed to shield him from adversity, but his fealty endured to the end." These memoirs are significant because one may judge fairly well of Gilbert's own feelings on religious matters by what he said of such feelings in others, and especially by the way he said it. In this instance, the passage quoted clearly reads as if he believed that unswerving devotion to science was a good religion, and that unbroken fealty to it was to be admired.

Religion is usually so largely concerned with supernatural beliefs that omission of all mention of such matters in the preceding paragraphs concerning Gilbert's religious views may lead some readers to regard them as chiefly negative and as lacking in faith. It is not to be questioned that he went farther in his disbeliefs than many modern churchmen, even though they also are in large measure "unbelievers" because they have discarded the great majority of the theological complications which encumbered the faith of their early Christian forebears; but those who should judge Gilbert's views to be in large measure negative would fail to recognize the great affirmations that he founded upon his experience of life, and the loyal acceptance that he gave to the natural order of the world. To these positive principles he was devoutly attached. They represented his religion as fully as the most elaborate creed can represent the religion of persons who profess it. Indeed, as compared with the faith of those who refuse to "believe" unless they have miraculous evidence of another world where future reward is to be given for the encouragement of present righteousness, he, in unconditionally accepting the duty of right living as a part of the natural order of this world, had the greater faith.

---

[6] Amer. Geol., xxi, 1898, 1-11.

CHAPTER XXII

FIELD WORK IN COLORADO: 1893–1895

A TEMPORARY RESUMPTION OF WORK IN THE WEST

After the relinquishment of his office as chief geologist, in which he had to direct the field work of others, Gilbert was freed for field work of his own and spent the seasons of 1893, 1894, and 1895 in Colorado. It may be presumed that he was glad to be again "astride the occidental mule" anywhere in the open country, and his correspondence expresses a certain measure of satisfaction with the work he undertook; but on the whole it seems to have been performed in a perfunctory manner, more so indeed than any other field study that he ever conducted, with the possible exception of the irrigation studies that he made for the Powell survey in 1878. From the present point of view it is greatly to be regretted that he was assigned to the Great Plains instead of to the Great Basin, where the complicated structure of one of the basin ranges would have been a more fitting subject for his investigational skill than the areal geology of a quadrangle of nearly horizontal structure. The field of his work appears to have been determined by the fact that certain topographic maps were waiting to be geologically colored, if one may judge by a phrase in one of his letters to a friend, written in March, 1893:

> We are not investigated after all. It is rumored that the instigators of the investigation found they were unpopular with their constituents, but I have no notion whether truly or falsely. We do not yet—at least I do not yet—know just how the appropriations were finally arranged but we probably have 30 or 40,000 dollars more than for the current year—and that means for me that I shall get some field work. It is not likely to be in N. Y. but more likely in Kansas, where we have maps waiting for geological colors.

In spite of his apparent relinquishment of administrative duties and actual assignment to western field work, it was difficult for Gilbert to leave Washington early in the field season. Many office problems were still referred to him for solution. When he finally set out, his field proved to be a district of the Colorado plains around and south of Pueblo, instead of farther east in Kansas, as he had expected. The assignment caused him some apprehension, briefly expressed the following autumn:

> There is no probability that I shall ever complete the Pleistocene studies I began in the Erie and Ontario basins.

That must have been a disappointing thought; for having, as it were, acclimated himself by several years' work to Great Lake studies in the East in replacement of his earlier Great Basin studies in the West, it was hard to be turned away from the eastern field before he had finished his problems there; but as he was a true philosopher, he may have reconciled himself to his fate by reflecting that a Government geologist can not always pick and choose his problems for his own satisfaction, but must sometimes take up such subjects as are held to be most in need of solution for the advancement of the national welfare. Fortunately his apprehension that the Great Lake studies would have to be abandoned was not justified; he did much work upon them in subsequent years.

In the meantime quadrangles of the Colorado plains were Gilbert's field of study for three summers. During the long journeys over familiar routes thus occasioned he habitually busied himself with reading or writing. On taking his place in a Pullman car, he would call for a table and settle down promptly to work of some sort. This economy of his time was equaled by his economy of Government funds; for it is related that on a field season when one of his sons accompanied him in camp and tried to help provision the party by shooting rabbits with a pistol, the father charged the survey only with the cost of cartridges used for the few successful shots, and paid for the many more unsuccessful ones himself.

An unsatisfactory result of the first season in Colorado was the discovery that the area assigned for study was so poorly represented on the topographic maps which were to be geologically colored that it was found necessary to have at least one of them resurveyed; and this is the

more singular in view of the fact that the inaccurate map was the work of Gilbert's former assistant in Utah, W. D. Johnson. The fault in the matter may be reasonably ascribed rather to the system in vogue during the earlier years of the survey under which Johnson had been required to work—a system that demanded a certain area to be surveyed in a certain time—than to any lack of skill on his part; for other erroneous maps of early production are known to have been due to that time-and-area method, and Johnson is known to have been an expert topographer, with a natural as well as trained capacity for representing relief by contours; witness the beautiful maps of the San Francisco district, made under his direction while Gilbert was in Colorado, and witness also his selection as topographer when Gilbert undertook a study of certain basin ranges in 1901, as will be told below. But apart from the difficulty of having to work with an inaccurate map and from the not infrequent annoyance of swarms of mosquitoes, Gilbert appears to have enjoyed the return to field work, as the following extracts from personal letters written in the summer of 1893 will show.

> Everything but the entomology is charming. The geology is full of anticlinals and baselevels, and the climate averages delightfully. . . . What is happiness? "The soul's calm sunshine." True enough, but too abstract and metaphoric; give us something specific. Well, specifically, happiness is sitting under a tent with walls uplifted, just after a brief shower, when most of the flies have quit lighting on the lobster-red wrists burnt during the morning ride, and gone off to see what the cook is going to do next, and when the thirsty air is rapidly exchanging its heat for the moisture left by the shower. It is rising at 4.30, while Jupiter is still palely visible but there is no longer any temptation to hunt for the comet, taking a sponge bath in the open, breakfasting from off a box lid gaudily decked by a painted table cloth, and then sallying forth on the white horse Frank to study the limits of the alluvial veneering on the base-level mesas, measure the dips of rows of rusty nodules, sketch problematic buttes, and gather the houses of Ammonite, Scaphite, and Hamite. It is going to bed by early candle light in the midst of a grove of Rhus tox, hunting the double stars near Lyrae and Cygni among the branches of overhanging cottonwoods, moralizing on the development of character through the trying associations of camp life, congratulating yourself that you are not a pessimist, and finally dropping off to sleep.

Among the "trying associations of camp life" may have been a trouble with one of the hired helpers; for a member of one of Gilbert's parties recalls that he never saw his chief so near "getting mad" as when a driver, thinking he had the chief at a disadvantage, attempted to raise an agreed wage on him.

In the second season it was August before Gilbert could escape from the Washington office. He wrote of the delay:

> This year again the Fates are perverse. Work that I could not refuse and must not neglect holds me here when I ought to be scouring the plains—so to speak. . . . Fortunately the energy I stored up by last year's outing has kept me from wilting as I am apt to in this summer climate and it has only been for a few days I have felt the physical need of a change.

One of his letters spoke of "luxuriating in camp life on the Plains," as if freedom from duties in Washington were truly grateful. During this season he was accompanied by two assistants, both novices but both assiduous and successful to the point of giving satisfaction to their chief. He nevertheless seems to have enjoyed trying them out in various ways, as well as aiding and encouraging them in their work. One of them sometimes gave trouble by remaining out of camp so long after dark that searching parties had to be dispatched to guide him in; on such an occasion, when there was nothing with which to make a beacon fire, it is recalled that Gilbert, as one of the searchers, walked out for a certain distance on a straight path guided by the stars, then turned 120° to one side and walked the same distance, and finally making a similar turn came back to camp. The incident is here recorded not because there is any great marvel in its performance, but because it seems to exemplify a practical application of Gilbert's love for mathematical problems, for an equilateral triangle is about the best route on which a searcher can cover the greatest amount of ground farthest from camp in a given time.

Camp fare was varied from tinned meat to rabbits and prairie dogs; and the unconventional leader of the party even argued that, as rattlesnakes live on clean food such as birds and frogs, they ought to be good eating; so one was killed without being allowed to bite itself, and as the conventional cowboy cook refused to have anything to do with serving such a dish, it was geologically prepared for the camp table and found to be "really very nice," but it does not

appear to have become a regular article of food. However, the single trial was a practical example of what Gilbert would, in connection with accepted scientific opinions, have called "going behind the postulates"; it illustrates a readiness to control behavior by logic rather than by habit; but this readiness was so well tempered on Gilbert's part that it did not make him the fatiguing companion that most logic-controlled men become. All who have been in camp with him testify to his sympathetic enjoyment of nature and to his unusually entertaining manner of talk. He encouraged his assistants on the Colorado plains to think for themselves and to form working hypotheses by asking their opinion on the meaning of new landscapes; and he increased their appreciation of outdoor life by pointing out the double stars mentioned in the letter quoted above, as well as the elliptical outline of the morning sun as it rose over the even horizon of the plains, and the evening sky-shadow of the mountains over the shadow of the earth in the east after sunset. It was a liberalizing education to work in the field with him. Trouble was again found in using the base map, and one of the assistants, who had been a member of the topographic branch several years before, gave "nine days to topographic sketching to secure a base for the mapping of boundaries and faults in places where the engraved map proved incomprehensible."

PUBLISHED RECORDS OF WORK IN COLORADO

The chief published products of these three seasons of field work are a report on "The underground water of the Arkansas Valley in eastern Colorado,"[1] the Pueblo geologic folio, and several short essays. The report is for the most part a straightforward geological account of the successive strata in the district treated, but it contains an important generalization which is outlined in the next paragraph. A small but characteristic illustration of Gilbert's clarity of statement is: "The ocean is the 'base-level of erosion'" (575), which is significant because of its definiteness in contrast to the vagueness of Powell's original use of the term. Another statement gives an account of the production of flood plains by torrential water courses:

> On the outside of each curve of its course the torrent digs into the bank, so that the river encroaches on the land; but the channel does not grow broader, because in the quieter water on the inside of the curve, some of the sand settles from the water and new land is thus built up. The whole flood plain of the river has been formed in this way. (578).

This simple explanatory statement is here quoted, not because of its profundity or originality, but because Gilbert thought such an explanatory statement was worthy of formal official publication in 1896.

Much the most important product of Gilbert's work in Colorado was the recognition of a largely fluviatile origin for the fresh-water Tertiary strata of the plains, which, like similar strata in basins among the mountains farther west, had been universally treated as of lacustrine origin by all other geologists. A lacustrine origin had indeed been announced for the Tertiaries of the plains in an essay in an annual report of the survey published no earlier than a year before the one which contained Gilbert's discussion; but he did not treat the problem historically and no reference to previous interpretations was made. The new view was briefly and confidently announced, and its inferred processes were asserted as facts. The strata in question were described as lying unconformably upon Cretaceous beds, the long-continued erosion of which had made the region of the Great Plains "more nearly level than it is now." The change from erosion of the Cretaceous to deposition of the Tertiary strata was—

> brought about by some modification of conditions which is not yet clearly understood. Perhaps the plains region was depressed at the west, and the slopes thus rendered so gentle that the streams could no longer carry off the detritus which came from the mountains, and it was deposited on the way. . . . Whatever the cause, the streams . . . filled their channels so that their beds lay higher than the neighboring country . . . and they thus came to flow in succession over all parts of the plains and to distribute their deposits widely, so that the whole plain in the district here described was covered by sands and gravels brought from the canyons and valleys of the Rocky mountains. . . . The chief material is coarse sand . . . in irregular beds with much oblique lamination.

This was the beginning of an altogether new conception of the physiography of the Rocky Mountain region during Tertiary time.

[1] 17th Ann. Rep. U. S. Geol. Survey, 1896, Pt. II, 557-601.

Gilbert himself had in earlier years accepted without particular inquiry the orthodox lacustrine interpretation of certain fresh-water Tertiaries in the high plateaus of Utah; but his subsequent experience with the Quaternary clays and marls of Lake Bonneville gave him a better understanding of the composition and structure by which lacustrine deposits, laid down in quiet and comparatively deep water, must be characterized. There is, however, nowhere any indication that he, while working in Bonneville, harked back to the Tertiaries of Utah plateaus and saw that, in view of their frequently varied texture and irregular structure, an altogether lacustrine origin should not be attributed to them. It was not until later, when he came to know the obliquely laminated, sandy strata of the plains, that their contrast with the regular stratification of the fine-textured Bonneville sediments appears to have compelled an interpretation chiefly by fluviatile processes; and even then nothing was said regarding a similar origin for any of the Tertiary formations in the intermont basins. A year later Haworth, after quoting extracts from Gilbert's Colorado report, extended the fluviatile explanation eastward into Kansas; and in 1900 Johnson brought out his comprehensive essay on the high plains,[2] in which the brief explanatory statements made by Gilbert about the Tertiaries of eastern Colorado were expanded in a truly philosophical manner and extended over a much larger area.

The shorter articles that Gilbert based on his studies of the Plains concern the measurement of Cretaceous time, elsewhere referred to, some small laccoliths to which reference has already been made, and, jointly with one of his assistants, an account of "Tepee buttes,"[3] or small residual eminences like Indian wigwams or "tepees" in form, which owe their survival over widespread surfaces of degradation to the presence of colonies of fossil shells. Gilbert gave much attention to fire clays during this period, and many samples acquired by correspondence were analysed for him; but no report of the results gained was published. The average composition of sedimentary rocks was another subject that he took up with the assistance of the chemists of the survey during these years; it was as if his active mind did not find sufficient occupation in examining the simple structure of the horizontal strata of the plains, and he was therefore driven to consider certain large problems in connection with their origin. Much time must have been given to measurements and calculations, but the statement of the results reached was published only in abstract; its chief conclusion was that "assuming the sedimentary rocks to have been supplied by erosion from the crystallines . . . somewhat more than a mile in thickness of crystalline rocks upon areas equal to all the present land of the globe must have been worked over to give our sedimentary rocks." [4] All these subordinate problems are good enough in their way, but they were rather random and discontinuous efforts; and that they should have taken Gilbert's attention from larger problems was as inappropriate as that he should have had, when he was chief geologist, to advise other members of the survey as to the manner of keeping their accounts and as to the choice between buying a bicycle and hiring a buggy for transportation.

THE PUEBLO GEOLOGIC FOLIO

Only one geologic folio, that of the Pueblo quadrangle, was published under Gilbert's authorship. The Apishapa quadrangle adjoining on the southeast was surveyed under his direction in 1894, but the folio was not published until 1912, when it was edited by one of Gilbert's assistants after revision in the field. Besides the standard series of maps, the Pueblo folio contains a stereogram made under Gilbert's direction, representing the warped and faulted surface of a standard stratum, uncovered where still buried and restored where already eroded, and also a special map indicating by underground contours the depth of the uppermost water-bearing strata. It will be remembered that, as to stereograms, Gilbert had nearly 20 years before prepared two excellent examples for the illustration of his Henry Mountains report, and that these plates had inspired the preparation of similar illustrations by de la Noë and de Margerie for their "Formes du terrain"; it may be added that this Pueblo stereogram is probably the best example of its kind that has been produced since the Henry Mountains report appeared.

[2] W. D. Johnson, The High Plains and Their Utilization. 21st Ann. Report., U. S. Geol. Survey, 1901.

[3] Bull. Geol. Soc. Amer., vi, 1895, 333–342.

[4] The chemical equivalence of crystalline and sedimentary rocks. Amer. Geol., xiii, 1894, 213–214.

The text of the Pueblo folio adheres to the scheme adopted for the atlas series in attempting to give a generally intelligible form to a condensed account of geological structure and surface features; and it must be said that the scheme is in this instance applied with greater measure of success—that is, of success to be measured by the capacity of a nongeologist really to understand the text—than is usually the case, for Gilbert's descriptions are simply phrased and are exceptionally free from technical terms. But, as a consequence of its untechnical simplicity, some matters are omitted to which attention might have been advantageously directed. Even the section entitled "Origin of surface forms" fails of being as penetrating in its exposition as it might easily have been made, in spite of Gilbert's great interest in physiography as compared with structural and historical geology. For example, the district treated being constituted of gently warped and moderately faulted Mesozoic strata which have been degraded to a surface of low relief, monotonously plain over large areas, it naturally exhibits among occasional residual inequalities many low scarps which survive here and there along fault lines, wherever strata of unlike resistance are juxtaposed. Such scarps are manifestly not the direct result of faulting, but the result of unequal erosion long after faulting; hence they repeatedly illustrate the important physiographic principles that their height gives no indication of the amount of displacement on the fault plane, and that their aspect gives no indication of the side of downthrow; and inasmuch as the distinction between these purely erosional fault scarps and true fault scarps of displacement has been frequently overlooked, it would seem as if attention might have properly been drawn to this lesson of the Pueblo quadrangle as well as to its other lessons, for it is not to be doubted a moment that these principles were familiar to Gilbert. Hence their omission suggests that the simplicity of a folio text and the avoidance of technical terms may be carried so far as to be disadvantageous to the general reader, and especially so in such an instance as this, in which the features shown on the map might be very properly used to make clear the meaning and value of the technical terms that are allotted to them.

On the other hand, a general statement touching the effects of degradation brings out a physiographic principle which, in spite of its simplicity, is often overlooked. Here it is first pointed out that the existing surface forms, which are 1,000 feet or more below the initial surface of the region, "are only one phase of a changing scene"; and it is then added that, as the streams have to carry away the waste of their drainage areas, they can not cut down their channels indefinitely in advance of the degradation of the interstream areas, but must always preserve a fall sufficient to give their current competence to do their work as transporting agents. "Thus the slopes of all the stream beds are automatically adjusted in a harmonious system, so that the wasting of the whole district is nearly uniform and its entire surface is gradually reduced"; for such is the manner in which degradation acts when a very advanced stage of a cycle of erosion is reached; moreover, its action in this manner is particularly significant in a region that lies 1,000 miles or more from the mouth of its main river in the sea, for at such a distance inland an enormous volume of rock waste may be removed from the surface by fairly uniform degradation after it has been so far degraded as to merit the name of peneplain and before it merits the name, plain of erosion.

SUBSEQUENT VALLEYS

The treatment of certain streams and valleys in the Pueblo quadrangle calls for comment, as it brings up again the question of subsequent valleys to which reference has already been made in the analysis of the third chapter of the Henry Mountains report on land sculpture. Here, as in the case of the fault scarps above mentioned, it is difficult to understand why Gilbert did not use certain facts of the map to their full value as typical examples of a class of forms for which the text might provide a name as well as an explanation. The Front Range of the Rocky Mountains, composed of crystalline rocks and here known as the Wet Mountains, is included in the southwestern part of the quadrangle; and along the mountain base is a valley, excavated in the inclined strata of the weak Fontaine formation, inclosed on the outer side by a monoclinal ridge of the resistant Dakota sandstone and drained by short longitudinal branches of outflowing consequent streams. Several other smaller consequent streams, which formerly

flowed directly from the mountains across the weak beds of the longitudinal valley and through notches in the inclosing sandstone ridge, have been diverted to one or another of the larger outflowing consequents by the normal headward growth of their deeper-lying longitudinal branches. But the text avoids the use of the technical terms by which the several kinds of streams and valleys may be concisely named, and briefly announces that the outflowing streams "enlarged their valleys in the yielding beds of the Fontaine formation and finally drew off the headwaters of the former streams," the lower courses of which are now marked by wind-gap notches in the Dakota ridge and by crystalline gravels from the mountains farther out on the plains.

This brief statement is followed by another, which makes assertion instead of giving explanation of the general principles here involved, and which is therefore just about as impenetrable to the general reader as if it were an explanation couched in technical terms. It is as follows:

> The diversion of these [headwater] streams because they were too weak to keep pace with their neighbors in carving channels through the resistant strata is part of a general process of rearrangement by which the minor elements of drainage are turned away from resistant rocks. The arrangement of small waterways is continually adjusted to the arrangement of resistant rocks.

Not one nongeological reader in a thousand will, on reading this insufficiently explanatory passage, really apprehend the spontaneous origin of longitudinal streams by headward erosion along belts of weak strata. Even the three figures in the folio text which illustrate the progressive revelation and breaching of a resistant anticlinal formation in the northern part of the quadrangle do not suffice to make clear the development of longitudinal streams within the anticline, although several examples of such streams occur there. A fuller and more explicit explanation of the processes here involved would seem to be quite as appropriate, if they are to be explained at all, as the explicit explanation of so simple a matter as a flood plain, above quoted from the report on underground waters; and a generic name for the kind of valley that is so well represented in the southwestern corner of the Pueblo quadrangle would seem to be physiographically as desirable as a formational name is geologically desirable for the strata which in the Pueblo quadrangle outcrop only in that valley, but which are nevertheless very properly given a special designation and a special color.

There was, of course, an earlier period in the evolution of physiography when neither Gilbert nor anyone else in America recognized the existence, much less the origin, of the kind of streams and valleys here considered. In the article on the "Plateau province as a field for geological study," Gilbert gave no hint that Powell's classification of streams as consequent, antecedent, and superimposed was not as complete as Powell himself thought it to be; but there came soon afterwards a second period in physiographic progress when the streams and valleys that follow along the weak formations in and near the Henry Mountains were understood to belong to a different class from the other three, and this understanding appears to have been in mind during the study of the Pueblo quadrangle; yet curiously enough, as far as Gilbert was concerned, a third period of progress was not reached in which the additional class was thought to be important enough to have a name similar in rank to that of the three names proposed by Powell.

The reason for this failure is difficult to discover; and to those who, on the one hand, regard Gilbert as a master of physiographic investigation and who, on the other hand, find that their own investigations are aided by giving appropriate technical names to peculiar classes of streams and valleys, the fact that he did not feel the need of giving such names to other than the three Powellian classes of streams is somewhat embarrassing. His failure to introduce or to use a special name for streams that have grown spontaneously by headward erosion along weak belts of inclined strata can hardly have been due to a disinclination to coin a new term, for he took that liberty when he felt it was advisable to do so; witness the introduction of "laccolite" in the Henry Mountains report; witness also an earlier statement in the account of monoclinal flexures by which the plateau province is traversed: "It is necessary to explain one or two terms, which have had to be coined in order to describe the new group

FIG. 11.—GILBERT IN COLORADO, 1894

of facts."[5] Much later he invented the convenient term, "rock-feet," to serve as a unit for the measurement of a local excess or defect of gravity,[6] and at about the same time he coined the word, "isobase," to designate a particular kind of contour line,[7] and proposed to use the word, "discreet," with a meaning to which he had long wished to give a name.[8] A few years later he took issue with a geologist who deprecated the introduction of new terms into scientific usage; [9] and in a presidential address of 1909 he employed the novel term, "malloseismic," without apology but with explanation. That he recognized the helpfulness of technical terms was moreover shown by the care he took in the proper use of those which he adopted; on one occasion he wrote critically to a correspondent regarding the proper use of the words drumlin and drumloid, recommending that "the name drumlin should be applied to the species, including all varieties, and that drumloid will do best service as an adjective to be applied to things that resemble drumlins in any one of many ways. . . . The restriction of the word drumlin to a variety, and the assignment of drumloid to another variety, not only spoils a good adjective but breaks up one of the best defined species in the morphology and taxonomy of the drift."

More significant still is the emphasis that Gilbert placed, in his tribute to Powell at a memorial meeting held in Washington in 1904, on the importance of "classification" as a part of his chief's work in science, along with "observation, explanation, and application to welfare," and especially on the value of the ideas that are embodied in Powell's three river terms. They "fell on fertile ground, and have had a marvellous development. . . . Geologists and geographers now recognize that each hill, hollow, and plain of the earth's surface originated by some process of change and is therefore susceptible of explanation and interpretation. Whereas geological history was formerly read in the rocks alone, it is now read not only in the rocks but in the forms of the land and in the arrangement of the streams." It is, however, not simply Powell's ideas that have prevailed, but also the terms in which the ideas were embodied; and it would therefore seem that if, after the three kinds of streams and valleys then known had been named, a fourth kind was found, as unlike each of the three as they are unlike one another, it also should have been given a name; and it is all the more regrettable that no fourth name was proposed when one recalls that the qualifying adjective, "subsequent," which goes so well with consequent, had been used by Jukes as long before as in 1862 to indicate that, as already told, the retrogressive erosion of longitudinal streams along belts of weak structure takes place as a sequence to the downward erosion of the transverse rivers which such streams join. It may be fairly urged that streams of this class are as different from consequent, antecedent, and superimposed streams as laccoliths are unlike dikes, necks, and sills. Why Gilbert left such streams without a name is a puzzle. It surely can not be that the absence of a technical name for the fourth class of streams is due, not to its noninvention, but to its having been held back by the editorial net through the meshes of which the folio texts are very properly strained, for the texts of various folios include, and with entire propriety, such terms as syncline, paleozoic, unconformity, and metamorphism; freibergite, microperthite, chalcopyrite, and pyrargyrite; Brachyphyllum, Widdringtonites, Ctenopteris, and Onychiopsis; to say nothing of granodiorite, metadiabase, augite-grano-cecilose, and biotite-hornblende-grano-bandose; all of which illustrate the keen pursuit by which various kinds of facts are traced, captured, classed, and labeled. In the presence of a multitude of admitted terms represented by the few just cited, it does not seem that a technical name for a fourth kind of streams and for the valleys which such streams drain would be a heavy additional burden for the folios to bear. Indeed, had Gilbert sanctioned such a name, it would surely have passed through the meshes of the editorial net and it might now have gained as acceptable a place as syncline, metamorphism, and the rest; and precisely as those terms compactly replace long paraphrases, so the needed stream name might have replaced in certain Appalachian folios such paraphrases as "longitudinal courses on the long narrow outcrops of the easily eroded limestone," and "lesser valleys along the outcrops of the softer rocks."

[5] Amer. Journ. Sci., xii, 1878, 94.
[6] Bull. Phil. Soc. Wash., xiii, 1895, 69.
[7] U. S. Geol. Survey, 18th Ann. Rept, 1898, 604.
[8] Science, vii, 1898, 94.
[9] Science, xxi, 1905, 28–29.

Apparently the real reason for the absence of a generic name for the longitudinal streams of the Pueblo quadrangle must be that, here as well as elsewhere, Gilbert had had little experience with such streams. This point has already been adverted to on earlier pages in connection with the open monoclinal valleys developed on weak strata in the great flexures near the Zuñi uplift, where the drainage was inconspicuous because the climate was arid; with the short and dry monoclinal valleys behind the piedmont revet crags of the Henry Mountains; and with the much more striking example of a self-generated longitudinal stream, Hoxie Creek, which follows the weak beds of the Water-pocket flexure between the Henry Mountains and the high plateaus. True, self-developed monoclinal streams and valleys are numerous, indeed predominant in the Appalachians, where they are often scores of miles in length; but although Gilbert was for a time in charge of the Appalachian division of the national survey, his eastern field work was almost wholly limited to the Great Lakes region of nearly horizontal strata. Thus it would seem as if it was because longitudinal streams following long and narrow outcrops of easily eroded strata had not as a class attracted his attention that he did not give them a technical name; and it is even possible that he did not clearly recognize the few streams of this kind which he had seen as belonging in a class apart from other streams. This conclusion is supported by a brief statement in a review that he wrote in 1883 of McGee's study of streams in Iowa,[10] in the course of which he said:

> It is now many years since Powell first proposed to class all inconsequent drainage as either antecedent or superimposed, and no later writer has added to the number of categories.

But it is difficult to believe that a man of Gilbert's quality held this opinion in 1895.

[10] Drainage system and loess distribution of eastern Iowa. Science, ii, 1883, 762-763.

CHAPTER XXIII

DISCUSSIONS OF ISOSTASY

THE STRENGTH OF THE EARTH'S CRUST

The behavior of the earth's crust under varying loads and stresses was precisely the kind of problem to interest Gilbert's philosophical mind. Its discussion involved the balanced consideration of many factors, and he seemed to have a special enjoyment in, as well as an unusual capacity for, such balancing. It involved also the determination of many quantitative relations, and he had always shown a strong leaning toward quantitative work. His relation to the problem was first developed through his own geological studies, particularly in connection with Lake Bonneville, as will now be shown; it was afterwards extended into a discussion of the geodetic investigations carried on by the United States Coast and Geodetic Survey, as will appear below.

The Bonneville studies had led him to believe that the evaporation of a large lake, nearly 1,000 feet deep, had been accompanied by a gentle doming of the lake floor; and he was thus inclined to the opinion that the removal of the lake waters was presumably the cause of the doming. His presidential address on the "Inculcation of scientific method by example," before the Society of American Naturalists in 1885, was chiefly devoted to an exposition of the trains of thought by which this conclusion was reached. The same problem was discussed in a paper, essentially an abstract of a chapter in the Bonneville monograph not then published, on the "Strength of the earth's crust," his first communication to the newly organized Geological Society of America at its earliest winter meeting in December, 1889.[1] His direct entrance into the subject was characteristic:

> Conceive a large tank of parafine with a level surface. If a hole be dug in this and the material piled in a heap at one side, the permanence of hole or heap will depend on its magnitude. Beyond a certain limit, further excavation and heaping will be completely compensated by the flow of the material. Substitute for parafine the material of the earth's crust, and the same result will follow, but the limiting size of the hole or heap will be different, because the strength of the material is not the same. Assuming the earth to be homogeneous, the greatest possible prominence or depression is a measure of the strength of the material.

Attention is then directed to the relation of the uplifted Wasatch Range and the depressed Bonneville Basin; the range is unloaded by erosion and the basin is loaded by deposition; and some geologists might regard the progressive displacement of the two areas as a direct consequence of the continual transfer of load from one to the other. But if so, the depression of the basin ought to have been accelerated when an arm of Lake Bonneville, 500 or 600 feet deep, was rather quickly laid upon the piedmont part of the depressed area, and conversely ought to have been retarded when the lake arm was as quickly withdrawn. As a matter of fact, the depression of the basin with respect to the mountain range "continued alike during the presence of the water and after its removal." Hence the transfer of load should not be taken as the primary cause of the displacement. On the other hand, if the doming of the central area of the Bonneville Basin floor be due to the removal of the lake waters by evaporation, the effect is not so great as it should be if an isostatic relation had been preserved:

> A stress residuum was left to be taken up by rigidity, and the measure of this residuum is equivalent to the weight of from 400 to 600 cubic miles of rock.

This leads to the further quantitative conclusion:

> The measure of the strength of the crust is a prominence or a concavity about 600 cubic miles in volume. If this be strictly true, then there should be no single valley due purely to the local addition or subtraction of material, having a greater volume than 600 cubic miles.

[1] Bull. Geol. Soc. Amer., i, 1890, 23–25.

Various examples of volcanic mountains and eroded valleys are next cited, the volume of which is much less than this limit, and which are therefore regarded as being sustained by the rigidity of the crust. A general conclusion that is held to be worthy of consideration is as follows:

Mountains, mountain ranges and valleys of magnitude equal to mountains, exist generally in virtue of the rigidity of the earth's crust. Continental plateaus and oceanic basins exist in virtue of isostatic equilibrium in a crust heterogenous as to density.

It would be interesting to know what opinion Gilbert would have had as to the stability of the huge volcanic cones that rise from the bottom of the Pacific, with a total weight greater than the limit he regarded as supportable by crustal rigidity, even after allowing for the weight of the water replaced by the submerged part of the cones.

A chapter in the Bonneville monograph continued the discussion just outlined. It introduced a curious analogy in comparing "the earth when loaded by the water of Lake Bonneville with a bowl of jelly upon which a coin has been laid"; thus likening the apparently solid earth to the pliable jelly and the mobile lake waters to the rigid coin; nevertheless the analogy served its purpose remarkably well by very reason of its seeming contradictions; it was precisely the pliability of the earth's crust under the definite weight of the coinlike lake that was to be emphasized. After a more detailed statement on the same lines as those of the paper on the "Strength of the earth's crust," it is thought probable that, all things considered, "the Wasatch range and the parallel ranges lying west of it are not sustained at their existing heights above the adjacent plains or valleys by reason of the inferior specific density of their masses and of the underlying portions of the crust, but chiefly and perhaps entirely in virtue of the rigidity or strength of the crust." It is further said that "a mountain of the first class is the greatest load that can be held up by the earth"; but it is intimated at the end of the chapter that all of its conclusions are only tentative; even the doming of the Bonneville Basin floor may be due to the accidental coincidence of a compelling force of upheaval with the time and place of the vanishing lake waters.

It thus appears that while Gilbert's work in the Cordilleran region led him to regard the earth's crust as possessing a great measure of rigidity, he believed also that the inequalities of pressure due to continental masses would be greater than the rigidity of the crust could bear, and that the continental parts of the crust must therefore owe their height to being less dense than the suboceanic parts of the crust, so that, at a certain depth, both the continental and the suboceanic parts would exert a uniform pressure on the next underlying earth shell; in other words, that the density of the earth's crust varies inversely with the height of its surface above the shell of uniform pressure; or in still other words, that the crust is in equilibrium: such being the essence of the doctrine of isostasy.

Brief allusion to this doctrine is made in Gilbert's presidential address on "Continental problems" before the Geological Society of America at Ottawa in the winter of 1892–93: "The weight of opinion and, in my judgment, the weight of evidence lie with the doctrine of isostasy," so far as the larger reliefs of the earth's surface are concerned. It is here pointed out that if a spheroid be imagined, the oceanic parts of which are coincident with the average 2-mile depth of the oceans, the rock pressure on its subcontinental parts would exceed the water pressure on its suboceanic parts by about 12,000 pounds to the square inch and that such an excess of pressure urging the continent down and the ocean floors up could not be withstood. It thus appears again that in Gilbert's view the continental masses and their larger features are in isostatic equilibrium, but not individual mountains or mountain ranges of the second order.

THE GEODETIC TREATMENT OF ISOSTASY

Before taking up Gilbert's later papers on certain geodetic aspects of the isostatic problem, it is desirable to outline briefly the manner in which the geological doctrine of isostasy came to be involved in geodetic calculations. If the earth were a smooth, nonrotating sphere, composed of concentric shells, each of uniform composition and temperature and all systematically increasing in density from crust to center, gravity would be the same at all points of the surface; and each shell receiving a uniform pressure from the shell exterior to it, the whole earth would

be in equilibrium. If the earth were a smooth rotating spheroid of similar constitution, gravity would have its maximum value to the two poles and would decrease slowly and systematically to a minimum around the Equator, and perfect equilibrium would still prevail. But the actual earth is a rotating spheroid, the surface of which is not level; it rises in continents and mountain ranges, and it sinks in ocean basins and deeps; and although the density and temperature of the interior "shells" probably increase rather systematically toward the center, it is not known that the composition, density, and temperature in each concentric shell are uniform or that the pressure exerted by any one shell on the next interior shell is everywhere the same. The outermost shell in particular, having an irregular exterior surface and being known to vary somewhat in composition and in vertical temperature gradient as well as in surface temperature, departs from the conditions just assumed on an ideal spheroid.

Hence in the actual earth various departures from a systematic latitude variation of gravity on the surface and from a gravitative equilibrium in the interior may be expected. If the shell strength be very great, as in a rigid earth, each shell might have large variations of density from part to part, as dependent on variations of composition and of temperature, and the departures from equilibrium might be large. Great differences of level could occur at the surface of such an earth, and gravity would vary not only with latitude and surface altitude but also with variations in the density of the crustal and deeper rocks. On the other hand, if shell strength be small, as in a viscous crust resting in a fluid interior, the materials of the earth would tend to arrange themselves so as to develop a uniform distribution of density in each concentric shell, and all the shells would tend to acquire level surfaces, except that the outermost shell or crust might have an uneven surface if it were made of light rocks in its higher parts and of denser rocks in its lower parts.

Now the geodetic surveys of various nations have had to consider the hypothetical questions above intimated in connection with the accurate determination of an ideal sea-level earth or geoid, above which the actual surface of the lands rises in continents with their plateaus and mountain ranges, and below which the ocean floor sinks in its basins and deeps; for it is the surface of this geoid that is projected in maps and charts, and it is with reference to the geoid surface that heights of lands and depths of oceans are measured. Evidently, if the earth's surface were everywhere level, thus making the geoid actual, the departure of such an earth from a sphere could be determined by measuring the force of gravity at many points of known latitude along various meridians; for, as above noted, gravity on a spheroid should have its maximum at the poles and should thence decrease systematically according to the eccentricity of the spheroid to the Equator. Were it possible to increase the precision of gravity measures on a vessel at sea by the barometric devices already available, the number of such measures that could be made in a single year would furnish a more accurate value of the shape of the earth than has been obtained from all geodetic work thus far accomplished on the continents. And as it is possible, even on the actual earth, to reduce observations of gravity made by means of a pendulum at various positions on the uneven lands to the value that they would have at sea level in the same latitude, "gravity determinations furnish the most powerful, the most accurate, known method of measuring the flattening of the earth."[2]

It is in connection with the reduction of gravity from any given altitude on land to its sea-level value that the consideration of the density of the earth's crust is involved; and thus geodesy is concerned with the question whether the density of the crust varies inversely with its thickness, as is postulated in the theory of isostasy. For example, if gravity could be measured in a balloon at a height of 2 or 4 miles, it would there be less than at sea level by a small fraction, because of the increased distance from the earth's center. Hence in such a case reduction to sea level would involve the addition of a small correction dependent on the height at which the gravity measures were made. Similarly, on a 2-mile plateau surface or on a 4-mile mountain top gravity should again be less because of altitude, but not so much less as before because of the attraction of the plateau or mountain mass. It is therefore necessary, in calculating the correction by which gravity at a high-level station may be reduced to its

[2] J. F. Hayofrd. The importance of gravity observations at sea on the Pacific. Proc. Nat. Acad. Sci., ii, 1916, 394-398; see p. 395.

sea-level value, to make certain hypotheses concerning the density of the plateau or mountain mass, as well as concerning the density of that part of the crustal shell which lies beneath them. Naturally the most acceptable hypothesis as to distribution of densities will be the one under which observed values of gravity reduced to sea level agree most closely with calculated sea-level values.

THREE ESSAYS OF 1895

It was particularly in framing and criticizing various hypotheses as to crustal density that Gilbert's later contacts with the problem of isostasy were made. Three essays on this aspect of the subject were published in 1895. The first [3] was notable in establishing a closer relation between geology and geodesy than had usually been reached. In preparation for this study visits were made, on the way eastward from field work in Colorado in the autumn of 1894, to a number of Coast Survey gravity stations from Denver to Cleveland, with the object of determining the average density of the underlying rocks. The fundamental crystallines were taken to have a density of 2.70; beneath Chicago 2,800 feet of stratified rocks were assigned an average density of 2.52, and beneath Denver 15,800 feet of such rocks, 2.42. After the completion of this study it was stated that corrections for the densities of the local formations may advisedly be applied to gravity determinations under the theory of terrestrial rigidity but not under that of terrestrial isostasy; yet even if such corrections are applied the departures of gravity values at individual stations, after reduction to sea level, from the mean of these reduced values average six times greater than if the calculations are made under the theory of isostasy.

The second essay [4] is a further discussion of the same problem, but with the inclusion of 16 stations east and west of those before considered; and here Gilbert's competence to deal with mathematical concepts is well illustrated. Few geologists will care to give the time or thought necessary to follow his "reduction to the mean plain," and fewer still would of themselves perceive that, under the theory of rigidity, the apparent altitude and the altitude correction as usually applied is too small, because "under that theory the sealevel, or geoid of reference, rises in continental regions above the spheroid of reference," but the error thus introduced is thought not to affect the qualitative results of the inquiry. However, a number of the more important steps taken and the conclusions reached from them may be quoted. First to be noted is the explicit statement of the underlying isostatic principle:

> The ideal result of the correction of pendulum observations [of gravity] is uniformity.

That is, if all corrections, including those for the variations of density in the crust and nucleus of an isostatic earth as well as those for topography, altitude, and latitude could be accurately applied, the resulting values of gravity would be everywhere alike; hence, if the resulting values differ, some corrections, presumably those calculated from certain assumptions as to crustal or nucleal density or from the fundamental assumption of isostasy, must be wrong; and it is precisely in the indications that these differing values give as to the distribution of rock density within the earth that they attain a geological value and interest. Then comes a reenforcement of the conclusions reached in the previous paper:

> The measurements of gravity [by the Coast Survey] appear far more harmonious when the method of reduction postulates isostasy than when it postulates rigidity. Nearly all the local peculiarities of gravity admit of simple and rational explanation on the theory that the continent as a whole is approximately isostatic, and that the interior plateau [between the Alleghenies and the Rocky Mountains] is almost perfectly isostatic. Most of the deviations from the normal arise from an excess of matter and are associated with uplift.

Unhappily, later observations do not wholly support this simple generalization.

It is next explained more fully than in the previous essay that, while gravity measures at high-level stations must, under the theory of rigidity, be corrected for the attraction of the rock mass between the station and sea level, no such correction should be made under the theory of isostasy which is now by reason of its above-noted success to be further pursued;

---

[3] A report on a geological examination of some Coast and Geodetic Survey gravity stations. U. S. Coast Survey rept. for 1894, 1895, pt. 2, 51–55.

[4] Notes on gravity determinations reported by G. R. Putnam. Bull. Phil. Soc. Wash., xiii, 1895, 61–75.

for this theory postulates such a distribution of crustal densities as to make the correction zero. Next the mean value of gravity, reduced to sea level and to latitude 40°, for the 11 stations between the Alleghenies and the Rocky Mountains is adopted as the standard with respect to which the similarly reduced values for other stations to the east and west are to be compared; these 11 stations being selected to furnish the standard because they stand on "a vast plain, 1200 miles in its smallest dimensions," which "has been exempt from orogenic corrugation . . . for at least five of the great periods of geologic chronology. . . . The topographic evidences of earlier corrugation have been practically obliterated. Here, if anywhere on the continent, isostatic equilibrium should be established." The chief results of geological interest are reached when the corrected values of gravity for other stations are compared with this standard gravity.

In the expression of these results a device is introduced for which geologists may well be grateful, in that it changes their phrasing from a geodetic to a geologic form. Geodesists usually say that gravity at a certain station has a positive or a negative anomaly of, for example, 0.078, without delaying to state that corrections for altitude and latitude are of course understood to have been made, and without thinking it necessary to add that the numbers used represent the departures of reduced gravity-acceleration values from a standard value in thousandths of a centimeter per second of time; and particularly without explaining that a positive anomaly means that the reduced value of the locally observed gravity is stronger than standard gravity, or that a negative anomaly means that it is weaker. The geodetic form of statement is excellent for geodesists, but when a geologist hears that gravity at Pikes Peak has a positive anomaly of 0.078, he generally has to think twice—or oftener—before clearly conceiving just what is meant. Gilbert was good enough to rephrase these mystic figures in what he called "rock-feet," or the thickness of a sheet of rock, the attraction of which causes local gravity, corrected for altitude and latitude, to differ from standard gravity; or, in other words, the number of feet by which the earth crust is in excess of isostatic requirements at a station where gravity is too strong, or in defect where it is too weak. It is convenient to remember that the excess or defect of gravity expressed in thousandths of a centimeter will if multiplied by 30 give the excess or defect in rock-feet. The excess or deficiency may mean either that the land surface is too high or too low, or that the rock column below the station is too dense or not dense enough.

Thus stated, it appears that for the 11 stations in the great interior plain the average excess or defect of the crust is only 240 rock-feet. As to other stations, Gilbert says regarding Pikes Peak, a mountain 14,084 feet in altitude, and Gunnison, a deep valley at an altitude of 7,677 feet: "Gravity at the two stations exceeds the isostatic requirement by 2,300 and 2,200 rock-feet," and that is exactly what geologists wish to know when isostasy enters their discussions. As to the two high-level stations just mentioned, he goes on to say: "The evident suggestion is that the whole Rocky Mountain plateau, regarded as a prominence on a broader plateau, is sustained by the rigidity of the lithosphere." Similarly three stations in Yellowstone Park "indicate gravitational excesses of 1,500, 1,800, and 2,300 rock-feet." In a closing summary the Rocky Mountain region from Pikes Peak to Salt Lake City is said to have an excess of 1,345 rock-feet and this is taken to indicate a "greater sustaining power than is ordinarily ascribed to the lithosphere by the advocates of isostasy." Washington and Philadelphia, standing on the "fall line" at the inner border of the Atlantic coastal plain, were similarly found to have an excess of mass of 1,600 and 1,300 rock-feet, respectively; hence "a study of the anomaly at the fall-line promises to throw light on the general question of isostatic adjustment between regions of progressive degradation and deposition."

This instructive discussion was shortly followed by a concise summary of the same conclusions for geological readers under the title of "New light on isostasy":[5]

> The whole [Rocky] mountain mass above the level of its base [where it adjoins the Great Plains] is in excess of the requirement for isostatic adjustment; or, in other words, is sustained by the rigidity of the earth . . . These results tend to show that the earth is able to bear on its surface greater loads than American geologists, myself included, have been disposed to admit. They indicate that unloading and loading through degradation and deposition cannot be the cause of the continued rise of mountain ridges with reference to adjacent valleys, but that, on the contrary, the rising of mountain ridges, or orogenic corrugation, is directly opposed by gravity and is accomplished by independent forces in spite of gravitational resistance.

[5] Journ. Geol., iii, 1895, 331-334.

Gilbert published no further contribution to the problem of isostasy until 1913; but in the meantime he urged the Carnegie Institution of Washington to undertake a deep boring chiefly for the determination of the temperature gradient within the earth. He spent a small part of a grant of $1,000 for a preliminary investigation, and in 1904 asked for a further grant of $65,000 to be spent on a boring in the Lithonia district of Georgia, a district where a large granite batholith as well as the rocks into which it was long ago intruded have been reduced to a peneplain during a prolonged period of geological quiet, and where the absence of recent deformation, of alternations of deposition and erosion, and of ice invasion have presumably permitted the uniform granitic mass to assume a vertical distribution of temperature characteristic of the earth's crust as a whole;[6] but the recommendation came to nothing, as the grant was not made.

[6] Value and feasibility of a determination of subterranean temperature gradient by means of a deep boring. Carnegie Inst. Yearbook no. 3. 1905. 261–267.

CHAPTER XXIV
NIAGARA AND THE GREAT LAKES

A RETURN TO NIAGARA

The annual visits that Gilbert made to western New York and thereabouts during the six years when he was in charge of Appalachian studies and during the following three when he was chief geologist of the survey were interrupted by the three years of summer field work in Colorado; but he found time during the winter of 1894–95 to write in popular form an admirable summary of his work on Niagara,[1] above mentioned, which constitutes a worthy extension of the famous Toronto lecture. The temporary subdivision of the falls into a larger American and a smaller Canadian member, when they stood at Wintergreen flats a little below the present whirlpool, is here beautifully described, and visitors are advised not to overlook this eloquent record of a brief chapter in the story of the gorge. The flats in question are easily reached by stopping off on the Canadian half of the electric railway circuit of the gorge. Some account is also given of the varying depth of water in different parts of the gorge, a topic that is more especially referred to in a later paragraph of the present section. Hall's camera-lucida drawing of the falls in 1827 is introduced in connection with a photograph taken in the nineties from the very point where the drawing was made, this topic also being here further treated in a later paragraph. As to the age of the falls, Gilbert shows his customary caution. In spite of the great advance of knowledge in the preceding 10 years, or perhaps precisely because of that advance, he seems to regard the history of the falls as involving so many variable factors that the total time of their interaction can not be safely calculated:

> No estimate yet made has great value, and the best result obtainable may perhaps be only a rough approximation.

Popular impatience to have an answer to this popular question could not push him beyond a guarded statement.

His fear expressed in 1893 that work in the West might prevent a continuation of work on Niagara was happily not realized. When he completed the assigned task of areal surveying in Colorado in 1895, he acted like a bent spring which, when released from a constrained position, flies back to its attitude of free preference; he resumed his studies in New York in the autumn immediately on his return from a final season on the plains, and, in 1896 and the years following, Niagara and the Great Lakes once more received his first attention. Even more than before the field was then occupied by other workers than himself, but he seemed to have as much interest in the results of their work as in those of his own. He frequently made excursions with one or another of them, and on one occasion when work in the field was interrupted by rain his companion, whose scientific interests were not so limited that a single planet could fill them, was surprised to discover that Gilbert also had found geology too narrow a subject and that his speculations had been extended so far into astronomy that he had become seriously competent in celestial problems. If any local bystander listened in on the conversation in the village railroad station where refuge was taken by the two geologists, he must have been surprised when a theoretical explanation of the moon's face by the taller and older stranger was followed by some improvements on the nebular hypothesis of planetary origin proposed by the shorter and younger stranger.

Gilbert's relations with his fellow workers brought out certain remarkable traits of his character. He had a habit of delaying publication while he was making sure of his conclusions, even though this sometimes led to the anticipation of his results by others; but he seems never to have paid any attention to possible losses of that kind; indeed, he wrote to a correspondent who was actively working in the Great Lakes region:

> Having deliberately determined years ago that I would publish nothing prematurely for the sake of securing priority, I ought not to be perturbed when the world neglects to wait for me.

[1] Niagara Falls and their history. Nat. Geogr. Monogrs., 1895, 203-236.

He undoubtedly had occasion, as his research advanced, to modify earlier conclusions and perhaps sometimes to reverse them; but he was one who never seems to have had difficulty in maintaining an open mind even with regard to his own earlier opinions; his adoption of "simplified spelling" shows that. He wrote characteristically to the same correspondent:

You have my cordial sympathy in your regret that some of your work must be undone, but there is another side to the matter which has some importance from a personal point of view. The ability to recede from a cherished position is not so common but that it is highly appreciated by the scientific public, and the man who corrects his own mistakes instead of waiting for others to do it for him reaps a positive benefit in reputation.

This recalls the incident at the first winter meeting of the Geological Society of America, above narrated. A later letter commented on the unsatisfactory methods of a certain observer, who Gilbert thought was—

injuring himself in an intellectual way by continuing to theorize as to numerous details when he is unable to acquire personal knowledge of the facts. He is certainly unfortunate in not being able to give up a minor factor of a first theory when it is clearly shown that the facts are against him.

Gilbert regarded his own problems in so objective a manner that no such criticism could have been justly directed against him.

THE FIRST EASTWARD DISCHARGE OF THE PROGLACIAL LAKES

One of the most gratifying discoveries ever made in connection with the evolution of the Great Lakes and Niagara is to be credited to Gilbert for the season of 1896. It concerned certain minor effects of the change from a westward to an eastward discharge of the proglacial lake waters, which, as already intimated, took place when the receding ice margin opened a passage along the northern slope of the Appalachian plateau margin in central New York at a lower level than that of the outlet which till then had led westward to the Mississippi. His earlier conception of this change was imperfect. The statement concerning it in the Toronto lecture of 1889 was as follows:

The next change in the history of the lakes was a great one. The ice, which had previously occupied nearly the whole of the Ontario basin, so far withdrew as to allow the accumulated water to flow out by way of the Mohawk valley. The level of discharge was thus suddenly lowered 550 feet, and a large district previously submerged became dry land. Then for the first time Lake Erie and Lake Ontario were separated, and then for the first time the Niagara river carried the surplus water of Lake Erie to Lake Ontario.

This brief statement was truly a great advance over earlier vague ideas; but in describing the lowering of the level of discharge as taking place "suddenly" many detailed happenings that accompanied the lowering were overlooked.

It was these detailed happenings that were discovered in 1896. Gilbert wrote on October 15 of that year to the same worker to whom he addressed the several letters from which extracts are quoted in the preceding section:

In the longitude of Syracuse I find four channels crossing one N–S ridge [a spur of the plateau slope]. They are all in the space of six miles but have a vertical range of about 375 feet. Two of them had [east-falling] cataracts over Helderberg limestones, and one falling 160′ dug a hole which now holds a lake 50′ deep. I find that this lake already has a bundle of theories tied to it.

No explanation accompanied this brief statement, but when the explanation was announced it completely replaced the whole bundle of earlier theories. A week later, after he had led a local geologist over some of the channeled spur, Gilbert wrote again:

I find ——— skeptical about my old channels across this country and am going to take him tomorrow to see a fine one that I have just finished tracing. It starts beautifully in the air across a limestone sill and then alternately canyons through shale and builds deltas in transverse [north-south] valleys of the Finger lake type.

It thus appears that the channels were by no means self-explanatory, even when they were pointed out. The explanation that Gilbert gave them is in essence as follows:[2]

In the first place, the general principle was well understood that when the retreating ice sheet lay upon land that sloped away from the ice margin, the ice-water streams merely reenforced the normal drainage of the surface and ran away with it; but Gilbert hardly touched

[2] Old tracks of Erian waters in western New York. Bull. Geol. Soc. Amer., viii, 1897, 285–286.

this phase of the problem as it lay too far south of the Great Lakes. On the other hand, where the land sloped toward the ice sheet it would form a barrier and obstruct the drainage, with the result of producing proglacial lakes, such as have already been considered in connection with the shore lines on which Gilbert worked so long. The outlets of such lakes would be either over the lowest pass in the hills on their southern border—and such was the case with the proglacial lake of the Maumee valley, the outlet of which ran southwestward across Ohio to the Mississippi system—or along the depression that must have existed between the iceward slope of the land and the landward slope of the ice at one side or the other of the lake. In the second place, it must be understood that the Appalachian plateau in west-central New York is built up of essentially horizontal shales and limestones, and that its north-sloping margin is divided into broad spurs by wide-spaced valleys. When the retreating ice sheet lay against the plateau slope, a narrow lake must have occupied each of the valleys, and the lake outlet would have been either southward over a pass at the valley head to the Susquehanna system or laterally along the ice-margin depression to a lower lake on the west or east.

Let it be supposed that the lakes here to be considered all have lateral discharge. One of them must have been the highest of the series. Whichever way it discharged, all the little lakes to the west of it must have discharged westward to lakes of lower and lower level, and all those to the east of it must have similarly discharged eastward. While the lakes were of small area, their outflowing stream along the ice margin would not have eroded large channels, or "cloughs" as they might have been called, across the plateau spurs; but a time came when, by reason of the ice retreat, all the lakes to the west of the highest point in the ice-margin depression were united in one, and when that one vast water body nowhere had a westward or southward discharge so low as the eastward discharge at that highest point in the depression. Then and thereafter the channels would have been eroded across the plateau spurs by a great outflowing river as it ran to lower and lower east-discharging lakes, and the channels would therefore have gained impressive dimensions. This is of course all highly imaginary, and only when certain minutely specialized elements of so imaginary a scheme are found to be verified by the occurrence of their counterparts in nature can its verity be accepted. Hence it was to those specialized elements that Gilbert gave particular attention. In doing so he showed exceptional keenness of penetration and an altogether unusual capacity of visualizing a moving picture of the past. Some of the specialized elements may be described.

If a channel is cut eastward across a spur from a higher to a lower ice-barred lake, the west end of the channel ought now, as Gilbert phrased it, to begin "in the air," to fall from west to east, and to have a delta where its east end opens into a north-south valley. Furthermore, the channel ought to exhibit features characteristic of channels now occupied by rivers, such as steeper slopes over its concave banks, falls where hard strata are cut through into weak strata, and a plunge pool in the weak strata at the base of the falls. Again, the delta at the eastern or discharging end of a channel in one spur ought to stand close to the level of the west-end intake of the corresponding channel in the next spur to the east. All of these deduced elements of the scheme were repeatedly verified; and in some instances the verifying features were found to be tremendously impressive. One of the channel-cutting streams which crossed a broad plateau spur south of Syracuse appears to have taken a consequent course that locally led it to make a south-turning loop around a drumlin which was superposed on the plateau slopes; the present form of the channel shows that the stream proceeded to cut laterally into and thus to steepen the concave banks on the outside of its several curves, thereby enlarging the loop and at the same time narrowing its neck, until the stream eventually opened a cut-off passage through the narrowed neck, just as normal rivers do when they cut off a knob within a turn of an incised meandering valley. In another channel an especially fine cataract must have been developed: an upper-level channel bed of bare limestone leads to the top of the cataract cliffs, the cliffs descend into the amphitheatral head of a lower-level canyon, the plunge pool below the cliffs is now occupied by a little lake, at the farther end of the canyon a delta flat is built out into an open north-south plateau valley, and at the level of the delta but on the farther side of the valley lies the intake of another channel in the next plateau spur. All this is so clear that one can easily

imagine the rushing river still at work, sweeping along the upper level channel, plunging down where the channel is cut off by the cliffs, whirling and foaming in the pool below, and then dashing away through the low-level canyon to the delta in the long and narrow lake of the next north-south valley.

RELATIONS OF SUCCESSIVE CROSS-SPUR CHANNELS

But this is not all. The ice margin was not fixed; it was slowly retreating northward down the plateau slope. Hence in time a new ice-margin path across a spur might be offered, not only at a lower level than that of the first path adopted by the east-flowing lake outlet, but at a lower level than that of the channel floor which the outlet river was cutting down in the spur while the ice was retreating; for be it understood that when the river first ran across the transverse profile of the spur the channel was so short that it could be deepened rapidly, but the deeper it was cut, the longer it would become, and the slower it would be worn down. Hence rapid channel deepening might at first hold the outlet river for a time on a course that was deepened faster than the path along the retreating ice margin was lowered; but eventually the continued northward lowering of the ice-margin path would enable it to overtake the retarded deepening of the channel; thereupon the outlet river would desert the first-cut channel and begin cutting a second one farther down the spur slope. This process might be repeated several times, so that a single spur would be transversely trenched across by a number of separate channels. Now a definite relation as to altitude should obtain among the channels thus carved; namely, the point on the sloping spur crest where a new channel is adopted next to the ice margin must be at least as low as the west-end intake of the abandoned channel farther up the spur slope. This definite relation is so highly specialized that, if the facts confirm it, the theory from which it is deduced will demand acceptance. The facts do confirm it, and not only once, but over and over again, and Gilbert's explanation of the channels thus becomes irresistible; but in some instances the occurrence of till in a channel indicates that the retreating ice margin advanced again a short distance in its retreat. With the adoption of the explanation the glacial theory as a whole gained yet another group of facts for its support; facts whose existence had been individually and locally known for years, but whose real relations were never understood until Gilbert revealed them.

A LONG CHANNEL FLOOR AS A GREAT HIGHWAY

The cross-spur channels are in general rather small affairs. They are too small to be represented on ordinary State maps, although they appear clearly enough on the inch-to-a-mile topographic maps later issued by the national survey; but at the time of Gilbert's studies he had no such maps of his area and was much hampered by the lack of them. He expressed the need of them in a letter to a New York friend:

> If you have the ear of a State legislator, fill it full of the importance of topographic surveys—we all want those contour sheets.

That was about as near as Gilbert ever came to lobbying. Now that the maps are published, it is easy to see that certain cross-spur channels have a local significance when serving as low-grade roadways through the plateau spurs; and a few of them are advantageously used by railways as natural cuts. One of the most remarkable examples of an east-west channel thus utilized runs for nearly 30 miles, not across the plateau spurs, but through a part of the drumlin belt that occupies the lower land north of the plateau margin between Syracuse and Rochester; Gilbert had discovered this long channel a year earlier than the more strongly marked channels in the plateau spurs, and then wrote to a friend of the stretch from Palmyra past Lyons to Clyde as follows:

> You will find yourself in a valley whose dimensions are not determined in the least by the small stream flowing through it. It is of tolerably uniform cross-section, with steep walls, and it traverses the country with large sweeping curves. It is not, to any considerable extent at least, bounded by rock, but the walls are of drift, chiefly drumlins; and I think you cannot follow it far without being satisfied that the drumlins have been cut off by the agency producing the valley. This agency I understand to be the flow of a great body of water, a body large enough to determine by its momentum that the changes of direction should be in curves of large radius.

This unpublished description is a good example of the richness of Gilbert's records. The channel that he thus recognized is an important pathway. It is followed by a long-established main road, the Erie Canal, now enlarged as the Barge Canal, the four-track New York Central Railway, the double-track West Shore Railway, and an electric railway for more local traffic. It is therefore the route of hundreds of thousands of travelers every year. Its features are well shown on the contoured topographic maps of the district and may be reviewed to great advantage from the rear vestibule of an observation car, such as is commonly attached to through express trains; but probably not one in a hundred thousand of those who thus look at the channel see anything more than a pathway of travel, although it is of exceptional length. The cross-spur channels of the plateau margin are much shorter; they are, geographically, small affairs, but it would be difficult to name any other group of physiographic features of similar size that possess a greater scientific significance; and this significance lies not so much in the record that they preserve of a highly dramatic phase in the evolution of Niagara and the Great Lakes as in the extraordinarily convincing testimony given by the accordant interrelations of their detailed forms for the verity of certain curious conditions during a late phase of the Glacial period. One might be tempted to compare Gilbert to a wizard in view of his capacity to bring all these details into their true relations; but the comparison would be wrong, for the essence of a wizard's performance lies in something that is intentionally concealed, while the essence of Gilbert's presentation is its frank lucidity.

THE NIAGARA ESCARPMENT

Later in the same year, 1896, Gilbert made a characteristically accurate contribution to the long-debated problem of glacial erosion.[3] It has already been noted that he took small part in the old discussion as to the origin of the Great Lake basins by glacial erosion; it may now be told that he took a very definite attitude with respect to the effect of glacial erosion in modifying the preexistent escarpment of the Niagara upland, and further that it was in connection with his observations on this matter that he discovered the sites of four temporary Niagara falls in addition to the cataract that is still in existence. As to the glacial erosion of the upland, his argument was in essence as follows: The north-facing scarp of the upland, trending east and west where the Niagara River crosses it, was presumably as sinuous in preglacial time as non-glaciated scarps of similar structure are to-day; but its outline is now simpler and its simplification is due to ice scouring. Where the ice moving to the southwest advanced against the eastern side of a salient, the irregularities in the resistant limestone which caps the escarpment were exaggerated and small reentrants were worn back into furrows; but where the ice passed along the western side of a salient, the escarpment was planed off to a nearly smooth front. The weaker shales of the lowland on the north were scoured down and fluted in the direction of ice motion; the measure of ice erosion there was estimated to exceed that on the upland limestone in the ratio of 20 to 1.

Regarding the first establishment of Niagara River: Apart from a well-known preglacial and drift-filled reentrant west of the Niagara gorge, and in addition to the great gorge of the actual Niagara which opens at Lewiston, Gilbert detected, as just stated, four postglacial notches in the Niagara escarpment; and he was thus led to believe that when, by reason of a change of outlet, a great proglacial lake was divided into two smaller lakes separated by the Niagara upland, the northern edge of the upland was so nearly level that the higher lake on the southwest spilled over to the lower lake on the northeast at five different points. He wrote of this discovery to a friend on November 24, 1896:

> One of the most interesting points brought out in the last week of field work is the fact that the water drained from the Niagara escarpment at five points having nearly the same level. I think these drains were simultaneous, beginning all at once during the lowering of a lacustrine water plane. Two of the discharges . . . were of moderate volume and quickly ceast; a third . . . was more important, either in volume or in time . . . The other two were at Lockport and at Lewiston.

[3] Glacial sculpture in western New York. Bull. Geol. Soc. Amer., x, 1899, 121-130.

The deep notch cut by the Lockport discharge, which for a time appears to have rivaled Niagara proper, has been chosen as the best point at which the Erie Canal should pass by a series of locks from the higher level of the Niagara upland, which it crosses eastward from Buffalo, to the lower level of the Ontario lowland, which it follows thence to the "long level" of the Rome outlet into the Mohawk Valley. It is manifest that an understanding of the pattern to which the upland escarpment was reduced by glacial erosion, as stated in the preceding paragraph, was essential before the notches in the escarpment could be identified as the postglacial work of the several competing Niagaras, as stated in this paragraph; and it is also manifest that there is a gratifying measure of definiteness and precision, highly characteristic of Gilbert's quantitative work, in the conclusion of both paragraphs; but most remarkable is the skill in interpretation by which the enchainments of all these many and diverse items are brought into a well-ordered sequence.

GLACIAL EROSION IN WESTERN NEW YORK

In association with the sculpture of the Niagara escarpment, an important statement was made concerning the general forms of the land surface in the district of the Finger lakes. It was concluded that "the great body of Devonian shale underlying that district owes more to ice work than to antecedent water work, and that the face of the country is essentially a moutonnée surface, the bosses of which are measured horizontally by miles and vertically by hundreds of feet." Several years later a brief abstract of one of his papers contains the significant statement that the uplands of western New York, south of the drumlin belt which occupies the adjoining part of the Ontario lowland, constitute "a zone of great glacial erosion in which the aspect of the land was revolutionized by ice sculpture."[4] At about the same time a personal letter made reference to the same problem:

> It is undeniable that the erosion of the limestone ledges by the last ice [invasion] was small, but on the other hand the general configuration of the Finger lake troughs is distinctly glacial, and I see no way of accounting for them without invoking a large amount of glacial erosion.

The line of thought here appears to have been concerned much more with the visible forms of the land than with any hypotheses as to the behavior of an ice sheet. Gilbert saw with his outer sight that the Finger lake troughs have remarkably smooth sides, in which the cascading streams of to-day are cutting narrow little trenches, locally known as "glens"; and he saw with his insight that, if the troughs were the work of normal river erosion, the contemporary work of side streams ought to have produced lateral valleys by which the trough sides would be repeatedly interrupted. Thereupon he concluded that the abnormally smooth trough sides testify to the excavation of the troughs by some abnormal agency, during the action of which the erosion of side valleys would be prevented; and he identified the abnormal agency with the ice of the Glacial period, his reason for this probably being that if ice did erode it would erode troughs of such form as the Finger lakes occupy.

He evidently recalled some such train of thought when writing the following passage in a letter to his proglacial-lake correspondent in December, 1910:

> To appreciate how dependent they [the Finger Lake valleys] are on glaciation for depth and contour, we must compare them with the norm in topography. I never knew that the forms of my native New York needed special explanation till I had made a study of normal forms outside the area of glaciation.

In other words, Gilbert, like many others whose field of early observation lay in a glaciated area, assumed without special inquiry that its forms were of normal, subaerial erosion; and not until he had studied the forms of never-glaciated areas did he come to see how peculiar the forms of his home district are, and how large a share of their erosion must be ascribed to ice instead of to water and weather. His experience thus in some respects duplicates that of many European geologists who, in trying to resolve the problem of glacial erosion in mountainous districts, were retarded in their approach to its true solution by the unconscious acceptance of the forms of formerly glaciated mountains, such as the Alps, the Scandinavian mountains, and the highlands of Scotland, as of normal origin, and by their inattention to the forms of never-

[4] Physiographic belts in Western New York. Science, xvii, 1903, 221.

glaciated mountains. But Gilbert's experience differs significantly from that of some of the European geologists here alluded to in that, after extending his field of observation from a formerly glaciated to a never-glaciated area, he was open minded and clear minded enough to revise his early opinions, and to give glacial erosion its due measure. As he so truly observed, it is by comparing and contrasting the two types of form that the essential characteristics and the origin of each can be best learned.

MODERN VARIATIONS IN THE GREAT LAKES

A remarkable sequel to Gilbert's studies on the shore lines of the extinct proglacial lakes remains to be described. In the survey year 1896–97 he was assigned an appropriate problem: "Variations in the relative height of the land in the region of the Great Lakes and the associated history of Niagara River," the expectation being that the future changes in the volume of Niagara might have important economic bearings. His results were briefly announced at the summer meeting of the American Association at Detroit in August, 1897, and were published in two forms; a popular statement in a magazine [5] that permitted his use of a partially simplified spelling, and a more formal statement in an annual report of the Survey.[6] The latter opens with the resurrection of an article by G. R. Stuntz, a land surveyor, "On some recent geological changes in north-eastern Wisconsin,"[7] which is regarded by Gilbert as broaching "for the first time the idea of differential elevation in the Great Lakes region. . . . It contains the only observations that have ever been cited as showing recent changes of that character." Another article of similar tenor which came to Gilbert's attention between the publication of the popular and of the more formal statement of his work, was mentioned as "an important contribution to the subject"; it was by E. L. Moseley, of Ohio, who, while Gilbert's work was in progress, independently reached the conclusion that the western part of Lake Erie had recently risen.[8] One may see in these announcements that when it came to giving credit to his predecessors Gilbert was scrupulously careful to cite their publications, yet he still remained true to his principle of not claiming credit for himself.

The method followed in seeking to determine whether earth movements in recent times have affected the outlines of the Great Lakes was essentially simple, although its application involved a large amount of careful observation and study. First, the present attitude of the formerly level, but now slanting, surfaces recorded by the abandoned shore lines of the proglacial Great Lakes was shown by contour lines drawn through points on the abandoned shore lines having the same altitude. For these contours Gilbert proposed the name "isobases," a word so new that it is not to be found in the great Oxford Dictionary. The direction of greatest slant of the tilted surfaces was then immediately shown by lines crossing the isobases at right angles. Next, for each of four existing lakes, pairs of shore stations were selected which lie far apart and nearly on a line of greatest tilting, and at which gauges of lake levels have been maintained for the longest periods; and the gauges and records at these paired stations were personally examined to estimate their trustworthiness. The distances between the stations varied from 78 to 186 miles; the period of their records varied from 20 to 37 years. On calculating the rate of change of level for each pair of stations in feet per 100 miles per century, it was found that all four pairs showed the northeastern station to be rising with respect to the southwestern at the rate of 0.4 foot per 100 miles per century; or, more exactly, 0.37, 0.46, 0.39, 0.43 foot for each pair. The weighted mean of the four is 0.42 foot.

The conclusion was thereupon announced that "the harmony of the measurements and their agreement with prediction from geologic data make so strong a case for the hypothesis of tilting that it should be accepted as a fact, despite the doubts concerning the stability of the gauges," but a candid recognition of elements of uncertainty is immediately added: "The deduced rate of change . . . depends on assumptions which are convenient rather than prob-

[5] Modification of the Great Lakes by earth movement. Nat. Geogr. Mag., viii, 1897, 233–247. The titles of figs. 4 and 5 are interchanged. Words simplified, such as ceast, encroacht, gage, increast, markt, reacht, sketcht; but flowed, formed, planned, opposite, determine, and relative are not simplified.

[6] Recent earth movement in the Great Lakes region. 18th Ann. Rept. U. S. Geol. Survey, 1898, pt. 2, 597–647.

[7] Proc. Amer. Assoc. Adv. Sci., xviii, 1870, 206–207.

[8] Lake Erie enlarging. . . . Lakeside Magazine, Lakeside, Ohio, i, 1898, 14–17.

able." These are: (1) That the whole region moves together without internal warping and (2) that the direction of its present tilting is identical with the direction of the total change since the epoch of the Nipissing outlet of the upper lakes. What we know of the general character of earth movements gives no warrant for such assumptions of uniformity, but no better assumptions as to this region are now available. On under the law of probabilities the close agreement of four measurements, three of which are wholly independent, gives a good status to their mean, but there are other considerations tending to weaken this status. The probable errors of the individual measurements are rather high, ranging from 14 to 50 per cent, and this suggests the possibility that the closeness of their correspondence may be accidental. Several causes of small errors are then enumerated.

For all these reasons I am disposed to ascribe only a low order of precision to the deduced rate of change, and to regard it as indicating the order of magnitude rather than the actual magnitude of the differential movement.

On reading these passages and seeing how clearly the admirable quality of scientific candor is expressed in them, one must wonder that it is ever absent from geological essays; and yet how few geologists there are who, on reviewing their own writings, will not find that various half-conceived uncertainties have occasionally been left in the background instead of being frankly brought into the foreground of their conclusions.

THE FUTURE DISCHARGE OF THE GREAT LAKES AT CHICAGO

No element of Gilbert's study is neater than one that is graphically presented on an outline map of tho Great Lakes, in which lines parallel to the general course of the isobases are drawn through each of the lako outlets; for on the northeast of these lines the continuation of the tilting should cause the lake shores to emerge; whilo on the southwest it should cause them to be submerged. Certain facts recorded in the paper regarding the drowning of the mouths of certain creeks where submergence is inferred give support to this interpretation; but its full confirmation rests with the future. Then follows, "under the stated assumption as to rato of tilting," a specific prediction of expectable future changes which has gained wide currency:

Eventually, unless a dam is erected to prevent, Lake Michigan will again overflow to the Illinois River, its discharge occupying the channel carved by the outlet of a Pleistocene glacial lake. The summit in that channel is now 8 feet above the mean level of the lake, and the time before it will be overtopped . . . may be computed. Evidently the first water to overflow will be that of some high stage of the lake, and the discharge may at first be intermittent. Such high-water discharge will occur in 500 or 600 years. For the mean lake stage such discharge will begin in about 1,000 years, and after 1,500 years there will be no interruption. In about 2,000 years the Illinois River and the Niagara will carry equal portions of the surplus water of the Great Lakes. In 2,500 years the discharge of Niagara will be intermittent, failing at low stages of the lake, and in 3,500 years there will be no Niagara. The basin of Lake Erie will then be tributary to Lake Huron, the current being reversed in the Detroit and St. Clair channels.

This statoment has been often quoted, and deservedly so; but the careful statement in the original essay that the prediction is based on "the stated assumption as to the rate of tilting" has been very commonly overlooked. The prediction is plainly the most interesting part of tho wholo study; but the most valuable part, much the most valuable part, is the specific statoment of the assumption on which the prediction is made.

It should be here noted that in the popular statement of recent changes in the lake-shore lines, above cited, as published in 1897, Gilbert ascribed the prediction of the possible future outflow of the Great Lakes at Chicago and the resulting extinction of Niagara to another observer in 1894, although he had himself very clearly indicated the possibility of this geographical revolution in an article of his own in 1888, referred to in an earlier section. His statement then was:

Had the oscillation [of the land] received no check, our hydrography and avenues of commerce might have been very different; a further tilting of the land to the extent of three inches in each mile would send a great river from Chicago to the Mississippi, reverse the current in the Detroit, stop Niagara Falls, and rob the upper St. Lawrence of seven-eighths of its water.

The other observer's statement, in 1894, was:

> If the late rate of terrestrial deformation shall continue into the future . . . the drainage of the upper lakes will be diverted from the Niagara into the Mississippi in perhaps 5,000 or 6,000 years hence.[9]

The difference between the two statements is hardly more than a difference of auxiliary verbs. One says, in effect, a further tilting *would* send a great river out from the lakes to the Mississippi at Chicago; the other says a continued deformation *will* divert the lake discharge to the Mississippi. Whether the crediting of the prediction to the other observer was prompted by an indifferent forgetfulness or by an exaggerated generosity does not appear; but in either case it is historically misleading to a reader who happens on Gilbert's later article without knowing of his earlier one.

THE PROFILE OF THE BED OF THE NIAGARA GORGE

In the Toronto lecture on Niagara no special attention was paid to the influence of variations in the volume of Niagara River on the width and depth of its gorge; but this subject, with others, was taken up in the summer of 1896, following the three summers in Colorado, and quickly carried to a successful conclusion. The width of the gorge is seen to be reduced to its least measure for a certain distance below and above the whirlpool, and to increase again near its upper end. The depth of the mouth of the gorge by Lewiston had been sounded as 96 feet, and in the pool near the great cataract as 189 feet. In the intermediate stretch the strength of the current was such as to make sounding impracticable. Gilbert therefore determined the missing data by an ingenious method, suggested by a fellow worker, in which the volume of discharge being known the depth was calculated in terms of the surface velocity from the formula:

$$\text{Central depth} = \frac{\text{Volume}}{1\tfrac{5}{8}\ \text{central velocity} \times \text{width.}}$$

The central velocity was estimated by timing the passage of patches of foam over measured distances; the river width was taken from accurate surveys; the volume was taken from the records of the Lake Survey.[10] The formula being supplied with these quantities, the depth of the water below Wintergreen Flats was found to be 70 feet; opposite Wintergreen Flat, 35 feet; at the whirlpool outlet, 50 feet; in the whirlpool, 150 feet; and in the rapids above the whirlpool, 35 feet. In the interpretation of these values the great depth at the whirlpool was ascribed to the existence there of the drift-filled St. Davids Channel; the shallow stretches near Wintergreen Flats and above the whirlpool were correlated with epochs when the short-cut discharge of the upper lakes, first by the Trent and later by the Nipissing outlet, left the Niagara River so small and weak that it could not excavate a deep gorge; the somewhat deeper stretch between Wintergreen Flats and the whirlpool was tentatively ascribed to a temporary restoration of full river volume by a partial elevation of the northern land which raised the Trent outlet higher than Detroit, and therefore turned the discharge of the upper lakes through Lake Erie while the lingering ice sheet still obstructed the Nipissing outlet, but this interpretation has since been questioned; the deep pool between the whirlpool rapids and the present falls was the work of the finally restored full volume of the river, when the rise of land in the northeast finally turned the discharge of the upper lakes through the Detroit Channel, where it had run before while the Trent and the Nipissing outlets were obstructed by ice.

---

[9] J. W. Spencer. The geological survey of the Great Lakes. Proc. Amer. Assoc. Adv. Sci., xliii, 1894, 237–243; see p. 243.
[10] Profile of the Niagara in its gorge. (Abstract) Amer. Geol., xviii, 1896, 232.

CHAPTER XXV

GLACIERS AND GLACIATION OF ALASKA

THE HARRIMAN EXPEDITION TO ALASKA: 1899

When a great excursion to Alaska was planned for the summer of 1899, transportation being munificently provided in a special train from New York to Seattle and in a fine ocean-going vessel chartered for the voyage from Seattle to Alaska and return by the host of the party, Mr. Edward H. Harriman, it was natural if not inevitable that Gilbert should be included among its 25 scientific members, inasmuch as they were chosen as leading representatives in various lines of research. The party, known as the Harriman Alaska expedition, left Seattle in the steamship *George W. Elder* on May 31, reached Bering Strait before turning back, and had traveled 9,000 miles before arriving at Seattle again on July 30. The narrative of the journey records that—

> the ship had no business other than to convey the party whithersoever it desired to go. Her route was entrusted to a committee comprising the heads of the various departments of research; so that from day to day and from hour to hour her movements were made to subserve the interests of the scientific work. . . . Another factor which contributed materially to the results was the length of the days and lightness of the nights in northern latitudes, permitting work on shore the greater part of the night. . . . Naphtha launches were of the utmost service, landing large parties quickly and safely, and conveying men and supplies to remote points out of reach of the ship. . . . Nearly every evening an informal lecture or talk on some subject connected with the work of the Expedition, and illustrated by blackboard sketches, was given in the main cabin.

By great good fortune, the party was "favored with unusually fine weather so that on either the outward or the return voyage practically all parts of the coast from Puget Sound to Unalaska, including the splendid peaks of the St. Elias and Fairweather ranges, and the great mountains of the Alaska peninsula, were clearly seen from the steamer. . . . In all not less than five thousand photographs were secured," in addition to large natural history collections. The expedition was a noteworthy scientific event.

GLACIERS AND GLACIATION

Gilbert's share in the expedition was not limited to his special studies. He was a member of the executive committee, and of the committees on route and plans, geology, geography and geographic names, and lectures; and, as in every party that he joined, he was a prized companion for a walk on deck, a trip to the shore, or an evening in the cabin. The subjects of study assigned to him were "glaciers and glaciation." He treated them in the third of the handsome volumes published by the Smithsonian Institution as the Harriman Alaska Series. As to the first topic, to which John Muir also devoted a short chapter in the first volume of the series, Gilbert gave general accounts of nearly 40 glaciers, many of which reached tidewater; but as his observations were made either from the passing steamer or from short trips to the shore, he early decided that the phase of glacial science which he could best advance was the history of local changes shown by the advance or retreat of glacier ends. Effort was therefore made to visit as many as possible of the glaciers that had been previously described or mapped by earlier observers, with the object of determining fluctuations that had taken place later, and in the case of the numerous glaciers not previously studied, means were taken to define their ends by maps, photographs, and descriptions in such a way that their future fluctuations might be measured by later observers. So numerous are Alaskan glaciers and so little had they been explored, that this expedition discovered and named a good number of new ones. No finer example of a tidal glacier was found than the first one encountered, the Davidson Glacier in the Lynn "canal" or fiord; it is noteworthy that Gilbert's description of this beautiful ice stream recognized the possibility of its submarine extension beneath the low belt of gravel that surrounds its visible termination, and that his longitudinal section of the glacier represents its actual end resting on the fiord bottom and reaching about 2,000 feet beyond its visible end.

A summary of results declares the detected fluctuations of glacier ends to be unsystematic; not even when the retarded fluctuations of long as compared with short glaciers is allowed for did the advances and retreats exhibit an orderly relation. Hence Gilbert was led to speculate as to the possibility of other controls of fluctuation than those dependent on climatic variations as ordinarily conceived, and he therefore suggested that "the combination of a climatic change of a general character with local conditions of varied character may result in local glacier variations which are not only unequal but opposite" (109). The results of such a combination are worked out by a very ingenious discussion: It was shown that if the ocean water in the Gulf of Alaska has such a temperature as is "most favorable for the development of glaciers in the district as a whole, it will be too warm for the highest development of certain glaciers and glacier systems and too cool for others." Hence "whenever such a condition obtains, a change in ocean temperature will cause some glaciers to enlarge and others to contract" (111). The analysis is regarded as serving its purpose "if it has given plausibility to the suggestion that a change in some meterologic factor or factors may result in simultaneous modifications of glaciers which differ not only in amount but in algebraic sign." It is then pointed out that because of the greater variety of local conditions found in Alaska than in Europe or Greenland, "the complexity of interacting conditions may for a long time baffle attempt at its analysis, but when the complexity has been resolved, the resulting theory should have wider application than one founded on simpler phenomena" (112). The two pages devoted to this topic may be instanced as exhibiting Gibert's logical ingenuity in a highly characteristic form.

GLACIAL EROSION OF FIORDS

The second half of the volume on "Pleistocene glaciation" contains the most cogent presentation of the evidence for strong glacial erosion of a mountainous coast that has ever appeared. It is a model of orderly, powerful, and convincing procedure. The preglacial topography of the region is given an explanatory description as follows:

> After the folding and squeezing of the metamorphic rocks, there was a long period of erosion in which broad tracts of the land were worn down nearly to sea level. Then came uplift, producing a [slanting] plateau from 3,000 to 6,000 feet high. . . . The period of subsequent erosion has been long enough for the development of local peneplains [on weak rocks] at a lower level, and in that time the plateau [of harder rocks] has been greatly modified. When the glacial period opened, the larger part of the region was mountainous in the ordinary sense, with crests at various heights and a complicated system of steep-sided ridges, spurs and gorges. There were extensive remnants of the high-lifted peneplain, its plateaus marking the areas of most resistant rocks, and above these plateaus rose summits of the nature of monadnocks. There were remnants of a low peneplain . . . . and these occupied areas of relatively weak rock. There was a system of river valleys or master lines of drainage, narrow where the rocks were most resistant and more open among weak rocks. The bottoms of these valleys were in part below tide level (129, 139).

It is further stated that "at the date of the lower peneplain," the region presumably included all phases of the topographic cycle, "being infantile to adolescent where the rocks are most resistant, adolescent to mature . . . . [where the rocks are of medium resistance] and senile where the rocks are weakest" (130). Before taking up the question of glacial erosion, it was pointed out that the preglacial valleys of river erosion can not have been so deep as the present fiords, for that would have involved a relative elevation of 3,000 feet, and "such a change would carry a very large area above snow line, and would so promote the alimentation of glaciers as to flood the whole district with ice and abolish stream erosion" (136).

The problem of glacial erosion is then considered; and not only the cirques in the mountains that surmounted the plateau of the uplifted peneplain and the high-level lateral valleys above the fiords, but the deep fiord troughs also are ascribed to it. Gilbert writes:

> For this complicated system of troughs I have not been able to suggest an origin that does not involve an immense amount of excavation by ice (144).

Various combinations of river and glacier erosion are examined, but no combination makes it possible to account for the discordant relations of the high lateral valleys and the deep fiord troughs without powerful excavation by glaciers; and it is interesting to note that the discordance of depth between the high-level valleys—or "hanging valleys," as Gilbert proposed they should

be called—and the deep fiord troughs was considered "the most important witness yet discovered to the magnitude of the work accomplished by the Alpine glaciers of the Pleistocene" (118). When this conclusion was reached on the Harriman expedition, it must have given great satisfaction to Gannett, who was one of the party; for it was he who had only a year before published his remarkable paper on Lake Chelan in the State of Washington, in which after first pointing out the striking homology between branch and trunk river channels and branch and trunk glacial troughs, he had so ably used this homology to resolve the vexed question of the amount of erosion that the ancient glaciers of mountainous regions accomplished. The elaboration of his argument by so competent and so impartial an investigator as Gilbert in the presence of the innumerable examples of hanging lateral valleys and deep main troughs in Alaska settled the question beyond the need of further proof.

METHOD OF DISCUSSION OF GLACIAL EROSION

Several comments are suggested by Gilbert's discussion. It should be noted in the first place that it is preceded by a critical physiographic analysis, not of the fiord troughs and their hanging lateral valleys alone but of the region as a whole, and that this analysis resulted in giving a readily intelligible explanatory description of the varied parts of which the whole is composed; in other words, the treatment has the breadth and comprehensiveness that is usually found in Gilbert's essays. More significant still is the fact that the physiographic analysis was substantially the product of Gilbert's own observation in a region of complicated structure and of complex forms during a comparatively short excursion. The analysis may of course be erroneous in some respects and incomplete in others. No one would have recognized the desirability of further work more heartily than Gilbert; nevertheless the results of the analysis are distinctly valuable. It may be argued from this that a preliminary explanatory description of a region can be properly made during a short excursion, provided the describer is properly trained. The apprehensions of those who fear that explanatory physiographic description is useful only after long-continued and elaborate investigation are not warranted; it is as available in reconnaissance work as an explanatory geological description is; both may be helpfully carried as far as the occasion warrants, always provided that the worker is competent.

The phrasing of Gilbert's results is also of interest; it shows that this clear-minded student of land forms found value in such descriptive terms as infantile, mature, and senile, borrowed from the organic sciences where they are so naturally employed to designate certain stages in a systematic developmental sequence, and applied in an inorganic science where they may again represent stages in a systematic developmental sequence; and it shows further that he was not in the least embarrassed by the contemporaneous occurrence of these unlike stages of form-development on rocks of different resistance in different parts of a single region, although such association appears to have caused trouble among certain less clear-minded students who have confused stage, as representing phase of change, with age, as representing a measure of time. Gilbert's explicit sanction of these biological terms as representing stage and not age is quoted in another section. This must surely give encouragement to those physiographers who find help in the analogy between the orderly development of land forms and of organic forms.

The reasons that led Gilbert to a belief in the strong erosive power of the ancient Alaskan glaciers also deserve emphasis. These were not based on a study of the physics of ice, or on an investigation of the visible behavior of existing glaciers, or on an estimate of the volume of glacial detritus removed from the region in question; they were based, first, on a competent physiographic analysis of the Alaskan coastal region, as noted above; next on the striking contrast discovered by means of this analysis between the peculiar features of this formerly glaciated region and the forms of various other never-glaciated regions of ordinary or "normal" erosion with which Gilbert had had abundant experience for 30 years previously; and finally on the recognition of the striking agreement between the peculiar features of formerly glaciated Alaska and the features that Alaska ought to have if its former glaciers had strong erosive power where their currents were deep and relatively rapid, coupled with the recognition of much less erosive power where the ice was less deep and more sluggish. In a word, Gilbert's conclu-

sion was reached by a logical combination of induction and deduction in which both of these unlike processes were duly exercised and neither was allowed to obstruct or to dominate the other. His study of Coon Butte, 10 years earlier, employed a similar combination; but there no assured conclusion was reached, partly perhaps because only a single instance was examined. Would that he had taken occasion to analyze his method in the Alaskan problem, where instances were abundant and where an assured conclusion was attained, as fully as he had done in the Coon Butte problem where, by very reason of the excellence of his method of study, the attainment of a conclusion was impossible.

A particular instance of the interplay of induction and deduction in the Alaskan problem may be brought forward; it concerns the inequalities in the fiord-trough floors as discovered by sounding. They are explained by following "Gannett's theorem that the glacier-made valley [or fiord trough] is homologous, not with river-made valley, but with the channel made by the river. The bottom of a river channel is not evenly graded like its flood plain, but it abounds in hollows and hills, and the bottom of a glacier channel has irregularities that are similar but on a larger scale" (148). That the more striking of these irregularities may be interpreted as witnesses to the immaturity of the troughs that they interrupt is intimated in later passages. It is worth adding that certain examples of salient irregularities in glaciated trough floors have often been cited by opponents of glacial erosion as preglacial features which, in spite of their small size, glacial erosion could not remove. Such salient are here more reasonably regarded as the comparatively small residuals which survived the great erosion by which the trough as a whole was excavated, and which would have been more completely removed if the great erosion had been carried somewhat farther.

It is worth recording that Gilbert did not have recourse to down-faulting in explanation of the Alaskan fiords, although that process has been rather frequently called upon in recent years by certain European observers in explanation of Norwegian fiords; and it is also worth recording that the analysis by which the European observers were led to accept down-faulting was—in its published form at least—by no means so thorough or so critical as the analysis by which Gilbert was led to reject it. He believed that the Alaskan fiords follow the courses of normal valleys of less depth, and that some of the valleys had been eroded along the strike of the strata and others across it; he recognized that in the uplift of the older peneplain to its present plateau altitude, it was possibly "interrupted here and there by faults" (129); but he wisely avoids the extreme view that each fiord represents a narrow down-faulted block. This view has had two interpretations by European observers: One that the down-faulting was of so recent a date that the glaciers soon followed it and had comparatively little to do with shaping the troughs; the other that the down-faulting was of earlier date than the erosion of the highland peneplain, its effect being to let down narrow slabs of weaker rocks between larger blocks of harder rocks, so that after the mosaic mass was degraded and uplifted, only the excavation of the weaker slabs by a combination of normal and glacial erosion was needed to produce the fiords. Neither of these interpretations was mentioned in Gilbert's analysis; but his phrase regarding the interruption of the uplifted peneplain only "here and there by faults" suffices to show that neither would have been acceptable.

PHYSIOGRAPHIC ITEMS

On the other hand, Gilbert's statement as to the origin of the stream courses by which the uplifted peneplain was eroded in preglacial time contains a peculiar item:

> The master streams were largely consequent to the seaward slope of the old plain, but were in part directed to lines of strike; and minor streams were adjusted to rock structures (130).

If taken literally, the first clause implies that the peneplain, or "old plain" as it is here called, was so smooth and its slanting uplift was so rapid that the master streams which had drained it during its degradation were as a rule extinguished and replaced by new streams "consequent to the slope." As nothing is directly known about the master stream courses on the peneplain, and as the fiords to-day exhibit a considerable irregularity in their directions, it seems permissible at least to suppose that the region at the time of its most advanced degradation was not a plain but a peneplain, for some of its inequalities were recognized to be so large

FIG. 12.—MOUNT GILBERT, CHUGACH MOUNTAINS, ALASKA

Photograph by National Geographic Society

that they were called "monadnocks;" also that the local slopes on the residual hills of the peneplain were very commonly as steep as or steeper than the general seaward slope given to the peneplain as a whole by its slanting uplift; and finally that the incision of young valleys by the master streams beneath their old valley floors during the uplift of the region, taken with the local residual slopes on the seaward side of their old valleys, would have generally safeguarded these streams from being tipped into new courses "consequent to the slope" given by uplift.

Thus interpreted the present fiord courses should as a rule follow the courses of the peneplain drainage, except that streams which happened to flow with the slope given to the peneplain in its uplift presumably extended their courses by accelerated headward erosion and diverted other streams to their service. In so far as the fiords now follow the strike of the strata, it would also seem that such courses had been developed by the larger rivers during the peneplanation of the region rather than that the rivers were diverted from some other courses "to the lines of strike" during the uplift of the region. It is here once more to be noted that, as already pointed out at the end of the chapter on "Field work in Colorado," no class name was used for rivers that had spontaneously developed their courses along belts of weak rocks; they can not be called "adjusted," for in an adjusted drainage system it is quite as essential that the master rivers should here and there cross belts of resistant rocks as that the remainder of their courses, as well as the whole course of many smaller streams, should lie along belts of weak rocks. Hence it may be repeated that Gilbert does not seem to have been especially interested in this phase of physiographic terminology, great as his interest was in physiographic science.

SUBMARINE GLACIAL EROSION

It is stated above that Gilbert did not make a study of the physics of ice or of the visible behavior of existing glaciers in his attempt to determine whether the great glaciers of the past were efficient eroding agents or not. It may now be added that he did make a simple but extremely important suggestion as to the invisible behavior of both existing and ancient glaciers. He showed that a glacier occupying a fiord trough which extends below sea level must press so heavily on the trough bottom that the sea water can not enter beneath the ice and press it up. A capillary film of water may separate the ice and the rock, but this will "not prevent the bed rock from supporting (through the mediation of the film) the whole weight of the glacier. . . . It thus appears that there is no important difference, as respects its pressure on the rock bed, between a glacier resting on the land and one which is partly bathed by the water of a fiord; and, so far as glacial erosion is conditioned by pressure, the presence of the sea does not diminish the efficiency of the glacier" (215, 216). This is probably the most novel view that Gilbert reached in the course of his Alaskan journey and studies; it is an advanced term in a series of views through which the explanation of fiords has progressed. It was at first thought that, although the ancient glaciers occupied fiords, they did not create them; it was then believed, even by observers who regarded glaciers as effective eroding agents, that they had not eroded fiord troughs below sea level, and that fiords therefore testify to postglacial submergence; it was next seen that glaciers might erode their troughs to a certain depth below sea level, but that the erosion would proceed with increasing slowness until the fiord depth was about six-sevenths of the ice thickness, when it would cease because the ice would then float. Finally Gilbert adduced good reasons for thinking that a glacier would, other factors being equal, erode just as effectively when nearly submerged in a fiord sea arm as when on dry land. It would seem that only one further step remains to be taken: That a fiord glacier would continue to erode, even after its trough had been so much deepened that the ice surface lay below sea level, and that the limit to the glacial erosion of fiord troughs would not be reached until the increasing cross-sectional area of the trough caused such a diminution of the glacial current as to make it an inefficient scourer.

Gilbert's volume closes with an enumeration of the resemblances and differences between "rivers of ice and water." Of resemblances, 26 are listed. Among the differences it is noted on the last page of the volume that—

many features of rivers and river work which arise from inertia in association with swift motion are paralleled by features of glaciers and glacier work which arise from high viscosity in association with slow motion. In each

kind of stream, changes in the direction of flow are caused by irregularities of the channel, and complex series of phenomena arise from the resistance of the current to deflection. These series are strikingly parallel, but the resistance to deflection is occasioned in one case by momentum and in the other by viscosity.

After the return of the expedition, Gilbert spent the months of August and September, 1899, in the States of Washington and Oregon on local problems, and returned via California to the city of Washington on October 10. He gave much time during two years following to the report above outlined. The journey afforded him material for a number of communications to scientific societies: Alaskan glaciers were described to the National Geographic Society on December 1, and to the Washington Academy of Sciences on December 29, 1899. The morphology of southern Alaska was discussed before the Geological Society of Washington on January 27, and the statics of a tidal glacier on March 25; Alaskan scenery was the subject of an address to the American Institute of Mining Engineers on February 20; the relation of glacier flotation and fiords was explained to the Philosophical Society of Washington on March 29, 1900. The preparation of the volume on "Glaciers and glaciation" was not completed until the summer of 1902. It is fitting that, as this fine volume did so much to make known the striking features of Alaskan mountains, one of them which rises from Harriman Fiord to a height of over 10,000 feet should be given Gilbert's name, not merely in memory of his visit there but also in commemoration of his great contributions to American science.

CHAPTER XXVI

A LATER STUDY OF THE BASIN RANGES

THE BASIN RANGES ARE LONG NEGLECTED

There is little published indication that the origin of the basin ranges continued to occupy Gilbert's attention after he left the Wheeler survey. He saw many of the ranges while he was studying irrigation problems under Powell and Bonneville problems under King; but other duties appear to have been so engrossing that the basin-range problem was laid aside. His habit seems to have been to discharge past problems from his mind and to concentrate his attention upon problems in hand. In any case he took no public part in the discussion that the problem aroused during the years just before and just after the organization of the national survey. Very little mention of it is found in his writings for a period of 25 years after he left the Wheeler survey, most of his work during that time being in the East; nevertheless he must have followed the discussion while it was conducted by others, and in particular the additions contributed by Powell and Dutton to his tersely outlined views of 1875 must have been known to him. Nevertheless, such brief mention as he made of the basin ranges in connection with studies of other subjects took no account of these additions; the only novel elements in his own brief statements concerned the recent and low fault scarps in the piedmont alluvium and the much larger signs of faulting seen in the spur-end facets, as will appear from the following extracts, the first of which, however, merely repeat his original understanding of the problem. In connection with the possible occurrence of earthquakes in the district of Salt Lake City, the brief statement was made in 1884 that "a majority of the mountain ranges [of the Basin] have been upraised by the aid of a fracture at one side or the other, and in numerous instances there is evidence that the last increase of height was somewhat recent."[1] Similarly, the chapters prepared by Gilbert on the Salt Lake region in the guidebook for the western excursion that followed the International Geological Congress at Washington in 1891 concisely announced:

> The mountain ridges of the Great Basin are due directly to uplift (p. 263); . . . the mountains of the Great Basin are, in large part, carved from orogenic blocks uplifted along fault planes. The displacements, which were probably initiated in Mesozoic time, were continued during various Cenozoic epochs, and are now in progress. The steeper faces of most of the mountain ranges are rugged escarpments primarily due to faulting and at their bases are frequently to be found smaller [alluvial] escarpments of so recent date that the traces of subsequent erosion are scarcely perceptible (p. 376).
>
> The [Wasatch] mountains towering above the Utah valley are of wonderful boldness and beauty, and their precipitous faces are such as result from no orogenic process save that of faulting (p. 396)

A somewhat more explicit statement was made elsewhere:

> A range consisting of a faulted block generally has a bold front on the side of the fault, and is less abrupt on the opposite slope. On the side of the bold front the line separating the rock of the mountain from the alluvium of the valley [intermont trough] is simple and direct, while on the opposite side it is tortuous.[2]

At the time of the International Geological Congress and excursion, Gilbert's attention seems to have been given less to the larger features of the range margins than to the little fault scarps in the alluvium near the base of the ranges. This was perhaps natural because the little scarps had not been understood, even if they had been seen, by Gilbert during his service on the Wheeler survey; he first noticed them in 1876, when he was in Utah for the second season of work on the Powell survey. Brief reference is made to them in the address on the "Inculcation of scientific method," above analyzed; it is there announced that their "discovery was something more than the finding of a post-Bonneville fault; it was the discovery of a new method of recognizing faults—of a peculiar type of cliff produced by faulting, which tho by no means obscure had previously been overlooked by geologists." The occurrence of similar

[1] Amer. Journ. Sci., xxvii, 1884, 49-53.
[2] Lake Bonneville. U. S. Geol. Surv. Monogr. 1, 1890, 340.

scarps piedmont to a number of basin ranges was mentioned by Russell in his monograph on Lake Lahontan, and was of course well known to Gilbert. A brief account of these small "fault cliffs" is given in Gilbert's essay on the "Topographic features of lake shores," where it is shown that they differ from level-based shore cliffs in that they do not follow "exact contours but ascend and descend the slopes of the foot hills." They are described in some detail in the Congress excursion guidebook, and it has already been told that they were made the subject of special examination when the excursion party reached Salt Lake City.

More important than the discovery of the small and recent faults in the piedmont alluvium was the recognition of a new kind of topographic evidence for faulting that is provided by the facets which truncate the extremity of many spurs along the western base of the Wasatch Range; for these facets were taken to represent little altered parts of the great fault plane on which the mountain mass has been upheaved. The date of their recognition is not recorded; they were first announced in 1890 in the Bonneville monograph, as follows:

> The western front of the Wasatch is determined by a great fault. From the line of this fault . . . springs a steep face of solid rock, the escarpment of the up-thrown orogenic block. At intervals the rock face is divided by narrow clefts or gateways, whence streams issue from the interior of the range. Between each pair of adjacent streams is an acute ridge of rock, whose roof-like cross-profile marks it as the product of aqueous sculpture. The end of each is truncated by the great fault, and the truncated terminals, standing in line, constitute the rock face at the margin of the [piedmont] plain (307).

The recognition of the existence of these terminal facets was an important step in the study of the visible landscape; the explanation of their origin was an important step in the rational physiography of the lands. When once noticed, they are conspicuous enough, inasmuch as some of them rise several hundred feet over the mountain base; and better still, when they are once understood they become positively attractive features of the mountain view, because the eye turns with so much pleasure to those elements of a landscape which the mind understands.

Yet significant as are the well-preserved terminal facets of the Wasatch spurs, no mention was made of the occurrence of similar forms on the spur ends of any of the basin ranges, either as initial, little-worn facets, or as rounded and dissected facets; and no general statement was published concerning the share taken by spur-end facets in the theoretical aspect of the basin-range problem. They were introduced only as a side issue in a problem of an altogether different nature, and after their brief introduction little more was said of them. Even in the chapters on the Great Basin region which Gilbert prepared for the Congress excursion of 1891, no mention is made of the facets, but the large facets of some strongly truncated spurs were pointed out to the excursionists as their train, on its way southward and eastward after leaving Salt Lake City, approached the Wasatch front to enter the Canyon of Spanish Fork on the way through the range.

A DISSENTING OPINION

Such was the incomplete status of the basin-range problem, as far as Gilbert's own published contributions to it are concerned, when the whole subject was brought to the foreground of his attention by the work of a member of the survey who had had wide experience in the West and who presented an essay at the meeting of the Geological Society of America at Albany at the end of December, 1900, traversing the earlier explanation of the ranges by faulting.[3] The dissenting opinion thus announced seems to have arisen largely from the adoption of a geological rather than a physiographic method of approach to and treatment of the questions at issue. In common with certain members of the Fortieth Parallel survey, the dissenter from Gilbert's views believed that "many of the Great Basin ranges were probably formed . . . at the close of the Jurassic . . . contemporaneously with the Sierra Nevada" (248); he thus not only ascribed great antiquity to existing mountain masses, but failed to distinguish with sufficient clearness between the ancient date when the deformed structures within the existing mountain masses were produced and the modern date when the existing mountains were developed as topographic forms. He knew of the composite theory which, following King, dates the folding of the mountain strata as Jurassic, and which, following Gilbert, dates the upheaval of the

[3] J. E. Spurr. Origin and structure of the basin ranges. Bull. Geol. Soc. Amer., xii, 1901, 217-270.

existing ranges as Tertiary, and which, following Powell and Dutton, introduces a long period of erosion, almost reaching planation, in the intermediate periods whereby the earlier-formed mountains were essentially destroyed before the later-formed mountains were uplifted; but he designated this composite theory as a "compromise" (219), as if it represented a partial and unwilling concession of opinions by opposed partisans, whereas it really represented a well-advised acceptance, by open-minded truth seekers, of the vital elements found in two contradictory theories, and an ingenious reduction of the contradictions by the addition of an equally vital element of a third theory. Indeed the third element—the wearing down of the earlier before the upheaval of the later mountains—does not seem to have engaged the dissenter's attention to any great degree, for without especially inquiring into the characteristics that should be presented by fault-block mountains of a second generation, after the almost complete obliteration of their folded and faulted predecessors, he adopted the view that the existing mountains "as we see them, are the net result of compound erosion since Jurassic times, operating on rocks upheaved by compound earth movements which have been probably also continuous during the same period" (265).

Furthermore, on passing from general considerations to details, the dissenter did not analyze the specific consequences that must follow from the action of subaerial erosion on folded structures; for he concluded that long-continued erosion, unaided by recent faulting, could leave a whole anticline in one part of a range while consuming the greater share of it in another part (238). Indeed, he appears not to have regarded the frequently occurring discordance between the strike of the range structures and the trend of the range form—especially the trend of the base line along the steeper side—as either important or significant, for he compared the basin ranges with the ridges of the folded Appalachians, in which the accordance of rock strike and ridge trend is always a leading characteristic. He even asserted that if "the Appalachians, which likewise [i. e., like the basin ranges] consist of parallel ridges eroded along lines of folding, should become arid . . . there would develop in course of time exactly what exists in the Basin region . . . a series of parallel, synclinal and anticlinal ridges" (255). This clearly overlooks the repeated occurrence of more or less resistant strata which stand obliquely or even transversely within the basin ranges, and which are truncated along the range margins; for such discordance between structure and form is unknown in the Appalachians, except where it is caused by faults.

Finally, in common with others who rejected Gilbert's theory, the dissenter failed to discriminate sufficiently between scarps of different kinds and faults of different ages; for he argued that as "many of the most pronounced scarps are along no fault lines"—that is, are due to differences of structure—and "while many heavy faults have absolutely no direct effect on the topography"—that is, are so old that their original inequality has been obliterated—therefore the theory that the more abrupt sides of the basin ranges are fault scarps, more or less dissected and battered during and since their production, must be untenable. In this connection he stated that "actually ascertained heavy faults along the main fronts of the ranges are exceedingly rare" (259); and he evidently meant by "actually ascertained faults," those that are demonstrated by the occurrence of corresponding strata in discontinuous attitudes on the two sides of an invisible fracture, this demonstration being of course based on the universally accepted geological principle that the corresponding strata were continuous when originally formed. He stated also that "the mountains fronts are, in general, not marked by great faults" (265), thus rejecting or ignoring Gilbert's demonstration of faults based on the universally acceptable but more novel and therefore less widely accepted physiographic principle that if the strata of a mountain front in a continental interior are truncated by the mountain base line, their absence beyond the base line, not being explainable by erosion, must be explained by displacement.

But it was by no means only because of the dissenter's manner of approach to the basin-range problem that he rejected Gilbert's interpretation of it. An equally potent cause for dissent lay in Gilbert's failure to set forth his original views and the reasons for them in sufficient detail when they were first announced; and in his failure to make public announcement of

the modification that his original views suffered in view of the suggestions later made by Powell and Dutton. The brevity of his original discussion in the Wheeler report was regrettable, as has been shown in an earlier section of this memoir; his silence during the following period when others were debating the origin of the basin ranges is more regrettable still. His reticence is, as already noted, probably to be explained in part by a dislike of controversy, and perhaps still more by a feeling that, as he had had his say, he ought not to speak again on the subject until he had new opportunity of field study. If this be true, it is all the more unfortunate that he was not assigned in 1893 to field work in the Great Basin, for there, as well as in Colorado, a good number of topographic sheets were awaiting a geological coloring.

GILBERT AS CENSOR

Gilbert's reaction upon the announcement of the dissenting opinion was to ask for an assignment to field work in the Great Basin for the summer of 1901; his studies then made will be summarized below. In the meantime it is interesting to read his own opinion on the subject before his return to the field. Following its excellent custom, the Geological Society submitted the dissenter's essay to several censors before accepting it for publication; and of these censors Gilbert was one. One of his letter books fortunately preserves a copy of his typewritten report. After an introductory statement to the effect that different views obtained regarding the origin of the basin ranges, the report reads substantially as follows:

The earlier view [Gilbert's own] is that the ranges were made by uplift, the crustal blocks which underlie and compose them being lifted with reference to the blocks beneath the adjacent valleys, and usually divided from them by profound faults. Mr. Spurr's view is that the mountains were made by the erosion of the bounding valleys. After a careful reading of Mr. Spurr's paper and a review of the subject I find myself decidedly of the opinion that his view is erroneous and that the earlier view, especially as modified and restated by King, Dutton, and Russell, is in substantial accord with the main facts, including those adduced by Mr. Spurr in this paper. The considerations on which this opinion is based will be set forth in the following paragraphs.

1. *Relation of Trend to Strike.*—It has been satisfactorily shown that the Appalachian ranges are mountains of erosion in the sense which Mr. Spurr uses that phrase and I take them as a type. They have been developed by erosion from a broad mass of strata which had been previously folded, and also to some extent faulted. As a result of differential erosion valleys have been produced along the outcrops of the less resistant rocks and mountain ranges along the outcrop of more resistant rocks. The characters of the mountain ranges are a direct consequence of their mode of origin: (1) Each range is crested from end to end by a single bed or by a compact series of beds; (2) The trend of the range is identical with the strike of the composing rocks; (3) The range continues as far as the formation continues. . . . In respect to these characters the Basin ranges are contrasted with the Appalachian ranges. The Basin ranges as a rule are less simple in internal structure. In many of them the crest line shifts repeatedly from one formation to another. Anticlines and synclines often traverse a range obliquely, and the associated ridges, whether anticlinal, synclinal or monoclinal, run to the margin of the range area and there stop abruptly. In other words, fold and fault systems, more or less analogous to those of the Appalachian province, are found within individual ranges of the Great Basin system; but the strike of such a structure system is usually more or less oblique to the trend of the range, and the faults and folds are cut off abruptly at the margin of the range. . . .

In my judgment these characters take the Basin ranges out of the class typified by the Appalachian ranges. . . . The trends of the Basin ranges are not dominated by the strikes of constitutent formations, and it is probable that the valleys are not so dominated. It is quite conceivable that, in any individual case, the original position of a river (superimposed or antecedent), may have been oblique to the strike of the formations, and that a great [intermont] valley may have been developed by the river and its tributaries. But in the broadening of such a valley the lateral branches would become adjusted to the strike [of the weaker beds], and the sides of the [intermont] valley would be characterized in detail by strike ridges. If the floor of such a valley were subsequently buried by detritus, the resulting detrital plain would be invaded on both sides by tapering strike ridges. If the valleys of the Great Basin were formed in such a manner the line separating their detrital cloaks from the rocks of the adjacent ranges would be serrate instead of simple. Each of the oblique monoclinal or anticlinal ridges composing a range would project into the valley, and each of the mountain valleys would contain a bay of detritus; there would be a gradual passage from rocky range to detrital plain, instead of the abrupt transition along a simple line so characteristic of the region. In accounting for the oblique transection of folds the erosion theory seems to me full of difficulties and the fault theory comparatively free from difficulty.

2. *Unpaired Monoclinal Ridges.*—The portion of a Basin range adjoining a valley often consists of a monoclinal ridge with the escarpment toward the valley and the dip away from the valley. By Mr. Spurr these ridges are interpreted as the halves of anticlinal arches and the complementary halves, not being visible, are supposed to have been removed by erosion. . . . I doubt if subaerial erosion has ever accomplished such a

work as is here ascribed to it. In the Appalachian region, where the rock floors of the valleys are open for inspection so that the rock structure can be made out, there is no such phenomenon as an anticlinal arch of resistant rock from which one limb has been removed by erosion. . . . The hypothesis of faulting offers so easy an explanation of the unpaired monocline that it has been widely employed by students of Basin range structure; and I am of opinion that the unpaired monocline offers presumptive evidence of a fault.

3. *Cliff Characters.*—The ordinary cliff of differential degradation, or the receding cliff, as it is sometimes called, is determined structurally by a strong bed overlying a weak one, and is maintained by weathering and stream erosion. Where drainage lines cross it there are acute re-entrants, and between these re-entrants rounded salients, so that the contour lines are scalloped. Strongly contrasted with this are certain cliffs or steep slopes occuring at many points on the outer face of Basin ranges. For the convenience of a name I will here call them "cut-off" cliffs. Their lower contours, instead of being scalloped, are comparatively direct lines, often straight for considerable distances. Lines of outward drainage, instead of occupying valleys with flaring mouths, issue from comparatively narrow gateways, making but slight interruption in the lower cliff contours. These characters are best shown where the rock is resistant and of uniform character. . . .

In the study of the Basin ranges, I have regarded the cut-off cliff as diagnostic of faulting. Such cliffs may indeed be produced in other ways, but usually the local conditions seem to bar other explanations. Regarding a cut-off cliff as a product of faulting the mode of its origin is conceived as follows: A great fault is assumed to be made by small increments separated by long time intervals. By the first movement a scarp is produced, and each subsequent movement increases its height, but the slowness of the process gives time for the weathering of the scarp and the reduction of its steepness. It also gives time for the continuous adjustment of cross-drainage. Streams from the heaved block to the thrown block open gorges in the former and build alluvial fans in the latter, maintaining graded channels. Eventually their gorge walls meet above, reducing the interstream tracts to crested spurs which appear as tho truncated by the frontal cliff. . . . Cut-off cliffs are also produced by glaciers, but the glacial explanation need not be discussed in this connection. There is in fact but one other mode of origin which seems worthy of consideration, and that is wave erosion, the process suggested by Mr. Spurr. Where waves attack a mountain base their tendency is to straighten the contours by cutting away spurs, and if the process were sufficiently active and prolonged it would be quite competent to produce such cut-off cliffs as are seen in the Great Basin. . . . But the production of the Great Basin cliffs seems altogether too much to ask of the comparatively feeble waves which could be raised in the narrow arms of Tertiary lakes of the Basin. Moreover, the phenomena of Lake Bonneville and Lake Lahontan show that shore cliffs are characteristically associated with terraces or platforms carved from the rock and with embankments of gravel. If the cut-off cliffs of the Basin ranges are wave-wrought the complementary rock platforms and gravel embankments ought somewhere to be discovered. . . .

4. *New Fault Scarps.*—It was not until 1876 that certain low scarps, traversing detrital slopes in the Great Basin, were recognized as the surface indications of faults. After their recognition as a distinct physiographic type the positions and directions of numerous dislocations were traced in places where bed rock was and is wholly concealed by valley detritus. . . . The significance of these scarps is greatly enhanced by their association with other features. With few exceptions they follow mountain bases, cutting alluvial slopes close to the boundary between alluvium and bed rock. They occur on the sides of ranges where profound faulting is indicated by the transection of folds, by unpaired monoclinal ridges, or by cut-off cliffs. In each case the direction of throw . . . . is such as to depress the valley or lift the range.

It is furthermore significant that the fault theory for Basin ranges was founded on other characters, before the recognition of [alluvial] fault scarps, so that their testimony came as a verification; it proved the presence of faults in places where their existence had previously been inferred from less direct evidence. For example, in 1872 a fault was inferred along the East base of the north part of the House range [the Fish Spring range] because an unpaired monocline there presents its scarp to the east, and for similar reason a fault was inferred along the West base of the South part of the same range [House range proper]; and years afterward the theoretic positions of these faults were found to be marked by low fault scarps, with throws in the theoretic direction.

In spite of these cogent arguments, the dissenting essay does not appear to have been significantly modified by its author, if he read them. Perhaps it was because of their essentially physiographic nature that he regarded them as unconvincing; but surely they must be accepted as compelling by geologists of a more physiographic habit of thought. Two comments may be made upon them. The first is that the great advance which they show over the original statement in the Wheeler report proves that the basin-range problem had really been growing and taking on a more and more matured form in Gilbert's mind during the many years in which he had published next to nothing upon it. His report as censor does not appear to be the result of special study or to have cost him any great amount of time; it represents an accumulation of evidence that had been taking shape in his mind as it grew. When the dissenting manuscript was received, the evidence already formulated in favor of the fault-block theory had only to be written down. The second comment called for by the report is that it adduces no evidence in support of Powell's and Dutton's view that the mountains of Jurassic

date had been worn down to low relief before the existing ranges were heaved up; if that view be correct, traces of the worn-down surface of the old mountains ought to be found on the back slopes of the new ones; but no mention is made of such an old surface. Yet such worn-down surfaces would be of importance in the discussion, for they would correct the idea that the basin ranges result from "compound erosion" operating continuously on rocks that have been continuously upheaved by "compound earth movements" since Jurassic time. If the worn-down surfaces exist in the back slopes of the ranges, they would prove that earth movements had been wanting during a considerable interval of time, and that erosion as well had almost ceased in the latter part of that interval. Gilbert's omission of this significant element of the argument was perhaps because he had, at the time of writing his report as censor, no personal knowledge of any such surfaces. Fortunately, his notebooks of the summer of 1901 show that he discovered a good number of them, and to his field studies of that year we may next turn.

THE SUMMER OF 1901 IN UTAH

Gilbert fortunately secured authority to make "a review of the question of the mode of origin of the mountain ranges and intervening valleys of the Great Basin region" in the summer of 1901. W. D. Johnson accompanied him as topographer, as he had in the Bonneville studies, in spite of the untoward incident of 1893 in Colorado, recounted above. Some extracts from Gilbert's notebooks of this season are presented below, from which it appears that the House and Fish Spring Ranges in Utah were selected for special study; these being the same ranges which 29 years before, as already told on an earlier page, were most definitely described in the field notes of his first season as consisting of uplifted blocks, one with eastward tilt on the eastern side of an inferred fault line, and the other, farther north, with westward tilt on the western side of the same line; and it was these ranges which then impressed him so vividly as typical examples that, when he was at the bottom of the Colorado Canyon two months later, he drew from memory an imagined bird's-eye view of them in a notebook diagram, here reproduced on page 56.

The field season of 1901 seems to have been both successful and enjoyable, as it resulted in the collection of a large body of valuable material, closely pertinent to the problem under discussion; but this material was, most unhappily, never published on account of the disappointment caused by a calamity in the following spring. Before recounting that sad episode, a few items may be quoted from Gilbert's letters of the summer; both include passages that illustrate very well his quizzical way of putting things. The first was to one of his sons:

> This letter has been interrupted while the cook cut my hair, taking off all but about a quarter inch—and making prominent some scars I got two days ago in trying to lift the roof of a cave. I was in the cave for shelter from a rain storm when I heard the roar of a storm-torrent. It was two miles away, but I thought it close and started to run and see the spectacle. Then I met the cave roof and stopped and sat down again. Fortunately the damage was only skin deep. . . . We call this "Sunday" regardless of the calendar, because Johnson & I are both staying in camp.

The tumult of temporary torrents by which the silence of the desert is occasionally broken is a characteristic of the arid region; another instance of the kind was recorded in one of Gilbert's notebooks for the same summer, when an hour after a storm at night the roar of the flood was heard; the next morning a sheet of water covered the low-lying playa in the trough of the intermont detrital plain.

Later in the season a letter to a geological correspondent outlined the work in hand and commented on some of the conclusions reached:

> I have not been outside of Utah and am now homeward bound. I began with a study of the physiographic expression of faults in the Wasatch and continued it in the Oquirrh; and the chief thing I have done since is to work out the structure of the House range. What was long ago suspected is now proved, that certain triangular faces of the Wasatch, between canyons, are remnants of an inclined fault plane. They retain slickensides and slabs of indurated shear-zone. On the Oquirrh they are followed below ground by mines, the hanging wall being wash. . . . Among my interesting finds are a number of mistakes made by Gilbert, one of the Wheeler geologists, in 1872; but he was substantially on the right track as regards Basin range structure.

The mistakes in his own earlier work here referred to in third-person phrase probably lay first, in the failure then to recognize the long period of erosion between the early epoch of folding and the later epoch of block faulting, for in the season of 1901 he found abundant evidence of such an erosional period, as will appear in quotations from field notebooks introduced below; and, second, in the earlier assumption that block faulting had been accomplished chiefly by vertical upheaval, an assumption which it was impossible to maintain in view of the moderate inclination of the mountain-margin fault planes, as represented by spur-end facets in the Wasatch and other ranges. It was probably an experience of this summer that was recalled 15 years later when Gilbert wrote to his elder son, who was then engaged in engineering work in the dry country:

> The only remedy I know for a Utah desert wind is to camp in the lee of an irrigated farm, and that raises the question of preference—mosquitos or dust.

After stopping field work he made a visit to the Colorado Canyon with the director of the survey, and went as far as San Francisco before returning to Washington.

LOSS OF THE FIELD MAPS

Work on his Alaska report appears to have occupied much time during the following winter, but in December Gilbert spoke on "Western Utah" before the Geological Society in Washington, when it is to be presumed he gave some account of his summer's work; and at the end of the year when the Geological Society of America met in his home city of Rochester, he presented a brief paper on "Joint veins," illustrated by a much fractured rock from the House Range; this was an example of the small-topic problems which he repeatedly discussed at scientific meetings in his later life. It was in the following spring that, by a most unhappy accident, all the topographic records of the work in Utah were destroyed, and this so seriously discomfited him that he stopped further work on the basin ranges, published practically nothing on the campaign of 1901, and, after completing his report on "Glaciers and glaciation" for the Harriman expedition, turned from his old Great Basin problem to a newer one in California. Not until a year after his talk on "Western Utah" did he make a more formal statement of the results he had gained in 1901 at the Washington meeting of the Geological Society of America, in December–January, 1902–3; he then emphasized the significance of the simple base line that characterizes the margin of certain basin ranges, regardless of their structure, and showed that such a base line proves the presence of a strong fault; at the same time he stated his belief that this feature had not been sufficiently understood by others. Those who were present at this meeting will remember, and others who were not there should be told, that Gilbert's manner when making his statement was the gentlest possible, without a trace of controversial enmity. The printed record of the statement in the Proceedings of the society is unfortunately nothing more than a perfunctory abstract which occupies only nine lines, and of these but four bear closely on the problem at issue:

> Evidence of block faulting was shown to exist in extensive shear zones, in triangular facets truncating the ridges in front, and in the even linear bases of the ranges. That the faulting is still going on is shown by displacements in the recent alluvium.

Not a word about the long period of erosion between the early folding and the later faulting; not a word about the replacement of upheaval with little compression by upheaval with great horizontal extension. But both these important modifications of his early views are clearly indicated, one in his notes, the other in a later, much later, reference to the origin of the basin ranges, as will be shown in later sections, where a change in the statement as to linear range bases will also be found.

Gilbert did not again take up the basin-range problem until the last four years of his life. It was as if the loss of the topographic records had for the time completely thwarted him in the attempt to return to and extend his early studies in the western field. The disappointment must have recalled to his own mind the regrets that he had felt 20 years earlier when Powell summoned him from Utah and placed him in charge of Appalachian geology. But he was uncomplainingly silent in the matter; even his nearest associates on the survey heard no word of

impatience from him; the records were lost and there was an end to the season's work. The few intimate friends to whom he mentioned the calamity might have thought he was talking about the misfortune of some other person, so calm was his mien under this cruel buffet of adverse fate. Yet his uncomplaining reticence only exemplified the philosophy of life that he practiced more than he preached. While he always did his best to prevent trouble that could be prevented, he seemed to realize not only the uselessness but the harm of spending time and words in regretting what was past and beyond remedy. He turned instead to something new that could be accomplished; his life was one of practical optimism.

FIELD NOTEBOOKS OF 1901

How deep must have been the disappointment over the loss of his maps, if it caused a man so well balanced as Gilbert to leave unused all the laboriously recorded notes of his summer among the basin ranges and turn away from the fine problem that they represented. Even without the maps, he had the materials to make a noble essay, grounded on an abundance of critically determined facts, illustrated with detailed sections and excellent photographs, and set forth along many converging lines of convincing logic in a manner that Gilbert knew so well how to employ. And yet it all came to naught!

The field route of 1901 led Gilbert and his topographer, Johnson, for a short distance southward from Salt Lake City along the base of the Wasatch Mountains before they struck westward across the desert to the basin ranges proper. Many items were noted on the way concerning such physiographic matters as "mature topography," "graded plains," "cycles of erosion," and the like. One of the first was a high valley in the Wasatch front, belonging to a topography established "after some faulting but before the main faulting." The truncation of the valley ' was begun by faulting and continued (perhaps) by the breaking away of the [fault] cliff sapped by continuance of faulting." A diagram here shows in section the loss of a large part of an upfaulted mass, marked X, between the steep pitch of the fault plane and the gentler slope of the actual profile; and the notes ask:

> What has become of the wedge X? The visible alluvial cones, etc., do not account for it, but it may be buried under lake beds on the thrown block.

A more general statement is then added:

> This high valley is one of a system, all reaching the cliff above PB [Provo Beach] and some above BB [Bonneville Beach]. There is an upland topography, older than the cliff and truncated in the production of the cliff. Still older is the topography of the high alluvial cones. Still younger is the shallow trenching of the cliff by the drainage from the upland valleys.

At a point farther along the Wasatch front the spur-end facets, slightly dissected, were taken to represent the fault plane of mountain uplift, although they have the small dip of 30° below the horizontal, or the large hade of 60° from the vertical. At another point parts of three spurs were found to have fallen in a landslide, as if exemplifying the suggestion above quoted that they had been sapped by a continuance of faulting. This raises a question which the notes do not answer; namely, whether the spur-end facets are parts of a true fault plane which descends uniformly far down into the earth's crust, or whether they are parts of a curved fracture surface which, unlike a true fault plane, decreases its dip as it descends. Surely a surface which hades 60° to the downthrow can hardly be the plane of a deep fault caused by vertical forces of upheaval; but it might represent the lower part of a concave fracture surface, the upper part of which, now lost by erosion, was steeper, following a principle "enounced" by McGee but little attended to by his contemporaries.[4] If the better explanation be found in a curved surface, then the highest portions of the uplifted mountain block must as a rule have been rapidly eroded, by reason of the steepness of the fault scarp there; and erosion must now be slower.

---

[4] W J McGee. On the origin and hade of normal faults. Amer. Journ. Sci., xxvi, 1883, 294-298.

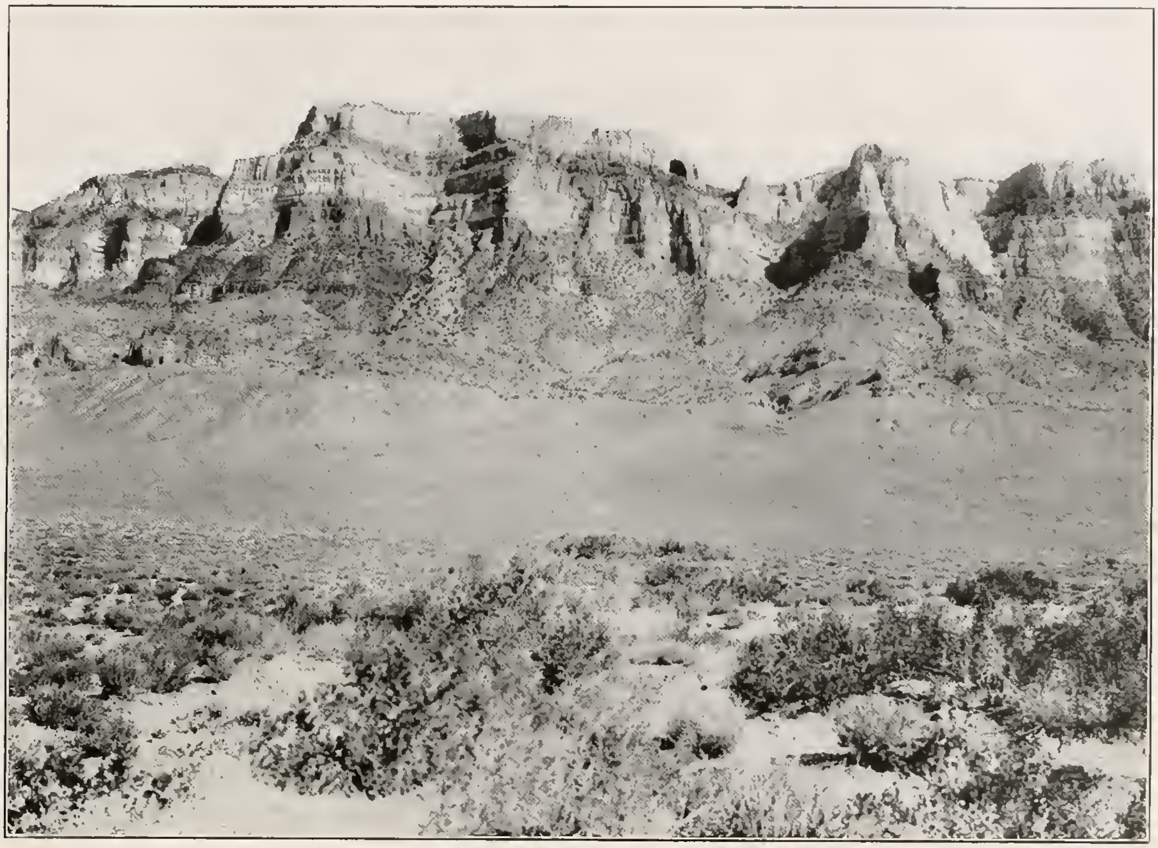


FIG. 13.—WESTERN FACE OF THE HOUSE RANGE, UTAH

Photograph by G. K. Gilbert

FIG. 14.—EASTERN SLOPE OF THE HOUSE RANGE, LOOKING SOUTH; SWASEY MOUNTAIN IN THE DISTANCE

Photograph by G. K. Gilbert

FIG. 15.—WESTERN FACE OF THE HOUSE RANGE, LOOKING SOUTH

Photograph by G. K. Gilbert

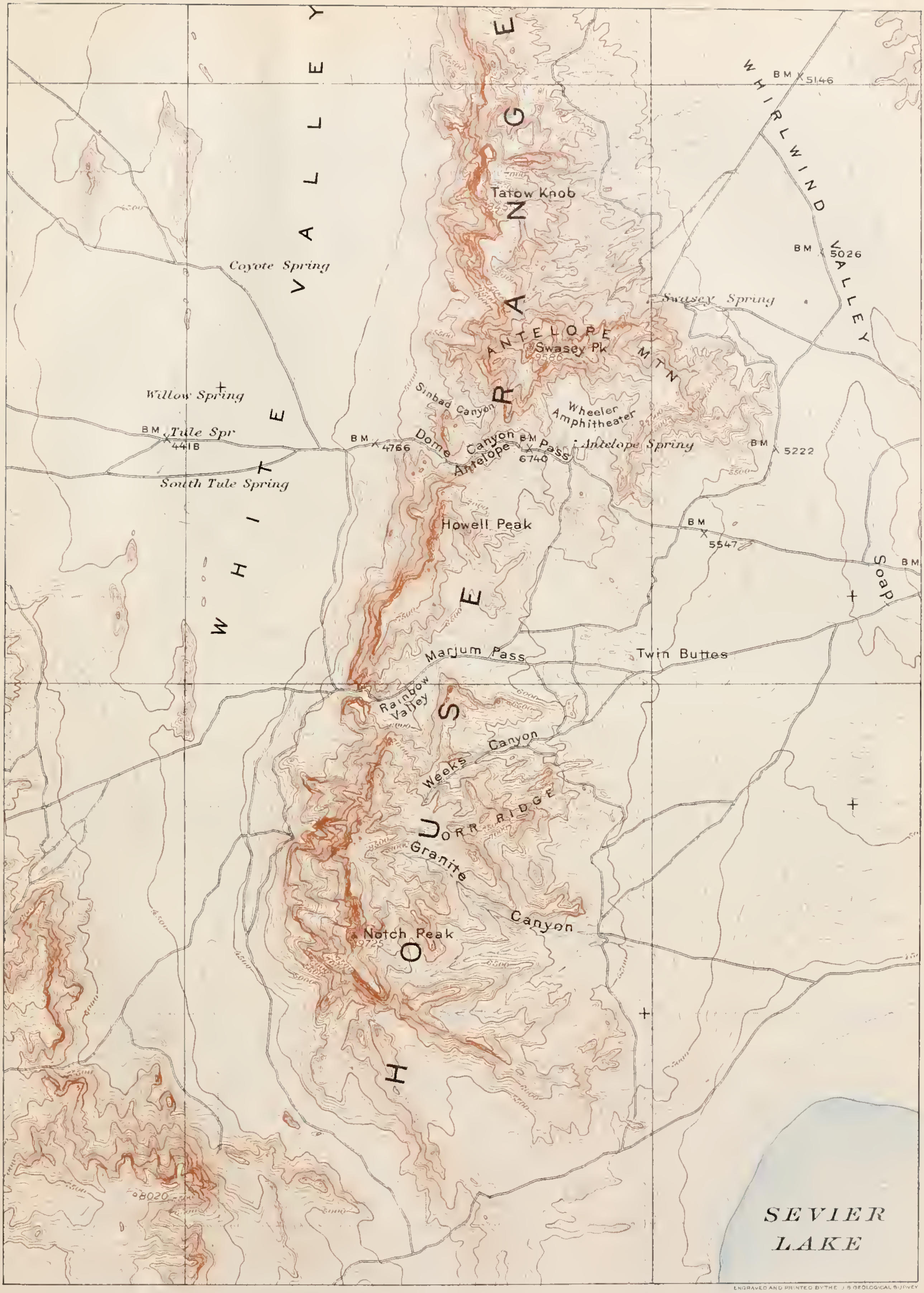


Fig. 16.—SOUTHEAST PART OF FISH SPRINGS QUADRANGLE, U. S. GEOLOGICAL SURVEY, EDITION OF 1910

Scale $\frac{1}{250000}$

2 1 0 2 4 6 8 10 Miles

2 1 0 2 4 6 8 10 Kilometers

Contour interval 100 feet

Datum is mean sea level.

Many instructive items are later recorded concerning the individual ranges to the westward. The Oquirrh Range "shows an anticline cut off obliquely by the Cedar Valley front," in which the harder strata stand up as ridges, while the weaker ones are worn down in reentrants and invaded by the alluvium of the "valley" or intermont plain.

In one of the reentrants . . . part of the alluvium is a veneer over degraded r. i. p. [rock in place]. This would seem to indicate that the range has long stood without uplift. The growth of the outer plain has raised the base level, thus causing the alluvial area to encroach, and in an intermediate zone the streams have been so long held at one height that they have planed the rock to their grade profiles. Naturally, with this condition there are no fault scarps.

This passage is significant as being one of the most explicit statements anywhere made by Gilbert with regard to the effects of time upon the form of a fault-block mountain front. Manifestly, with still longer time, even the hard-rock ridges would be worn back from the fault line, and the physiographic evidence of the faulting would be greatly weakened or lost.

Another novel point is soon noted. The Oquirrh Range was seen to have on its western side a sloping surface that had been planed independent of its structure, and that appeared to descend under the valley alluvium. This appears to be the first instance in which Gilbert recognized a prefaulting surface of degradation as such, thus verifying the views of Powell and Dutton as to a long cycle of erosion after the period of deformation by which the ancient mountains of the region were produced, and before the period of block faulting which gave rise to the existing ranges. Another old surface of the same kind was seen traversing the synclinal structure of the Lake Range at a high level; and here the field notes explicitly argue the case to its legitimate conclusion:

This plain traverses the structure and is a most remarkable survival of a condition before the creation of the faces of the range. It would seem a fair inference (1) that the synclinal structure is older than the summit plain, (2) that the production of the range (by uplift) is more recent than the summit plain, ∴ (3) that the uplift is independent of the synclinal folding. That this range should have been developed by erosion of surrounding [structures?] without obliteration of the summit plain is incredible ∴ (by elim.) the range is due to local uplift between faults (weak argument?).

This is, as far as can be learned, Gilbert's first written statement of the argument for a two-cycle history of the mountains; and as such the doubt parenthetically expressed at its close is not unnatural. Would that the geological world might have had explicit statement of his later and matured opinion upon this most significant matter. But what that opinion would have been is clearly enough indicated on later pages. The Aqui Range is described as having "a high inclined table, a bit of old peneplain uplifted unequally?" Here the final interrogation mark shows that an open mind was still preserved. Doubt is again given its rightful expression in a note on the Stansbury Range, where it is suggested that certain high-level remnants of an old topography might possibly survive after the erosion of the broad intermont valley; but the antidote to the doubt immediately follows:

The degradation of the valley by ordinary processes would take so long that the coincident erosion of the mountain would obliterate the old topography.

When the House Range was finally reached, the doubts vanish and a definite conclusion was reached, as will be told below.

Other ranges appear, like the Oquirrh, to have long stood still since their last uplift. The Cedar is well described as "a stagnant range draining to a stagnant valley." It has "no scarps; no trace of fault lines. This is an old worn-down range." The Simpson Range shows truncated structures on both sides; it is a rather narrow, boldfaced range, but its marginal faults are not continuously traceable. The Dugway is described as a "static range"; a fault, marked by truncated structures but not by scarps, was traced for 8 miles along its southwestern face and appeared to extend farther. "In its known extent, the fault [which is 'a mountain making fault, separating the mt. massif from rock waste'] truncated a syncline of great size, bringing in succession to the springing line of the mt. several thousand feet of strata. Its line is definitely a mountain front and at only one point does an important graded plain encroach upon the mountain mass." The Deep Creek Mountains give the impression of being "primarily an uplifted mass, the bounding fault being sharply marked out by the line between alluvium and rock."

THE FISH SPRING RANGE

On nearing the Fish Spring Range—the northern companion of the east-tilted House Range—Gilbert first gained the impression that it had been uplifted in mass and tilted to the west, with a fault along its eastern base; on closer approach it was described as consisting of four principal blocks; after more detailed inspection, during which a number of sections were studied out, the smaller complications of its structure were discovered:

The Fish Spring range is far from being a simple monoclinal uplift, flexed at the west and faulted at the east. It consists of a dozen or more large blocks, each lifted and tilted westward. They are separated by faults and unequally uplifted. The uplift is greater at the south than north, and with an exception is greater along the central axis than at the margins. Each block is traversed by minor faults in large number. In a general way it may be said that this structure has originated from general stresses acting within the zone of fracture and is the equivalent of folding and mashing in the zone of flow. It has originated in the upper part of the crust. I see no way to discriminate in this case, the structures produced in the making of the present range from the structures belonging to the older history of the region.

It may have been the accumulation of such experiences as this that led him later to say, in another connection:

I am satisfied that all our results in geology are tainted by the tacit assumption of simplicity that does not exist.

A good number of sections were carefully studied here, evidently with the idea of determining faults within the range by the repetition of identifiable strata in the way familiar to geologists, rather than by the physiographic evidence, which, to some geologists at least, is less convincing.

The Confusion Range lies farther southwest and south and is at first separated from the House Range by the broad desert plain of White Valley. Its northern part "has the aspect of the Cedar, an old range worn down toward the roots, fringed by veneering and invaded by graded slopes." Farther on "the aspect is different. The [eastern] front is bold and finally mural. The alluvial reentrants finally disappear. The top is a remnant of an old peneplain, truncating the structure." Here White Valley narrows as the Confusion Range approaches the House Range; near their southern ends the valley is only 2 miles wide.

THE HOUSE RANGE

Gilbert's goal in this season of field work was, as above noted, the House Range, which he had selected 30 years before as a typical example of an uptilted, fault-block mountain. Like its many companions, it rises over intermont detrital plains which hereabouts stand at altitudes of from 4,500 to 5,000 feet and, with the mountain ranges, constitute a characteristic part of the Great American Desert; undoubtedly great, undeniably American, and unredeemably desert. The range and its surroundings are well represented on the topographic map of Fish Springs quandrangle, surveyed in 1908 and published in 1910 on a scale of 1: 250,000, with 100-foot contours. The mountain mass is 40 miles in length, north and south, and from 5 to 10 miles in width; its crest is over 7,000 feet in altitude for 30 miles its length and has two summits over 9,500 feet. A gradual descent is made to the east; an abrupt escarpment much dissected by huge, steep-pitching ravines falls off to the west, with great piedmont fans of detritus outspread from each ravine mouth. Many Bonneville shore lines are lightly marked on the fans, thus exhibiting the relation from which Gilbert had years earlier inferred that the region had had a long arid history before the brief humid epoch of lake expansion, and that the post-Bonneville epoch was a very short-lived interval.

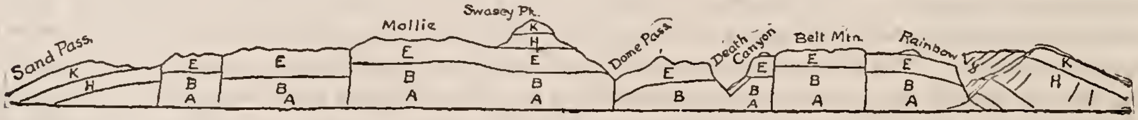


FIG. 17.—Generalized frontal section of the House Range; from Gilbert's notebook, 1901. Low-lying frontal blocks are omitted.

The range is high enough to receive light summer showers from the little wisps of rain which lofty cumulus clouds not infrequently condescend to trail beneath them as they drift

eastward; but the wisps usually vanish by evaporation in the dry lower air before they reach the desert plains; it is only when a great atmospheric overturning causes a violent but local thunderstorm that the lower ground is made "wet enough to run," and supply the torrents which Gilbert described as roaring along the dry channels—true "wadies"—of the plains. As the range is nearly everywhere barren, outcropping ledges are abundant. Near the crest there are occasional good-sized pines and firs; where springs occur in the uplands, they are marked by copses of green aspens, which stand at the focus of many converging paths worn by wandering herds of horses and cattle driven up the mountain for pasture by the neighboring stockmen. The streams are mostly intermittent and wither away on approaching the mountain base. Some of them are subject to a well-marked diurnal variation of length; evaporation during the hours of glaring sunshine compels them to shrink back into the rocky shelter of the upper glens and the cool shade of the high-level aspens; only after sunset do they, nymph-like, venture down the great ravines to the rocky notches at the mountain base; there they sing all night long a gleeful little serenade, as if to welcome the traveler who at the end of a fatiguing day on the shadeless desert plains toils up the tiresome slope of a piedmont fan to wait for their descent. By morning they have run out a short distance on the gravelly slope; but the parching heat of the day soon causes them to retreat to their mountain fastnesses again.

THE WESTERN FACE OF THE HOUSE RANGE

Gilbert approached the House Range from the north and followed its western escarpment southward, making ascents to the crest at various points and finally crossing to the eastward. He found the range to consist in the main of Cambrian sandstones and limestones with some shales, dipping eastward for the most part, but turning northeast and southeast at the lowering ends; the mass was discovered to be divided into seven or more blocks by transverse vertical faults of moderate or small displacement, and these structural features were generalized in a diagram, here reproduced in Figure 17, showing the range as seen from a point north of the middle of White Valley; the southernmost part is foreshortened to too small a length. The dissection of the northern part is described as so far advanced that its preuplift form was not detected there; but the highland of its southern part, although trenched by deep valleys, is said to be an east-dipping peneplain. The contrast between the more gradual eastern slope and the bold western face is repeatedly emphasized. The base of the western face is gently sinuous, but no distinct facets were found on the spur ends. Nevertheless it is held that only the modern uptilting of a great block of the earth's crust can reasonably explain such a mountain range. The fault along the western base is, however, not treated as a simple fracture; many fragments or small blocks of the great uplifted mass, seen along its western base, are regarded as due to step faulting:

> The irregular and sporadic inclined and downthrown blocks on the W face of the House range may be regarded as details of the great fault, a fault of which the minimum measure is . . . not less than 6,000 feet.

The possibility of a secondary origin for some of the small blocks is noted:

> Some of the dropped masses along the front of Belt [a division of the range, south of its middle, limited by transverse faults] may be landslips.

This recalls the suggestion made above as to the origin of certain landslides on the Wasatch front. It should be noted that, in so far as the transverse faults in the House Range are concerned, their steep or vertical attitude suggests that they are due to forces of unequal uplift in the upheaved block, and not to the extensional forces by which the block was upheaved.

While the northern part of the range was under examination and its bold scarp was in view it was concisely described as—

> a conspicuous example of uplift along a fault—as distinguished from survival from erosion. It is incredible that the erosion of White valley [the intermont depression to the west] should have left this great facade. The time consumed in developing such breadth of valley [10 or 15 miles] would have sufficed also to give the mountain a mature topography—unless the final work of erosion was at the mt. base and very deep. The many

buttes of White valley testify to its essential shallowness. . . . The wave theory is inadequate. I doubt if there is a shore cliff 3000 feet high—certainly there is none on the borders of a lake or land-locked bay.

This conclusion was fortified when the southern half of the range was examined. The highest limestones of the range—K in Gilbert's sections and diagrams—occupy considerable areas of its eastern slope; as seen from the west they are visible in small force at the northern end of the range and on one summit, Swasey Peak, near the mid-length of the crest; but in the southernmost 10 miles of the range they form bold cliffs which cap the west-facing escarpment as it gradually descends to its end. As the whole body of strata in the south has a gentle dip to the southeast, the shales between the resistant capping limestones and the resistant underlying strata determine an oblique depression—Rainbow Valley—trending northeast-southwest into the range. Gilbert was much impressed with the features of the range front on finding that the resistant basal strata seen along the middle of the range, the weak intermediate shales that obliquely descend to its southern part, and the resistant capping limestones that slant down to its southern end, are all cut off at the same gently sinuous line along the western base. The gradual loss of height by the capping limestones as they decline to the end of the range was repeatedly noted as explicable only by faulting of a comparatively modern date. It is indeed difficult to see how any other explanation can be entertained.

VERTICAL UPLIFT OR HORIZONTAL EXTENSION

Had the notes of the summer of 1901 been expanded in a written report it is probable that mention would there have been made of a new interpretation not recorded in the field, although the facts and inferences that warrant the record were written down clearly enough. It will be recalled that the original announcement of Gilbert's basin-range theory, quoted in an earlier section, included the statement:

The movements of the strata by which the ridges [ranges] have been produced have been in chief part vertical along planes of fracture, and have not involved great horizontal compression. . . . The forces . . . have manifested themselves at the surface as simple agents of uplift, acting in vertical or nearly vertical planes.

It should also be understood that the notes of 1901 make occasional reference to the "uplift" of the mountain blocks. But if the gently sloping spur-end facets of the Wasatch truly represent the fault surface on which that mountain block was displaced, and not landslide surfaces of later date and of much less inclination than the fault surface, and if the Wasatch fault thus determined is typical of the mountain-block faults elsewhere in the Great Basin, it would seem as if the forces by which the faults were produced could not be any longer described as "simple agents of uplift, acting in vertical or nearly vertical planes," but rather as peculiar agents of extensional uplift, acting on sloping planes and causing a marked broadening of the region of their operations. The mechanical interpretation of such extensional uplifts might have led to views as novel as those expressed in the explanation of the process of laccolithic intrusion.

A brief reference to this new interpretation is found in one of Gilbert's latest essays, in which attention is given, among other matters, to the depth at which subcrustal movement takes place in the reestablishment of an isostatic equilibrium after it has been disturbed. The case of compression is first considered:

In some regions, such as the Appalachian, overthrusts and folds testify to great reduction in the horizontal extent of rocks at the surface, the reduction having been accomplished in a small fraction of geologic time. If the subjacent portion of the nucleus had been correspondingly forced into narrower space there would have resulted an enormous mountain range, but the actual uprising was of moderate amount. Plausible explanations of the phenomena necessarily include horizontal movements of the upper rocks without corresponding movements of the nucleus and thereby imply mobility of an intervening layer.

The case of extension is then taken up:

In certain block-mountain districts of the West the master faults are antithetic in type to the overthrust and demonstrate pronounced extension of the upper part of the crust. The nucleal tract beneath could not share in this extension without creating an enormous depression, which does not exist; and the interpretation of the phenomena involves horizontal shear in material more mobile than the visible upper rocks.[5]

---

[5] Interpretation of anomalies of gravity. U. S. Geol. Survey, Prof. Paper 85-C, 1913; see p. 35.

It is a loss to science that Gilbert's genius was not applied to the further discussion of the problems thus brought to light.

By very reason of the abundance of material recorded in the notebooks of 1901, it is saddening to look them over. True, they indicate plainly enough that Gilbert enjoyed the summer and had the exhilaration of making many successful searches and many encouraging discoveries, even though the heat of the desert may have sometimes taxed his endurance. And the notes leave no doubt, as the extracts quoted above must suffice to show, that in so far as concerns visited examples, the basin ranges are displaced and eroded fault blocks; but precisely because the search was successful and because the origin of the ranges was so well proved, it is grievous to look over the rich records and to know that Gilbert was so disheartened by the calamity which followed their collection that he set them all aside and turned to other work. Great would have been the profit to American geology had this campaign among the basin ranges led to a report similar in volume to that on the Henry Mountains. Rich as the notebooks are, the report would have been richer still, for Gilbert had the habit of carrying a large part of his results and practically all his argument in his memory. It is not likely that much would have been told regarding the ancient deformation of the region and the resulting Cretaceous topography, but a clear statement of its modern deformation and of the existing topographic features resulting therefrom would surely have been forthcoming, and with a safety of demonstration and a wealth of illustration that would have convinced all readers. It may be confidently believed that, among the results reached, there would have been a satisfying definition of the two main periods of strong deformation and active erosion, as well as of the long intervening period when deformation essentially ceased and when erosion gradually weakened. While it would then still be in a certain sense true that the existing structures and forms of the Great Basin are the result of "compound earth movements" and "compound erosion" operative since Jurassic times, it would also be true that the replacement of these general, not to say indefinite, terms by more specific terms would constitute an important advance in the knowledge of the basin ranges.

RANGES IN THE HUMBOLDT REGION, WESTERN NEVADA

By good fortune it was just at the time when the discussion of the basin-range problem was renewed at Washington that a substantial contribution to its settlement was made by Louderback, of the University of California, in his studies of certain ranges in the Humboldt region of western Nevada; [6] and as Gilbert was asked to present the results thus gained at the St. Louis meeting of the Geological Society in December, 1903, he had the pleasure of reading to his assembled colleagues a very striking demonstration of the correctness of the composite basin-range theory, provided by work entirely independent of his own. Indeed, the verification for the three essential elements of the composite theory thus afforded was exceptionally complete: Mesozoic formations of marine origin were folded and upheaved, presumably in mountainous disorder, after the close of Jurassic time, as King and others had first demonstrated many years before; the post-Jurassic mountains thus raised were reduced by long-continued relief, as Powell and Dutton had inferred, and in the locality here studied the worn-down surface was unconformably covered by nearly horizontal tuffs and basalts; the compound mass was then, as Gilbert had supposed, faulted and irregularly displaced at a time so recent that but a moderate amount of erosion has subsequently taken place.

The unconformable volcanic cover, by which both the extinction of the post-Jurassic mountains during a period of crustal quiet and the modern upfaulting of the existing ranges were so convincingly proved, was an addition to the basin-range problem as unexpected as it was apposite; and Louderback's timely recognition of its pertinence in this respect constitutes the greatest advance made in the study of the basin ranges since the King, Powell-Dutton, and Gilbert elements of the composite theory were introduced a quarter century or more earlier. The history of the ranges in the Humboldt region probably holds good for many other ranges also; for it can hardly be supposed that the covering basalts selected an exceptional part of the Great

[6] G. B. Louderback. Basin range structures of the Humboldt region. Bull. Geol. Soc. Amer., XV, 1904, 289-346.

Basin for their area of overflow. The worn-down pore-faulting surfaces in these ranges may, like cans of baking powder taken from the open market for analysis, be regarded as sample areas preserved for inspection by chance—the chance of their having received a lava cover—from among many other worn-down Great Basin areas; and therefore the rock mass of the other ranges, even though they, having no basalt covers, are to-day so much dissected as not clearly to reveal their worn-down surface, have presumably experienced the same long period of down wearing by subaerial erosion after their ancient period of folding, and the same kind of later deformation by faulting as those in the Humboldt region. But as this argument from analogy even though supported by later studies of certain ranges that are not lava covered, may not appeal to all readers, it is still desirable, as was suggested in an earlier chapter, to examine the Mimbres and other ranges in southwestern New Mexico and southeastern Arizona, which Gilbert described in his Wheeler report as consisting of deformed Paleozoic rocks buried under lava sheets, in order to learn how far they confirm the evidence of the Humboldt Ranges.

The presentation of Louderback's paper at the St. Louis meeting called forth little discussion, because its arguments were conclusive. However, Emmons, who had long previously been associated with King on the Fortieth Parallel survey and who happened to be present, remarked that the conclusions reached seemed to him to support King's views as to the origin of the basin ranges by post-Jurassic deformation, because the post-Jurassic deformation was so clearly proved; but this was objected to by another member, who pointed out that the basin ranges are existing topographic features which Louderback had shown to be of very modern origin by upfaulting, while the mountains which King had inferred to result from post-Jurassic folding were of so ancient origin that they had been obliterated by erosion before the existing ranges were upheaved. When the objector afterwards asked Gilbert why he had not corrected Emmons's misconception, he replied with a laugh: "I didn't want to be too hard on him." It was as if he felt satisfied to leave the correction to the gradual processes of time.

CHAPTER XXVII

SCIENTIFIC RELATIONS: 1901–1910

DECREASING RELATIONS WITH SCIENTIFIC SOCIETIES

Following the turn of the century Gilbert's relation to scientific societies gradually decreased; he held few offices in them in later years because he had been president of most of them in earlier years. Nearly all of his communications during the later period were on relatively small subjects, briefly but keenly treated; and after a serious illness in 1909 he absented himself from all scientific gatherings. Previous to this withdrawal, the Philosophical Society of Washington heard him speak in 1902 on "Flotation and fiords," a problem suggested by his visit to Alaska with the Harriman expedition and here already treated in the account of that journey; in the same year he spoke also on the "Mechanism of volcanoes," and in the year following on the "Feasibility of measuring tides and currents at sea." The first of the last two topics was published in abstract,[1] and seems to have led to a second article[2] in which the fairness of Gilbert's nature is well revealed. He there said that he had inadvertently introduced as his own in the first article an idea proposed by another writer concerning the origin of a volcanic feature—the spine of Mont Pelée—but had afterward discovered that he must have unwittingly borrowed it. He frankly states: "As I find interest in the mental processes of my blunder, I venture to relate what I suppose to be its history"; and he then explains that he must have read the idea in question somewhere and held the ghost of it unconsciously in mind; for he recalls that when writing his own article he had a faint suspicion that his idea might have come from some outside source, and that this suspicion had led him to search through all the pertinent articles that he could find in hopes of discovering the origin of the idea; but he had overlooked the right article.

> Thus a mental impression too faint for complete identification, now that attention is directed to it, nevertheless rose into consciousness with the semblance of an original idea, and gave rise to a distinctly plagiaristic publication.

After the new century opened, Gilbert's relation to the Washington Academy of Sciences was practically limited to the part that he took at a meeting held in February, 1903, in commemoration of his long-time chief, Major Powell, his share being an account of Powell's work as a geologist.[3] But the words that he then spoke as well as the sketches of Powell's life that he published elsewhere[4] are, after his habit—one might say according to his principles—almost wholly impersonal; the rich fund of reminiscence by which the scientific content of the sketches might have been agreeably leavened was not drawn upon; and thus science lost the record of many an incident which would have thrown upon Powell's nature quite as bright a light as was shed by the account of his work as an explorer, an administrator, and an ethnologist. It is truly lamentable that inner views of the steps taken by the director of the national survey in developing that great organization and narratives of many illuminating incidents that must have been interwoven with its development were not recorded by his intimate associate who, first as his chosen counsellor and later as his chief geologist, had close personal knowledge of every item of progress. One passage, however, in which Gilbert revealed Powell's scientific generosity may be here quoted, because it is equally applicable to Gilbert himself:

> Phenomenally fertile in ideas, he was absolutely free in their communication, with the result that many of his suggestions—a number which never can be known—were unconsciously appropriated by his associates, and incorporated in their published results.

---

[1] The mechanism of the Mont Pelée spine. Science, xix, 1904, 927–928.

[2] A case of plagiarism. Science, xx, 1904, 115.

[3] Powell as a geologist. Proc. Wash. Acad. Sci., v, 1903, 113–118.

[4] John Wesley Powell. Science, xvi, 1902, 561–567; reprinted with revision in Ann. Rept. Smithson. Inst., 1902, 633–640. John Wesley Powell, The investigator. Open Court, xvii, 1903, 228–239, 281–290. The promoter of research. Ibid., 342–347.

Before the Geological Society of Washington Gilbert spoke twice on Alaskan problems; the "Morphology of southern Alaska" in 1901 and the "Statics of a tidal glacier" in 1903; in 1901 he had presented to this society some "Petrographic and geologic notes on western Utah," this being one of his few statements concerning the field season of that year among the basin ranges, here treated in a separate section; unfortunately not even an abstract of these notes was published. He presented before the same society several papers embodying the results of special studies in the Sierra Nevada which were also communicated elsewhere; in 1908 he gave an account of his "Hydraulic laboratory at Berkeley," and a year later repeated here the presidential address on "Earthquake forecasts" which he had delivered to the Association of American Geographers at Baltimore shortly before.

At the winter meeting of the Geological Society of America in Rochester, December, 1901, he gave a short paper on "Joint veins," a subject suggested by a body of shattered rock found the previous summer at the foot of one of the basin ranges; a year later in Washington, on New Year's Day, 1903, he presented his reasons for still holding to the view that the basin ranges are uplifted and dissected fault blocks, in spite of an adverse interpretation that had been published a few years before; but this important communication is represented only by a nine-line abstract in the society's bulletin. The interpretation of the basin ranges that he adopted is discussed in an earlier section of this memoir. At the St. Louis meeting, in December, 1903, he presented Louderback's essay on the Humboldt Ranges, in the absence of its author. Reference to his second election to the presidency of this society for 1909 is made below.

Residence in California for parts of a series of years brought various new problems to Gilbert's attention, as will be detailed in other sections; it also gave him opportunity of attending several meetings of the Cordilleran section of the Geological Society of America; in 1905 he presented there three papers on Sierra Nevada, which he gave also before the Geological Society of Washington, as above; and the next year he described the "Transportation of débris by the Yuba River," a chapter from his long Californian study of the outwash of débris by hydraulic mining in the mountains. In 1907 he was elected a counsellor of the Cordilleran section. In the same year he lectured to four different audiences in California on the "Salton Sea," which had been then produced in the southern part of the State by an overflow of the Colorado River into a depression, once the head of the Gulf of California, but later inclosed by the river delta and evaporated to dryness in the arid climate there prevailing. A reference to a Powell-Gilbert essay on this temporary sea has been made in an earlier section.

Gilbert attended the meetings of the American Association for the Advancement of Science only on two occasions after 1900; first in Washington in the winter of 1902–3, when he spoke before the geological section on "Physiographic belts in western New York" and on "A viscous rhyolitic eruption," and again at the end of 1903 in St. Louis, when he told of "Domes and dome structures in the Sierra Nevada."

It was a good fortune for the National Geographic Society that Gilbert was its vice president in 1904, as he was therefore available to greet the members of the International Geographical Congress in the name of the society when they assembled in Washington in September of that year. He addressed the congress on the "Sculpture of massive rocks," accompanied its peripatetic passage to Niagara, where he served as local guide and gave an evening address on the evolution of the falls, and then went to St. Louis, where he spoke before the Congress of Arts and Sciences gathered at the World's Fair on "Asymmetry of mountain crests in the Sierra Nevada."

The handful of teachers of geography who founded the Association of American Geographers in 1904 were gratified to include Gilbert among their original members; he was elected their first vice president in 1905 and their president in 1908. The Association was thus so fortunate as to hear from him a presidential address on "Earthquake forecasts," at the meeting in Baltimore, January 1, 1909, which made so favorable an impression that he was asked to repeat it later in the same month before the Geological Society of Washington and the National Geographic Society of the same city. This thrice-presented address, one of the most interesting that Gilbert ever wrote, was the last he delivered in public; it is here analyzed in another section.

It may be noted in this geographical connection that although Gilbert did not often employ certain terms that have come to be more or less generally used by physiographers in the last 40 years, he nevertheless liked the "characterization of the stages of the topographic cycle in terms of the cycle of human life," and when that terminology was assailed he took occasion to defend it.[5] He held that the analogy on which the terminology is based is good because it indicates a "close resemblance in some striking particulars, coupled with difference in other respects"; that being precisely the relation between the topographic and the human cycles. He added:

In my judgment there are few groups of terms which serve better than does this group the purpose of concisely expressing an idea. Its strength inheres, first, in the aptness and completeness of the analogy and, second, in the perfect familiarity of the group of facts to which the unfamiliar facts are likened. . . . The aptness and the familiarity make the terms permanently mnemonic, so that the use of any one oft hem brings to mind not only the sequence, but relative position in the sequence.

Gilbert attended the meeting of the Geological Society of America at Albuquerque in December, 1907, and at Baltimore a year later; it was then that this society elected him for the second time to its presidency, an honor bestowed on no other of its many members. Unhappily his serious illness in the summer of 1909 made it impossible for him to preside at the meeting in Boston and Cambridge the following winter; but he sent to the fellows there gathered a message that was read at the annual dinner. After saying that he found some consolation for his absence in the belief that the lusty vigor of the society "ensures the quick closing of every gap, and that the cherished cause for which our Association stands is imperilled by no default of an individual," he went on as if taking leave of old friends:

As thought roams backward over the long series of our fruitful meetings, I realize as never before how the growth of our science has been interwoven with the growth of our organization; how our individual efficiency and our associate efficiency have each enhanced the other, and how the harmony and solidarity of our geological body have been fostered by the personal contact of its members. As I dwell with longing on all the privileges that this week are ours, I realize more than ever before how strong are the personal ties which bind me to you and how large a measure of real comradeship has come to be implied by the formal title of Fellow. And so, Fellows of the Geological Society, I send more than a cordial—I send an affectionate greeting.

This message appears to have been Gilbert's last direct relation with any scientific society; although he lived eight years longer, it was his farewell to many friends. That the message should have been addressed to the Geological Society of America was most appropriate, for it was through his membership in that body about as much as through his position on the national survey that he came into personal relation with his geological colleagues. Mention has already been made of the great importance of the society in bringing American geologists together in a friendly way and in promoting the progress of their science by personal intercourse. It may be added that in contrast to many earlier gatherings of the geologists in their section of the American Association, those of the Geological Society were relatively peaceful, as if an era of comradeship and friendly discussion were replacing a former era of rivalry and dissension. No one member contributed more to this beneficial change than Gilbert. He seemed never to feel exasperation, much less to show it, however directly his statements were traversed by those of another member. To be sure, his statements were not often thus traversed, because when made they were as a rule seen to be so well based that an opposing opinion could not find ground for its support; but it was by no means only for this reason that his words and his manner were always pacific; it was quite as much because he was by disposition utterly opposed to polemical disputes. Even the few who announced a difference from his views could not let their differing detract from the admiration they felt for him personally, because when he expressed his difference from them he was so fair, so courteous, so impartial, that he elevated controversy into conference. If he found himself by rare chance in the wrong, he promptly and frankly avowed his mistake. If he found others in the wrong, he made little of what he conceived to be their error, but emphasized instead the objective reasons that led him to take another view than theirs. On one occasion, when a competitive, not to say combative, member made report of certain conclusions based in

[5] Style in scientific composition. Science, xxi, 1905, 28-29

part on facts observed during a joint excursion which Gilbert had arranged with him in the hope of reaching an agreement in the field, the comment made by the noncombative one of the two upon what he believed to be an altogether erroneous interpretation of the facts was not phrased in such a way as to throw all the blame on the other side, but rather in such a way as to share it: He remarked that, although the excursion had not brought the two observers into agreement, it had "served to prove that we differ widely as to the criteria by which shore ridges and shore terraces are distinguished from ridges and terraces of other origin."[6] Geology gained immensely from his method and manner as well as from his observations and inferences.

LATEST WORK AND WORDS ON NIAGARA

Gilbert's latest field study and report and his latest written article on Niagara both followed the essays and lectures above analyzed by about 10 years. The field study[7] was undertaken in connection with a new survey of the falls executed in 1905 at Gilbert's instance, and involved a critical comparison of its outline with those of all earlier surveys. The report in which the results of this study are presented calculates a recession of 5.3 feet a year for the central part of the Canadian or Horseshoe falls. This brief numerical value should be quoted as a standard until it is modified by later surveys; but the report in which it is announced has a greater and more enduring value as an illustration of the manner in which a scientific inquiry should be conducted. It deserves to be analyzed as to its method even more than to be quoted as to its result; it is a model of careful, critical, and impartial procedure. It opens with a thorough historical review of the problem under discussion. The relative accuracy of various surveys is nicely evaluated, and a curious error in the famous survey of 1842 is brought to light by means of a most ingenious series of tests. Geology is indeed a curious science inasmuch as it here demands that the position of the verandah of a hotel, of which only the foundations now remain, should be identified in order to erect a scaffolding in the same position from which a photograph of the falls might be taken—official authority and an engine had to be secured to open the desired prospect by removing an obstructive freight car from a near-by railroad siding—and thus duplicate the record of a camera-lucida drawing made in 1827 by Capt. Basil Hall, an English traveler, so as to measure the change in the vertex of the Horseshoe falls with respect to a line of the same bearing in two views. And yet there are persons who think that geology deals chiefly with rocks! A profitable essay might be written on the various mental qualities and personal activities that entered into this simple investigation.

Gilbert's last words on Niagara appeared in a review written in 1908 of a work on the "Evolution of Niagara Falls,"[7a] and considered especially the interpretation there given to the variation in the rate of the falls' retreat in response to the variation in the volume of water that passed over them. In the work in question, the retreat of the falls had been taken as proportional to the volume of falling water; and the age of the falls had been calculated on that basis after estimating the volume to which the river was reduced in the two epochs when it drained only the Erie Basin, and the length of the two parts of the gorge that were eroded in those epochs. Gilbert's view was that the rate of the falls' recession was reduced much more rapidly than in direct proportion to the diminution of river volume, and that the calculated age of the falls was therefore much too small. His reasons for this view were chiefly that, considering the river as an eroding engine, the greater its energy, the greater the share of energy would be applied to its work; but he also pointed out that various other factors would enter the problem. For example, the greater the thickness of the capping limestone, the more abundant would be the fallen blocks which could be used as grinding pestles by a river that was large enough to churn them about, but which would act as obstructions to erosion in a river so small that it could not move them; also, that even with a constant volume of river discharge, the recession of the falls would vary in some manner inversely with their breadth. But the chief factor being variation in volume, his inclination was to assign a much greater age to the falls than the 39,000 years that had been calculated on the assumption of a direct relation between volume and recession.

[6] Bull. Geol. Soc. Amer., iii, 1892, 493.
[7] Rate of recession of Niagara Falls. Bull. 306, U. S. Geol. Survey, 1907.
[7a] Science, xxviii 1908, 148-151.

THE NAMING OF GILBERT GULF

A remarkable sequel to Gilbert's work on the Great Lakes was the later demonstration that for a time after the opening of the St. Lawrence outlet by the retreating ice sheet the valley of that river was still so low that the sea entered it and thus found access to the basin of Lake Ontario, which was therefore temporarily occupied by a salt-water gulf. The gulf was converted into a fresh-water lake only after the region of the St. Lawrence Valley was raised above sea level. It was not unnatural that when the temporary existence of the salt-water body was discovered, the proposal should be made to attach Gilbert's name to it, but he did not altogether approve of the idea; he wrote in 1906 to the chief mover in the proposal:

> You have stolen a march on me in the matter of Gilbert gulf. Hitherto I have managed to stave off things of that sort, but I see no way to escape this. I appreciate of course your kindness and the honor, but have the feeling that there is a bad precedent in the use of names of the living for such purposes.

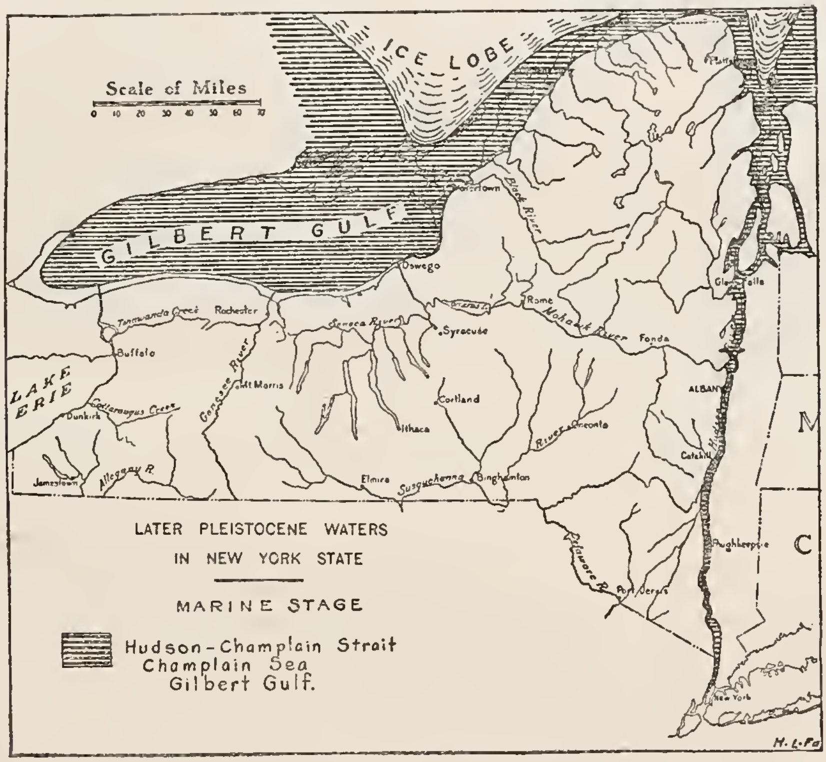


FIG. 18.—Gilbert Gulf, an arm of the ocean temporarily occupying the basin of Lake Ontario; from report by H. L. Fairchild (p. 251).

He expressed the same opinion two years later, when it was proposed to add the name of another living worker to another extinct water body:

> I question, as you know, the general policy of using the names of contemporaries. It grades into toadyism—and then you can't be sure until a man is dead that he wont do something to destroy his reputation, scientific or otherwise. When you named Gilbert gulf, you doubtless tho't that Gilbert had made some contribution to the pleistocene history of the lake region, but his exposure in . . . had not then appeared.

He enjoyed this third-personal manner of referring to himself; and had he been of a combative disposition he would have enjoyed also making a quarrel out of the "exposure" here referred to; but he was not combative, and he never quarreled even under provocation.

CHAPTER XXVIII

NEW FIELDS OF WORK IN CALIFORNIA

STUDIES AND VACATIONS IN THE SIERRA NEVADA, 1903, 1904

Most of the last 15 years of Gilbert's life were divided between residence in Washington and in California. Parts of four summers, 1903, 1904, 1907, and 1908, were given to studies and vacations in the Sierra Nevada, and many months of the years from 1905 to 1917—except for detention in the East by illness in 1909 and 1910—were devoted to the investigation of hydraulic-mining débris, during which numerous visits were made to various mountain districts in the northern half of the State.

Of the first two summers spent in the mountains, one was with an outing party of the Sierra Club of San Francisco on the High Sierra about the head of Kern River and around Mount Whitney, the other with a friend in the Yosemite district. Gilbert was a delightful companion in the mountains and at the camp fire. His quick grasp of the meaning of natural phenomena made him their proper exponent, his vigor and enthusiasm insured him a hearing, and his humor and good fellowship made him always a welcome addition to any group. He pointed out with inspiring animation the many structures of geologic significance and the varied forms of glacial sculpture to his botanical and zoological friends, and in return listened attentively to their accounts of Alpine plants and animals. It must indeed have been a high privilege to make a mountain excursion with so learned a nature lover. It is worth noting that these trips in the Sierra Nevada appear to have been the first on which Gilbert spent a considerable time in mountains of temperate latitudes at a height where the effects of extensive Pleistocene glaciation were observable close at hand, although he had seen from sea level the effects of much more intense glaciation in the mountains of Alaska on the Harriman expedition a few years before. He had indeed many years earlier detected, apparently by long-distance observation, the signs of local glacial erosion on some of the higher ranges in the Great Basin without ascending them, for in his first Wheeler report he wrote regarding the Schell Creek range:

> The crest is remarkably acute, and is buttressed by lateral spurs, between which are close, hopper-shaped valleys, that once contained very small glaciers (III, 88).

He had also ascended the eastern face of the Sierra near Mono Lake when inspecting Russell's work there in 1883; and with the memory of observations then made he mentioned four years later, the occurrence of "high level débouchures of low-grade tributary cañons" over the deep main canyons by which the eastern face of the range is there incised. He added:

> I am disposed to think that ice may have had much to do with the production of the phenomena. It is manifest that a trunk glacier would greatly exceed a tributary in eroding power, and the conditions of ice erosion do not seem to require any equalization of level at the junction of [glacial] streams.

This foreshadowed but did not reach a valid explanation of hanging lateral valleys. His experience in Alaska corrected the misapprehension expressed in the last sentence.

The visits to the Sierra in the summers of 1903 and 1904 enlarged these earlier experiences, and led to the preparation of a number of short essays: "The systematic asymmetry of crest lines in the High Sierra of California," [1] in which the asymmetry was explained by the greater strength of glacial erosion on the shaded or leeward slopes where snow was collected in greatest thickness during the Glacial period; also, "Domes and dome structures of the High Sierra," [2] "Variations of Sierra glaciers," [3] "Crescentic gouges on glaciated surfaces," "Moulin work under glaciers," "Gravitational assemblage in granite," "Terraces of the High Sierra" and ' Lake ramparts." [4] These communications all represent relatively small problems, briefly treated;

[1] Journ. Geol., xii, 1904, 579–588; Sierra Club Bull., v, 1904, 279–286.
[2] Bull. Geol. Soc. Amer., xv, 1904, 28–36; Sierra Club Bull., v, 1904, 211–220.
[3] Sierra Club Bull., v, 1904, 20–25.
[4] Bull. Geol. Soc. Amer., xvii, 1906, 303–316; 317–320; 321–328. Science, xxi, 1906, 822.

but they represent also the skill in observation and the delicacy of discussion in which Gilbert was preeminent. The last article cited may be taken as an excellent example of the orderly sequence in which facts and inferences march by when marshaled by their master; and it has a further value in including an element, the absence of which from nearly all of Gilbert's writings is to be lamented, namely, a persoual reminiscence. It touches the skating adventure in his boyhood, to which reference has already been made in the first chapter of this memoir.

The cracking of lake ice from cold is a familiar occurrence wherever winters are severe. As a boy, in western New York, I skated on a landlocked bay several miles long, and each winter its ice sheet was divided by cracks that ran from side to side. On a still cold night I have heard them form, the ice bursting with a booming sound that slowly died away. I thought then that a crack which started near me required a minute or more to span the bay, but suspect now that its propagation was much swifter than that of the sound waves on which my impression depended. Sometimes the cracks opened so widely that the skaters found pleasant excitement in crossing them by flying leaps; and a crack into which I once fell must have been three or four feet broad. Being then quite innocent of theories, I did not compare its width with the temperature, but the air that day must have been bitterly cold, for my clothes were frozen before I could reach the nearest house.

How unfortunate it is that every worth-while Johnson does not more often serve as his own Boswell.

Gilbert published no comprehensive statement of his views on the origin of the larger forms of the Sierra Nevada, or on the action of glacial erosion in modifying them, but as to the latter subject, he wrote in a personal letter: "To one who is on the ground the evidence of great glacial erosion in the Sierra is quite as clear as the evidence of old river scour in the Onondaga-Syracuse region" of New York; this old-river evidence being that which he had discovered in the channels cut across the plateau-margin spurs by ice streams, as already told in an earlier section. As to the former subject, the brief view of physiographic development in the next-to-last paper above cited shows clearly how rich a fuller statement would have been. The area there treated lay in the higher part of the range between Kern and Tuolumne Rivers, a distance of over 100 miles, where the leading features are—

(1) summit plateaus, characterizing interstream areas and recording a long period, or periods, of degradation soon after the commencement of the Sierra uplift. Many of the peaks overlooking the summit plateaus, especially in the neighborhood of the crest line, have (2) remnant surfaces of moderate slope strongly contrasted with the surrounding cliffs, produced for the most part by glacial erosion. And many of the valleys are bordered by (3) high terraces, in some cases expanded so as to constitute important plateaus. It is believed that the remnants of old topography at high altitudes were in the main once continuous with the summit plateaus, but the correlation is difficult, because the connecting slopes have been destroyed by the excessive development of glacial cirques.

It is "anticipated that the plateaus and terraces of the higher parts of the range will eventually be correlated with similar features near the western base of the range; but the latter have not been studied."

A HOUSE PARTY IN THE SIERRA, 1907

The Sierra excursions of 1903 and 1904 proved so enjoyable that three years later Gilbert resolved upon what he called his "one great extravagance," nothing less than a pleasure party in the mountains with a few chosen friends, who in the spring of 1907 received the following delightful message from him:

You are cordially invited to join my house party in August; my cellar is the Yosemite valley, my drawing room the Tuolumne meadows, my attic Mono pass, and my stairway the Tioga road.

It is not to be wondered at that no refusals came from the fortunate guests; they were Gilbert's sister, Mrs. Loomis, of Jackson, Mich., then in her seventieth year; Prof. and Mrs. J. H. Comstock, nature lovers of Cornell University, whom Gilbert had so often visited during his summer field trips in New York; and Miss Alice Eastwood, botanist of the California Academy of Sciences, with whom acquaintance had begun "before the earthquake" of the preceding year—an intimate group, within which the tall leader bore the name of "Charlemague." The party gathered at "Crocker's," a hotel in the highlands, where their host was waiting with a carefully selected outfit, including 2 packers, a cook, 16 horses, and plentiful supplies. Everything was carefully arranged with due regard to the comfort of the visitors, none of whom were

hardened mountaineers, but all of whom therefore enjoyed the more delightful novelty of well-ordered camp life in the superb scenery of the mountains during a season of assured fine weather.

One of the party has recalled its pleasures. The days were long, as early starts were made, but not strenuous, for the distances covered were moderate and the halts were deliberate. The campers were roused by a shout from the leader in the dawn and breakfast was eaten at or before sunrise; then the horses, driven in from a near-by meadow by one of the centaurlike packers, were lined up along a rope stretched from tree to tree, as if they were a class of children, toeing the mark and waiting for their tasks. The arduous work of saddling and packing over, the party would set out on the morning's ride till a halt was made by a spring or stream for lunch and prolonged while a chapter was read aloud from some book, perhaps one of John Muir's. Then on again to overtake the pack train which had preceded the leisurely riders to a well-selected camping ground for the night, where a hot supper was ready for hearty appetites and the evening camp fire brought all members of the party together for readings and stories, till the hour came to "turn in" for the refreshing sleep of a quiet night in the open; or if perchance a waking half hour intervened, it would permit a drowsy glance upward at the branches of the trees, lighted by flickering flames of the dying fire against the black mountain-arching sky whence the stars shine down with a brightness hardly known to dwellers on lowlands; and then another morning came. Sometimes the waking shout was unusually early, so that all might see in the pale eastern sky a comet that seemed to have been summoned to add to the glory of the dawns in that wonderful August.

Day by day the party would set out through grassy meadows tinted with wild flowers, across hurrying streams or around the shore of a placid mountain lake, past glades of aspen on moist ground near the waterways, up through the forests of pines, spruce, and firs, some of them giants 200 or 300 feet in height, and over the rock-floored highlands where broad views were gained of a chaos of peaks, barren and snow-patched; for such is the course of the Tioga Road, originally constructed to reach a mine near the mountain crest, christened with a name far away from its aboriginal habitat, and much appreciated by mountain parties long after the mine to which it led ceased to be worked; and thus Mono Pass was reached, where a marvelous prospect over the desert country far below to the east was disclosed. Some of the easier mountains were ascended to enjoy their views of the higher and more difficult summits; and there, as everywhere, the leader who loved every aspect of nature served as its interpreter. He knew all the rocks, mountains, and canyons, all the flowers and trees, all the birds and beasts of the highland country; and yet in spite of the many rough experiences of his outdoor western life, he still retained so refined a sympathy and so undulled an imagination that he cared for neither hunting nor fishing, because of the pain that such sports inflict upon their victims. The month was spent in ideal companionship, the memories of which are prized by the invited members of the party, all of whom still survive; but their most treasured memory is of the happiness felt and shown by "Charlemagne," their host.

SCIENTIFIC HOSPITALITY IN THE SIERRA, 1908

Hardly less remarkable was another Sierra excursion the following year, 1908, when Gilbert had for his guests his old topographic associate, W. D. Johnson, and the geologist, E. C. Andrews, of Sydney, Australia. The two excursions were alike in being holiday outings, but otherwise they were very different. The party of 1907 consisted of intimate friends, among whom scientific problems ranked lower than personal relations. The party of 1908 was essentially scientific, and the Australian member had never been seen by the two Americans. Their coming together was a fine illustration of the world-wide comradeship that is developed among men of similar pursuits; the bond that united the three in this case being a common interest in "the magnificent illustrations of deep glacial erosion" which the High Sierra affords. It seems that Andrews had a few years before published a brief but forcible account of the mountainous coast of southwestern New Zealand, the deep fiord troughs of which he ascribed to glacial erosion, not because he had previously studied the action of glaciers, but because the forms of the fiord troughs were so unlike those of the valleys of normal erosion that he had

seen in New South Wales. His paper made a strong impression on those who received it in this country, and one of them suggested to Gilbert that he might well send a copy of his report on the "Glaciers of Alaska" to the antipodal geologist; but as this suggestion was received before he had himself seen Andrews's paper he demurred, remarking that as each presentation copy of the Alaskan report cost him $3 he could not afford to distribute it freely; but on seeing the paper and its excellent photographs, the like of which he had vainly tried to secure in Alaska, he sent the volume promptly and gladly; and with it a letter from which the following significant extracts are taken:

> I am also interested in a psychological point you mention. Your sensitiveness to the topographic peculiarities of Southern New Zealand was due to previous training in non-glacial regions. I myself had a similar experience except that the sequence of events was inverse. My youth was passed and my early geologic studies were made in a glaciated region. . . . I think it quite possible that the English geologists who frame such curious arguments against the actuality of ice sculpture have never seen anything else, and are therefore not sensitive to the contrasts between the two types of sculpture.

It was on the basis of an acquaintance thus begun that, on the occasion of Andrews's trans-American journey to England in 1908, he was welcomed by Gilbert and Johnson in San Francisco and invited to spend a couple of months camping in the mountains where various problems of glacial sculpture are so finely displayed.

It is not to be questioned that glacial problems were actively discussed by the keen-eyed trio, but the account of the excursion given by the stranger [5] has more to say of personal than of scientific matters, for it appears that upon a vacation trip such as this, Gilbert's interests, like those of his companions, were not restricted to geology or physiography. He pointed out all manner of plants and animals, from dwarfed willows less than an inch high near Mono Pass, to a spider that made a web in the shape of a paraboloid of revolution in the district of the big trees. During progress through the highland forest he challenged his companions to cap verses; at rests he would give and ask for mathematical problems; in the night he pointed out the constellations; his mind was always active. The visitor was entertained by his companions with readings from American authors and with reminiscences of American geologists: Gilbert told of a trip when Shaler and he stopped at a country tavern where a bowl and a single roller towel did duty for all comers in preparation for the noonday meal. "Shaler," Gilbert said, "was fond of dichotomous classification: he said to me—'There are two kinds of men, those who wipe on the outside of the towel and those who wipe on the inside; I belong to the smaller group.'" Truly, it was a liberal education for the Australian to travel over the mountains in such company, an education which many an American must envy him.

EARTHQUAKES AND FAULTS

Gilbert was in California at the time of the San Francisco earthquake of 1906. What he saw there must have brought to his mind the views regarding the relation of faults and earthquakes which he had announced a quarter of a century earlier in an article first published in the Salt Lake Tribune for September 20, 1883, and reprinted in the American Journal of Science a year later. He then described the little fault scarps of unknown though recent date in the alluvium at the western base of the Wasatch Mountains in Utah, and compared them to similiar scarps produced in 1872 by the Owens Valley earthquake at the eastern base of the Sierra Nevada, where long gathering strains had caused an old fault to snap and slip again. He added that the few inhabitants of that arid region noticed that the adobe houses near the earthquake scarp were destroyed, while the frame houses survived the shock; hence the destroyed houses were rebuilt in wood; but he regarded this precaution as unnecessary, because after a shock "the accumulated earthquake force is for the present spent, and many generations will probably pass before it manifests itself again." On the other hand, "any locality on the fault line of a large mountain range, which has been exempt from earthquake for a long time, is by so much nearer to the date of recurrence"; hence in Utah, those parts of the Wasatch base where the alluvial scarp is low or wanting were regarded as peculiarly liable

[5] E. C. Andrews. Grove Karl Gilbert, Sierra Club Bulletin, xi, 1910, 60–69.

to shocks in the future, although no one can say how soon because the rate at which a mountain range grows is unknown. The article concluded with some salutary but unpalatable suggestions:

By the time experience has taught us this [the rate of Wasatch growth], Salt Lake City will have been shaken down, and its surviving citizens will have sorrowfully rebuilt it of wood. . . . What are the citizens going to do about it? Probably nothing. They are not likely to abandon brick and stone and adobe, and build all new houses of wood. If they did, they would put themselves at the mercy of fire; and fire, in the long run, unquestionably destroys more property than earthquakes. It is the loss of life that renders earthquakes so terrible. Possibly some combination of materials will afford security against both dangers.

The Salt Lake citizens did not like science in that form; it was too disturbing. Whatever the eventual danger from earthquake might be, no one could tell how long a time would pass before it would come; and in any case the removal or the remodeling of the city was out of the question. Its situation in other respects was so favorable that, as to earthquakes, the citizens naturally would take the chance and run the risk, for the chance might be remote rather than imminent, and the risk might be small instead of great. Time enough when it comes. If anyone had written a similar article, warning the citizens of San Francisco of their danger because of the existence of a fault rift not far away, they also would have taken little heed of the warning; it is inevitable that a city, a great city, must stand there. But after the earthquake took place and the local earth strain was relieved, the San Franciscans may well have taken comfort in Gilbert's view that a city near a fault is likely, after a slip has taken place on it, to be safe from further disturbance for some time to come; but whether "for many generations," who can say! On the other hand, Gilbert himself must have been somewhat dismayed to see that both the dangers which he foresaw for Salt Lake City had combined in a double disaster for the city at the Golden Gate.

THE SAN FRANCISCO EARTHQUAKE

Gilbert's unusual capacity for the popular exposition of scientific subjects has already been alluded to. It is well exemplified in a general account of the San Francisco earthquake [6] which he prepared shortly after its occurrence, the first paragraph of which is written in a singularly objective style.

It is the natural and legitimate ambition of a properly constituted geologist to see a glacier, witness an eruption, and feel an earthquake. The glacier is always ready, awaiting his visit; the eruption has a course to run, and alacrity only is needed to watch its more important phases; but the earthquake, unheralded and brief, may elude him through his entire lifetime. It had been my fortune to experience only a single weak tremor, and I had, moreover, been tantalized by narrowly missing the great Inyo earthquake of 1872 [in southeastern California] and the Alaska earthquake of 1899. When, therefore, I was awakened in Berkeley on the eighteenth of April last by a tumult of motions and noises, it was with unalloyed pleasure that I became aware that a vigorous earthquake was in progress. The creaking of the building, which has a heavy frame of redwood, and the rattling of various articles of furniture so occupied my attention that I did not fully differentiate the noises peculiar to the earthquake itself. The motions I was able to analyse more successfully, perceiving that, while they had many directions, the dominant factor was a swaying in the north-south direction, which caused me to roll slightly as I lay with my head toward the east. Afterward I found a suspended electric lamp swinging in the north-south direction, and observed that water had been splashed southward from a pitcher. These notes of direction were of little value, however, except as showing control by the structure of the building, for in another part of the same building the east-west motion was dominant.

In order to make the most of this rare opportunity, he left the fire to the firemen, the injured to the doctors, the homeless to the charitable, and devoted himself as a geologist to the geological phenomena, especially the displacements along the trace of the old fault plane on which the earthquake originated. After giving an account of many striking instances in which the horizontal displacement of the earthquake is apparent, he expresses the hope that studies should be made by engineers and architects regarding the safer construction of buildings, so that—

the new city should be earthquake proof. . . . Timidity will cause some to remove from the shaken district and will deter others who were contemplating immigration, but such considerations have only temporary in-

[6] An investigation of the San Francisco earthquake. Pop. Sci. Monthly, lxix, 1906, 97-115.

fluence, and can not check in an important way the growth of the city. The destiny of San Francisco depends on the capacity and security of its harbor, on the wealth of the country behind it, and on its geographical relations to the commerce of the Pacific. Whatever the earthquake danger may be, it is a thing to be dealt with on the ground by skillful engineering, not avoided by flight; and the proper basis for all protective measures is the fullest possible information as to the extent and character of the danger.

This wise counsel is repeated at the close of an address on "Earthquake forecasts," which is analyzed below.

GILBERT'S CONCEPTION OF AN EARTHQUAKE

Gilbert was appointed by the Governor of California as a member of a State commission to investigate the effects and causes of the San Francisco disaster. He also took part in a study for the United States Geological Survey; and his chief report was published in its Bulletin 324 in 1907. He there presented a brief statement of his conception of the "motions constituting the earthquake in the region of its destructive intensity," from which the following passages are taken:

The San Francisco earthquake had its origin, wholly or chiefly, in a new slipping on the plane of an old fault [The San Andreas fault, passing a little to the west of the city]. The trend of the fault is northwest and southeast, and it . . . has a length of 300 miles, possibly more. Nothing is known of its depth. . . . The fact of recurrence on the same plane shows that the rock faces in contact had not become welded, so that the molecular force which there resisted motion was less than the cohesion of solid rock, and may have been little stronger than adhesion. A tract of the crust including the fault plane had come to be affected by a system of slowly increasing shearing strains, and the associated stresses were the forces directly causing the fault. When the stress component coincident with the fault plane at some point became greater than the adhesion (or cohesion) a local slipping took place. This caused a redistribution of strains and stresses, the local relief of strain being followed by an increase of strain and stress in all adjoining tracts of the fault plane, with the result that the adhesion was overcome in those tracts and the area of incipient faulting thereby enlarged. Thus from the initial tract the lesion was propagated as a sort of wave through all the fault plane.

At the initial tract a small movement sufficed to relieve the local strain, and the motion was then arrested by friction, but the movement was renewed by reaction from other tracts, and it alternately started and stopped till the accumulated stresses had spent themselves. There was a similar rhythmic sequence in other parts of the fault, the frequency of the alternations depending on local conditions; and the total movement of dislocation at each point was accomplished by a series of steps and not by a single leap. The time consumed in these reactions was not infinitesimal. The rate of propagation of changes in strain was the same order of magnitude as that of earthquake waves in general, and the rate of propagation of initiation of movement on the old fault plane may have been somewhat slower because of the necessity of accumulating a certain amount of stress increment to overcome the adhesion. It is probable that the completion of the fault required more than one minute, and it may have required more than two minutes. It is even possible that the displacement had been completed at the initial point before it began at the most remote.

In the succession of slippings and stoppings at any point of the fault plane each separate slip communicated a jar or pulse to the surrounding rock, and this pulse was propagated in all directions. The earthquake at any locality in the neighborhood of the fault consisted of such pulses from different directions. The general distribution of intensity indicates that the pulses weakened in transmission somewhat rapidly, whence it may be inferred that the particular pulses constituting the dominant elements of the local earthquake were those from the nearer parts of the fault. If this conception of the earthquake is correct, the rhythm observed in the region of high intensity was a phenomenon distinct from the rhythm of harmonic waves. It was essentially a frictional rhythm, dependent on the relation of certain rock strains and rock stresses to the resistances afforded by adhesion and sliding friction. It was irregular not only because the intervals of local starting and stopping were unequal, but because it was derived from a considerable area of the fault surface, in which the local rhythms were neither harmonious nor synchronous.

The compounding of unevenly spaced pulses from different points of the fault plane caused both reenforcement and interference, introducing a character analogous to beats in music, but without the regularity of musical beats. It also at times made oscillatory motions swifter in one direction than the other, so that reciprocal accelerations were not always symmetrically arranged. In less technical language, the motion was jerky and included abrupt phases that were almost blows. The compounding also introduced variety in the direction of motion, especially at the end, when for a short time the pulses from remoter parts of the zone of origin ceased to be overpowered by those from the nearer parts. The motion in that closing phase of the violent part of the earthquake has been compared by an observer to the motion of a vessel in a choppy sea; and I conceive that this comparison is the expression of a veritable analogy.

Few readers of the present memoir are likely to be so well informed as to the nature of earthquakes as not to profit by this analytical summary.

The horizontal displacement of the earth's surface on the trace of the San Francisco fault plane has been frequently described; the occurrence of wavelike undulations in low-tide mud flats at the time of the earthquake and their preservation for a considerable time afterwards may be less familiar. Concerning this curious effect Gilbert wrote as follows:

> Observers of strong earthquakes sometimes report visible progressive undulations of the ground, similar to water waves, and such observations are usually made where the formations are alluvial . . . Not only is there a gradation in physical condition from dry earth through mud to water, but the shaking of a loose formation, whether wet or dry, overcomes the adhesion of particles and thereby imparts for the time being a mobility analogous to that of liquids. It is therefore conceivable that gravity waves, altogether analogous to those of water, may be produced by a violent earthquake in the surface of a loose formation. Certain ridges on soft ground caused by earthquakes in Japan have been inferred by Omori and Kikuchi to represent such soil waves and to indicate a wave length (crest to crest) of 20 to 40 meters. The San Francisco earthquake produced a similar ridging on tidal mud in Tomales Bay [on the outer coast, 50 miles north of the Golden Gate], the average ridge interval being not more than half that of the Japanese examples . . . There is no reason to question the statement that the whole mud plain had the smooth surface common to tidal flats until it was disturbed by the earthquake. Nor do I find any room for doubt either that the ridges originated as waves on the surface of the mud while it was rendered quasi liquid by violent agitation, or that they persisted because the mud promptly resumed its normal coherence when the agitation ceased. It is by no means equally clear that the arrested waves were true gravity waves rolling across the mud plain. Whatever their mechanism and history, they illustrate a mode of response of wet, unconsolidated material to powerful earth tremors, they suggest an explanation of certain wavelike ridges produced on areas of made ground in San Francisco, and they contribute to an understanding of the peculiar destructiveness of the earthquake in such areas.

As to the important question of accumulating crustal strain and its expression in the deformation of the surface near the trace of a fault rift before relief in an earthquake shock, Gilbert wrote briefly in a review[7] of the State report; he regarded the "lack of chronologic unity in the trigonometrical surveys, which were strung along through several decades" in the the San Francisco region as—

> peculiarly unfortunate. . . . This fact made it impossible to discriminate between deformation at the time of rupture and progressive deformation during accumulation of strain before rupture. . . . Nevertheless the results invite the careful attention of geophysicists. To the reviewer the distribution of dislocation, and especially the existence close to the fault, on each side, of a belt of maximum distortion, seems clearly not that which would obtain if the fault passed completely through a solid crust to a liquid substratum. And it also appears that, on the assumption of continuous solidity from the surface downward, the geodetic results might yield to adequate analytic treatment a conception of the order of magnitude of the vertical distance to which the fault penetrated.

The grounds for this opinion are not stated, and the method of adequate analysis is not outlined; both doubtless were conceived in Gilbert's mind, and if so the layman must regard the depth of his penetration as profound as that of any earthquake fault plane.

EARTHQUAKE FORECASTS

The Association of American Geographers elected Gilbert as their president for the year 1908, and thus had the pleasure of hearing an address from him at their meeting in Baltimore on January 1, 1909. It was the last address of the kind that he ever gave; for although the Geological Society of America elected him for the second time as their president in 1909, he was unable to attend their meeting at the end of that year. The subject of the address before the geographers was "Earthquake forecasts," and in substance and manner it is one of the finest if not the finest that he ever delivered. Its preparation must have demanded a great amount of study, for it contains not only an ingenuous discussion of many factors determining the place and time of earthquakes, but also much specific information in a field that he had not previously entered. Its treatment is marked by the deliberate and penetrating analysis which characterized all his work; the conclusions reached and the moral drawn from them are set forth with a tempered wisdom that even in his own earlier addresses was seldom matched. The style is genial, at times almost conversational; and interest is maintained to the very end. Some of the most striking passages are here quoted, the opening one first:

> The outlook for earthquake forecasting is my theme today. As you are aware I am not a seismologist. My point of view is that of the geologist and general geographer. I speak as a layman, and present impres-

[7] Amer Journ. Sci., xxvii, 1909, 48-52.

sions acquired chiefly during somewhat amateurish work on the physical history of the San Francisco earthquake. That event was so far unseen that no seismologists were at hand, and the duty of investigation fell, in the emergency, on a corps of geologists and astronomers. . . . But while this much is offered by way of explanation and to prevent misunderstanding, you are not to infer that an apology is made because I trespass on fields to which I have not title, for I am an advocate of the principle of scientific trespass.

No fenced-in scientific preserves for him! He then points out the difficulties in the way earthquake forecasting in both time and place:

If the geologist Whitney, in warning San Franciscans forty years ago that their city would suffer by earthquake, had been able to specify the year 1906, and to convince them that he had warrant for his prophecy, the shock, when it came, would have been a phenomenon only and not a catastrophe. If any of those mysterious oracles who were said to have predicted earth convulsions in 1906 had named San Francisco, and told their reasons, the course of history might have been different.

An elaborate discussion follows concerning the conditions under which the place and time of earthquakes may be defined and predicted, after which comes a concise summary: As to place of earthquake occurrence "malloseismic districts [districts "likely to be visited several times in a century by earthquakes of destructive violence"] will eventually be subdivided with confidence by means of geologic criteria." This work has been begun already, is now progressing, and will probably be far advanced in the near future. But as to time of occurrence the problem is vastly more difficult, and is not likely to be solved for an indefinite period. Of these two tasks, "the one lies largely within the domain of accomplishment; the other still lingers in that of endeavor and hope." However, "we may congratulate ourselves that it is not the place factor which lags behind, for knowledge of place has far more practical value than knowledge of time." Indeed, if prediction of probable time were made, it would follow that "possibly for many months, expectation would be tense, and the cost in anticipatory terror would be great." If, on the other hand, the place of peril come to be known even though the dates are indefinite, wise construction of buildings will provide "all necessary precautions, and the earthquake-proof house will not only insure itself but will practically insure its inmates."

The moral of this is drawn in a strong condemnation of the mistaken policy of concealment prevalent in San Francisco after the shock, a policy based on the "fear that if the ground of California has a reputation for instability, the flow of immigration will be checked, capital will go elsewhere, and business activity will be impaired." It was under that policy that a scientific report on the earthquake of 1868, due to a slip on another fault not far to the east of San Francisco, was suppressed. But the policy is vain because it does not conceal. It merely "reflects a low standard of commercial morality, which is being rapidly superseded." Moreover, this policy and the fear that dictates it are both based on a great exaggeration of the actual danger. Risk of death by earthquakes in California is only one-tenth of the risk of death from measles; and the annual premium for an insurance of $1,000 on death by earthquake should be only a cent and a half, plus the cost of its share of the insurance business and its contribution to the business profits. Indeed, "if a timid Californian should migrate in order to escape peril by earthquake, he would incur during his journey a peril at least two hundred times as great, whether he travel by steamship, sailing vessel, railway car, motor car, stage, private carriage, or saddle."

EARTHQUAKES IN ALASKA

In reviewing Gilbert's published essays it is important not to omit the short ones, for they not infrequently contain admirable passages. A case in point is the one-page preface which he contributed to Tarr and Martin's report on the "Earthquakes at Yakutat Bay, Alaska, in September, 1899."[8] Here he set forth certain essential matters in a truly sagacious way, as the following extracts will show.

In its relation to man an earthquake is a cause. In its relation to the earth it is chiefly of an incidental effect. It is the jar occasioned by a sudden faulting, and the faulting is a minor expression of deformation.

[8] Prof. Paper 69, U. S Geol. Surv., 1913.

. . . The determination of surface deformation in connection with this [Alaskan] earthquake . . . is . . . exceptionally full and exceptionally valuable. Measurements of surface displacement are numerous, nearly all of them are referred to sea level and are thus absolute instead of merely differential, and the coast line is locally so intricate that the field of exact observation is areal instead of linear. The new configuration of the surface is compared with the old through an area of approximately 1000 square miles, and the deformation is shown to include not only faulting, with associated uplift and downthrow, but tilting and warping of a complicated character. In the dominance of vertical displacement the tectonic characters of the Yakutat region are strongly contrasted with those of the California earthquake district, where horizontal motion dominates.

RESIDENCE AND INVESTIGATIONS AT BERKELEY, CALIF.

Gilbert's first two summer vacations in the Sierra Nevada were followed by his assignment on an investigation in California up to which a curious sequence of events had slowly led, and in consequence of which a large share of his time for the last 12 years of his life was spent in the Golden State. The sequence of events began, as narrated in his report, in the fifties at the time of the first placer gold washing in the Sierra Nevada; the next step was the introduction of high-pressure washing, or hydraulic mining as it was called, as a result of which the "tailings" of the auriferous gravels on the uplands of the Sierran slope were swept in huge volumes into the narrow valleys; then in 1862 came a great flood which carried the tailings in vast quantity out from the valleys and spread them on the broad fluviatile plain known as the valley of California, to the injury and alarm of holders of riparian lands; the protests and lawsuits then instituted culminated in 1884 in a series of injunctions whereby the miners were restrained from washing the tailings into the streams. This was followed by the creation through act of Congress in 1893 of a permanent board, known as the California Débris Commission, under which hydraulic mining was prohibited except when licensed after approval of arrangements for impounding the tailings; mining was thereby so greatly reduced that in 1904 the California Miners' Association made the final step in the long sequence by memorializing the President of the United States and asking that the National Geological Survey should "undertake a particular study of those portions of the Sacramento and San Joaquin Valleys [the northern and southern parts of the great fluviatile plain] affected by detritus from torrential streams" in the mountains. In addition to much other work thereupon instituted under the survey, including a detailed topographic survey of a large area on the fluviatile plain, Gilbert, who had long before qualitatively analyzed the transportation of débris in the third part of his Henry Mountains report, was appointed to study in a quantitative way by field observation and laboratory experimentation that part of the California problem which involved débris transportation.

The greater part of the field work for this investigation was done in the years from 1905 to 1908. It began with observations on the uplands and in the valleys of the Sierra Nevada, and it was carried across the broad fluviatile plain into San Francisco Bay, where it was eventually extended to the tidal bar outside of the Golden Gate; but after some progress had been made in writing a report, the need of laboratory work was felt so seriously that the manuscript was set aside until the results of experimentation could be secured. An hydraulic laboratory was therefore established at Berkeley, where Gilbert spent several years as a guest of the State university and where his presence was much enjoyed. At first, however, he had not been so comfortably situated. He wrote to a friend in July, 1905: "I've been two months in the State with headquarters at Sacramento, which place I find so dull that I've downslidden to billiards in a public billiard room. . . . If I spend the winter here, the base will probably be shifted to Berkeley—where there are people I like to know"; so to Berkeley he moved the following November, and for a good number of years afterwards made his home in the hospitable Faculty Club on the university grounds whenever he was in California. He wrote later of his life there:

> I've a very comfortable room in the Club . . . and I'm enjoying the Club life—tho the typical Club-man would find it dull.

In return the club members enjoyed his presence greatly; when he was not absent on his travels up and down the State, he had more invitations to the houses of his Berkeley friends than he could accept. No need there to resort to his "knitting" as an evening occupation, as he had done in the commercial metropolis of the country.

One whose acquaintance with him extended over many years of his sojourn in California as well as in Washington, writes:

I never heard from him one arrogant opinion or the least indulgence in petty personalities. . . . Whatever the issue he brought to it the qualities and the temper which we have learned to connect with the man of science at his best—the entirely open mind, with detachment and caution, curious but not expecting overmuch, and shy of enthusiasms. . . . It was perhaps because of his admirable reticence that conversation with him had its charm and its authority.

In spite of his high scientific rank his bearing was marked by a certain humility, and his remarks were characterized by a mental honesty that gained for him a deep and genuine respect. His manner was always serene, his intercourse pleasantly familiar without being too free. He gave many of his recreation hours to games, especially to billiards and dominoes; he still preserved a lively sense of fun, and could be jovial as well as serious. But in this period he seemed to avoid the discussion of geological problems, as if his brain were sufficiently exercised in that direction by his own work; indeed he took care not to concentrate his mind too much upon his own subjects and thus kept himself as a rule in remarkably good health; but he enjoyed talking with his associates on many subjects other than his own, and especially on social questions, regarding which he expressed regret that his interest had not been aroused until late in life.

Little wonder that the friendships created in the Faculty Club were warm and enduring, or that its members should join in writing some years later one of the many letters of congratulation, on the seventy-fifth birthday that he so nearly reached:

You are an integral part of the Club. You were our first honorary member. Your photograph hangs on our walls. Your room is always waiting for you. When your duties call you away we miss your genial company, and we look forward to the time when you will rejoin us.

Although not a sportsman he had a favorite fish story, which he would occasionally tell in condensed form when it seemed to him that others were using the old English weapon too freely.

A man once caught an unusually fine trout, carried him home alive and kept him in a pool; fed him by hand till he was tame; then lifted him out of water daily for longer and longer periods. The trout seemed to like it, and learned to follow his master by wriggling along on the ground. But [this with pathos] he came to a sad end: tried one day to follow his master on a narrow board over a creek; wriggled too much, fell off the board and was drowned.

The report on his hydraulic experiments conducted at Berkeley was published by the national survey in 1914, under the title of "The transportation of débris by running water," as Professional Paper No. 86; it preceded the report on the field work, which appeared in 1917, as "Hydraulic-mining débris in the Sierra Nevada," in Professional Paper No. 105. In the meantime an inquiry into the structural relations of the California earthquake of 1906 diverted his attention for a time, and a serious illness in 1909 interrupted and for a year or more threatened the completion of his débris studies. After a section on his illness, return will be made to the two professional papers in the next chapter.

ILLNESS OF 1909–10

While Gilbert was at Berkeley in the spring of 1909 he became seriously ill. Brief notes of his failing condition were entered in his diary. March 13: "A billiard game with . . . interrupted by head trouble." March 29: "Appreciable muscular change in left hand." April 1: "Numbness in left leg." In the meantime he had consulted a physician, and on March 30 he wrote, in spite of his somewhat alarming symptoms, a singularly calm and dispassionate statement of his case to an official of the national survey in Washington:

It is proper that you should be informed of a new factor which may affect my work in important ways. For three weeks my health has been impaired in such a way as to lower my capacity for work of any kind. While my physician's tone is optimistic, my own impression is that my general physical condition is permanently lowered and that any later change will not be in the way of improvement. Whether or not my fears are well grounded, the situation seems to demand that I limit my attention to one or two of the many subjects that interest and tempt me. I plan to write a report on the débris problem this summer. . . . The one other matter that I do not relinquish is the preparation of an address to be read to the Geological Society of America next winter.

But these plans were not realized. Three days later he wrote again:

Since I wrote you, some of my symptoms have developt far enough to warrant my physician in a diagnosis—a brain trouble of apoplectic character. So there is warning of more serious trouble—sometime, and while I *may* still do a lot of work I can't depend on it. . . . It is hard to relinquish my varied interests, but there seems no other way.

On April 4 he wrote, less formally, to one of his sons:

You will be surprised to learn that I am quitting California, and don't expect to return. I have had disquieting symptoms for some weeks, and the doctor has at last diagnosed the trouble as apoplectic—my general appearance to the contrary notwithstanding. Now an apoplectic tendency means a collapse sooner or later, and I should hate to have it occur away from kin and old friends, and so I am going east. . . . Fortunately my work redily lets me go. The laboratory I drop altogether. . . . The débris investigation has reacht such a stage that it can go to report without material loss. I have pland more field work but it is not essential. So I shall devote myself to the report, and I am sloughing off all other undertakings so as to complete that if possible. I am not visibly ill, but tire easily and have lerned to rest frequently. Have very little discomfort and am still enjoying life. Find it a grind to have to think so much about myself as I have recently.

In spite of the comfortable quarters that Gilbert enjoyed at Berkeley, the last quoted letter shows that he did not feel altogether at home there. On April 11, accompanied by E. C. Murphy, his assistant in the hydraulic laboratory, he took the "Overland Limited" and reached his sister's home at Jackson, Mich., on April 14. There he stayed through the summer. A few days after his arrival a member of the national survey who had been for a time associated with some of his work in California came from Washington at his request and took notes of various matters which the invalid wished to place on record so that, should he be unable to complete his study, what he had done might not be wholly lost. His strength was so reduced that after talking for 15 or 20 minutes he felt exhausted and had to rest for an hour or more; yet by dint of returning to the task several times a day for over a week, about 100 pages of notes were written down. His control of mind and memory at this time of distress was remarkable. He never uttered a word of complaint, but addressed himself as a matter of duty to preserving the work he had done. Although living from day to day under a great shadow, his serenity was unruffled and his clearness of vision undimmed. At a low ebb physically, he still exhibited the extraordinary self-control and the unselfish philosophy that his life had for years before so beautifully exemplified. At each successive interview the subject was taken up where it had been dropped before, without repetition of matter or interruption of logical sequence. The notes thus came to contain a consecutive account of his program of work; but fortunately his subsequent recovery made it unnecessary to call upon them.

So serious had his condition become after reaching Jackson that an endeared friend, as well as his elder son, made long journeys to spend successive weeks with him in the second half of April; but then, instead of failing altogether as he had feared, his health slowly improved. On June 17 he replied to a correspondent's inquiry:

At present it does not seem probable that I shall be able to attend the scientific meetings next winter. . . . I fear you will find my interests in your future projects passiv rather than activ. . . . I am, however, accomplishing a little at the desk. Having whittled down my responsibilities to a narrow point—namely, the preparation of a report on my California work—I am able to make sufficient progress to keep cheerful.

After a temporary setback in the following September, he went to Washington to spend the winter in the Merriam household as in previous years, and there, with habits of industry gained during a life steadily devoted to work, he had the hard experience of laying aside his usual activities and waiting as patiently as possible until they could be resumed. He was still discouraged at times lest he should never again be able to take up his unfinished work, but showed great patience and fortitude in observing all rules for the regaining of his health and the restoration of his normal powers. His recovery depended on absolute fidelity to a rigid schedule, day by day; every act was given its proper measure, and he held to his time-table like the stoic that he was. His long-time friends, Prof. and Mrs. J. H. Comstock, of Cornell University, with whom he had spent a part of many vacations since 1884, were his hosts at their summer cottage, the "Hermitage," on Cayuga Lake, through the warmer months of 1910. It was there that, between the light tasks in simple housework which he shared with his hosts, he first began to write again and prepared outlines of his débris report as if to test his strength; he found encouragement in his success, but the outlines were all written over again a year later.

CHAPTER XXIX

REPORTS ON HYDRAULIC MINING DÉBRIS

TRANSPORTATION OF DETRITUS

The preface to Professional Paper No. 85 explains the relation of Gilbert's experimental work in California to his earlier studies by an allusion to the third part of the Henry Mountains report:

Thirty-five years ago the writer made a study of the work of streams in shaping the face of the land. The study included a qualitative and partly deductive investigation of the laws of transportation of débris by running water; and the limitations of such methods inspired a desire for quantitative data, such as could be obtained only by experimentation with determinate conditions. The gratification of this desire was long deferred but opportunity for experimentation finally came in connection with an investigation of problems occasioned by the overloading of certain California rivers with waste from hydraulic mines.

A primary purpose of the investigation was then stated to be the determination of the relation which the load that is swept along the beds of actual rivers bears to various important factors, such as river volume, velocity, slope, etc., but the complications of the natural problem were found to be such that this purpose was not attained. A secondary purpose was to study the mode of propulsion of the load and the laws connecting the total load with each of the chief factors taken separately; and in this direction a greater success was achieved.

The experiments, conducted at Berkeley chiefly by E. C. Murphy under Gilbert's direction, were made in horizontal troughs, 31.5 and 150 feet long, respectively, through which a controlled current of water flowed. Sand of measured fineness was fed into the current at a controlled rate until the slope of the sanded trough bed automatically came to be just sufficient to enable a specified current to transport a specified quantity of a specified kind of sand supplied to it. By varying the different factors involved many combinations resulted, of which 130 were especially studied, an average of 10 determinations being made for each combination; thus quantitative relations were obtained between width, depth, volume, slope, and velocity of current, and texture and quantity of load. The problem was one in which Gilbert's exceptional powers of mental and mathematical analysis were called into play, as all the experimental data were submitted to computation in a critical fashion. As a measure of the complexity of the computations, it may be noted that over 80 letter symbols were employed in the mathematical expression of the relations studied. A characteristic indication of the nature of the considerations which entered the problems treated is given in the following extracts from the chapter on the relation of transportation capacity to "form ratio": or ratio of stream depth to width.

When identical discharges are passed through troughs of different width and are loaded with débris of the same grade [texture] and the loads are adjusted so as to establish the same slope, it is usually found, not only that the capacity varies with the width, but that some intermediate width determines a greater capacity than do the extreme widths. That is, the curve of capacity in relation to width exhibits a maximum. . . . The explanation of the maximum, so far as its main elements are concerned, is not difficult. . . . Conceive a stream of constant discharge and flowing down a constant slope but of variable width. The field of traction is determined by the width, and the evident tendency of this factor is to make the capacity increase as the width increases. The rate of traction for each unit of width is determined by the bed velocity in that unit and the bed velocity is intimately associated with the mean velocity. Velocity varies directly with depth, and, inasmuch as increase of width causes (in a stream of constant discharge) decrease of depth, the tendency of this factor is to make capacity decrease as width increases. Velocity is also affected by lateral resistance, the retarding influence of the side walls of the channel. The retardation is greater as the wall surface is greater, therefore as the depth is greater, and therefore as the width is less. As capacity varies inversely with the retardation, and the retardation varies inversely with width, it follows that the tendency of this factor is to make capacity increase as width increases. Thus the influence of width on capacity is threefold: Its increase (1) enlarges capacity by broadening the field of traction, (2) reduces capacity by reducing depth, and (3) enlarges capacity by reducing the field of the side-wall resistance. Now, without inquiring as to the laws which affect the several factors, it is evident that when the width is greatly increased a condition is inevitably reached in which the

depth is so small that the velocity is no longer competent and the capacity is nil. It is equally evident that when the width is gradually and greatly reduced the field of traction must become so narrow that the capacity is very small, and eventually the current must be so retarded by side-wall friction that its bed velocity is no longer competent and capacity is nil. For all widths between these limits capacity exists, and somewhere between them it attains a maximum.

A perusal of the above passage, in which a branching concept is pursued along its diverging paths, must make it clear why Gilbert was a good chess player. A later passage touches a practical application of the same special phase of the total problem: "The ratio of depth to width which gives to a stream its greatest capacity for traction is of importance to the engineer whenever he has occasion to control the movement of débris," but unfortunately "no way has been found to extend the quantitative results to rivers. It can hardly be questioned that the optimum ratio [of depth to width] for rivers varies inversely with slope, discharge, and fineness of débris, but its absolute amount cannot be inferred from the experimental results. River slopes are relatively very small and river discharges are relatively very large, and the two differences affect the ratio in opposite ways. To compute the joint result we should have definite and precise information as to the laws of dependence, but our actual knowledge is qualitative and vague." The belief is later expressed that "all the generalizations from the laboratory results may be applied to natural streams, but only in a qualitative way; the disparity of conditions is so great that the numerical results can not be thus applied." A closing chapter on the application of the study to natural streams therefore hardly goes beyond a refined descriptive and explanatory account of various elements of river behavior. This somewhat disappointing conclusion is, however, in a measure offset by an account of various actual examples which serve to verify the principles involved, one of which may here be reproduced.

In this connection it is of interest to record a single observation on river efficiency. Where Yuba river passes from the Sierra Nevada to the broad Sacramento Valley, its habit is rather abruptly changed. In the Narrows it is narrow and deep; a few miles downstream it has become wide and shallow. Its bed is of gravel, with slopes regulated by the river itself when in flood, and the same material composes the load it carries. In the Narrows the form ratio during high flood is 0.06 and the slope is 0.10 per cent. Two miles downstream the form ratio is 0.008 and the slope is 0.34 per cent. Thus the energy necessary to transport the load where the form ratio is 0.008 is more than three times that which suffices where the form ratio is 0.06; and it is evident that the larger ratio is much more efficient than the smaller. The data do not serve to define the optimum ratio, but merely to show that it is much greater than 0.008.

PREPARATION OF FIRST REPORT

The hydraulic laboratory experiments were conducted, as has already been mentioned, at Berkeley, and were almost completed before Gilbert's illness in 1909; but the discussion of the experimental data was carried on chiefly in Washington; and although it was undertaken after a year of slow recovery from illness, it constituted the most arduous task that Gilbert ever entered upon. The Wheeler reports were avowedly incomplete summaries based on hurried observations. The Henry Mountains volume was based on a brief period of field observation, mostly qualitative in nature, the discussion of which was largely thought out in the field and speedily written up in Washington. The Bonneville monograph was equally Gilbert's own work, and differed from the Henry Mountains report chiefly in the longer time allowed for the collection of facts by himself and his aids and in the overlong delay in preparing the manuscript caused by the distraction of other duties. Various studies on Niagara and the Great Lakes were unlike the basin-range, Henry Mountains, and Bonneville reports, in calling for a large acquaintance with the results gained by many local observers, although these results were similar and supplementary to those gained from Gilbert's own intermittent field work; the novelty in these studies lies chiefly in their ingenious generalizations and interpretations. Later work on the basin-range structures was interrupted before it was much more than begun. In contrast to all these earlier investigations, the discussion of the débris experiments demanded an extended examination of technical studies by experts in a nongeological field, and a large amount of original analysis and computation; and in both these respects it has no parallel in Gilbert's earlier work. Letters written to his elder son between 1911 and 1914 confirm this statement.

The outlines of successive chapters prepared during Gilbert's convalescence at the "Hermitage" in 1910 were superseded by new outlines a year later:

I've had to change my point of view repeatedly and go back and rewrite.

Progress was therefore slow not only because of short working hours but also because of frequent revisions. A letter of January, 1912, gives an idea of the volume of computation involved:

In my work I have struck another stretch of computing and again the office has loaned me Mr. ———. His facility with the slide rule makes me wish I had lerned it years ago. It is rather late to train myself now, and I am likely to stick to logs. as a mere matter of habit. Twice I tho't I would take it up and bo't a rule, but it was always easier to do the next job in the familiar way than the new way, and I followed the line of least resistance. But this present work has introduced me to log. section paper, and that has become quite familiar. Of course, it involves the same principles as the slide rule.

Then referring to the many different values of various factors that have to be computed, he adds:

Does it look attractive? Really it is not unattractive to me and I call for help only to save my own time.

A month later he wrote:

I am rather dismayed at the amount of computation that seems desirable; but it will probably pay to give all my material a pretty thoro pre-digestion.

In spite of the labor thus called for, Gilbert bore it better than he had ventured to hope a year earlier. A letter of February, 1912, included a reassuring statement:

I am working comfortably and stedily and must be increasing my average hours per day. In January I reported—as the equivalent of my time in hours—22 days, which is ¾ of a full month.

But this success was at the cost of declining outside efforts, as the following extract from a letter of March, 1912, indicates:

The President of the U. of R. [University of Rochester] directs my attention to the fact that the approaching commencement is the 50th anniversary of my graduation, and invites me to give the commencement address—taking a scientific subject. Very sorry to have to decline.

Computations were continued even during the summer on the New England coast, as told in a letter of July:

Work has gone slowly for a few days—stald on the mathematics of mean values, and had finally to go to the library at Gloucester to get put on the right track. I find my equations and computations all right, but I did not know how to put the theory of it into words. What cleared the matter up was a batch of definitions in the Century Dictionary, and I found from the introduction that the mathematics of the C. D. was written by C. S. Peirce—a man so metaphysical I should never have tho't of going to him with a practical question.

Much time was taken by a phase of the work to which Gilbert, like certain other older members of the survey, had not been accustomed to give much attention; for during the early years of governmental surveys the practice of reading what foreign observers had accomplished on problems similar to those attacked by our own geologists in the new western field had been more honored in the breach than in the observance. But here it was dutifully followed, and in the course of his work Gilbert made a much fuller examination of publications by British, French, and German authors than he had ever made before. Indeed his manuscript was set aside several times while foreign studies were searched out and read over, the aid of a foreign-born member of the survey being sought on articles in French and German. A large amount of pertinent matter was thus brought to his attention, although the main line of his own work was on new ground. Sometimes he found himself lost in the modern terminology of physics, as appears from a letter of October, 1912:

When I have time I mean to read up on volts and amperes and such, to understand the gages [?] better than I do. The electric units have all come in since my school days.

Many books must have been consulted, for he wrote, in January, 1913:

Still busy with the libraries, and found the French engineers have done some good work I did not know of. Glad I did not go to print without finding out as to it.

And a little later:

Shall know right smart of hydraulics and some of hydrodynamics when I get thru.

But let no philologist imagine that "right smart" is New York State dialect because it is here used by a man of Rochester birth. As a result of this careful examination, the first débris report appears to contain citations of a larger number of foreign authors than are to be found in all the rest of Gilbert's writings put together. The completion of the report is thus referred to:

Friday the 13th [of June] 1913, was not an unlucky day for me, but one of exceptional plesure, for I turnd in a report on which I have been at work more than two years. The title is "Transportation of Débris by Running Water", and it will be printed as a "Professional Paper."

It was probably a consciousness of having thus entered upon a new field of study that led Gilbert to wish to have his manuscript looked over by an expert:

I want to have it criticized by some very competent man who will express his mind freely. Talkt with ——— a little when I was in Ithaca, but am a little afraid he would be too polite to be candid on its weak points.

In the judgment of readers after the report was published there were no weak points in the treatment of the material under discussion; if the report had a defect, it lay in the difficulty of applying its results to actual problems in river engineering, a difficulty which Gilbert himself saw and lamented. It is because of the abstract nature of its results that this study seems not to deserve so high a rank as its sequel, in which the actual distribution of mining débris was examined. Toward the close of this elaborate research Gilbert wrote his son a simple rule based on his own experience for the estimation of river discharge, essentially as follows: (1) Select a direct reach of uniform flow; (2) measure the cross section in square feet at a few points and take their average; (3) measure a base line along the bank; (4) throw a float, such as a tree branch with leaves, into midstream, time its passage along the base line and thus determine the surface velocity in feet per second; then (5) the discharge in cubic feet per second = 0.8 × sectional area × surface velocity.

DISTRIBUTION OF DEBRIS

Gilbert's second report upon his work in California deserves to rank with the finest of his studies. There is a brilliancy about it that even he rarely equaled. It embodies an exceptionally fine treatment of a high-grade problem in river engineering based upon a secure physiographic foundation; a fine treatment, because of its breadth and elegance; an exceptionally fine treatment, because the combination of thorough competence that Gilbert possessed both in engineering and in physiography is rarely encountered. Unlike the first report, which is largely experimental and theoretical, the second gives many actual examples of the distribution of hydraulic-mining débris, which is not by any means limited to the mountains, as its title, "Hydraulic-mining débris in the Sierra Nevada," would suggest, but which, as already noted, extends its consideration across the fluviatile plain or valley of California and through San Francisco Bay to the tidal bar outside of the Golden Gate. Yet in spite of its high quality, the second report is not so well known as it deserves to be; perhaps because it was preceded by the first, which by reason of its largely mathematical treatment was not generally readable and may therefore have discouraged examination of the second; and perhaps also because, unlike the Henry Mountains report and the Bonneville monograph, which appeared at an earlier time when survey publications were not so very numerous, this important report was only one in a great annual flood. Nevertheless, the second report deserves to rank in originality with the Henry Mountains volume, to which it is similar in size, and it is a close second to the Bonneville monograph, which it almost rivals in thoroughness. And as the last completed study of a great geologist, over 70 years old at the time it was issued, the report merits attentive reading by all his legion of admirers. Yet it would have been surpassed by the unfinished report on the basin ranges had that not been interrupted a few years later by Gilbert's death.

Like the report on the "Transportation of hydraulic-mining débris," the report on its "Distribution" entailed an immense amount of study; but here the greatest part of the observational work was in the field instead of in the laboratory, and the discussion of the observations involved chiefly a most ingenious train of physiographic inferences, instead of a laborious series of mathematical computations. The field work, begun in 1905, was long interrupted by the "Transportation" study, as well as by the year of illness. It was resumed in 1913, when California was revisited. In July of that year, Gilbert wrote to his son:

I have been up to Bear river and driven a buggy horse three days—trying to find what the river is doing with the mining débris, and looking for a dam-site for débris storage. Not much result and the work was rather trying to me. The weather changed from hot to hotter—110° in the shade the last day—and I was too exhausted to continue. I met the sea breeze at Suisun and it was delicious to feel chilly once more.

It appears to have been during these excursions in the uplands of the Sierra that Gilbert's attention was turned, and this time successfully, to a problem which, under the title, "The convex profile of bad-land divides," was left unsolved in his report on the Henry Mountains 30 years before. In certain areas of the uplands where the auriferous gravels had been hydraulicked off, a body of decomposed granite was revealed which had been given a sort of bad-land form by the action of the weather; its close-spaced stream lines were sharply incised, but its little divides were pronouncedly convex, and their convexity was explained as the result of the creeping of a thin surface soil. He gave the explanation thus found a general application:

Convex hill tops are found alike in forested regions, prairies and deserts. . . . Soil creep is omnipresent and appears to be competent.[1]

In contrast to its immediate predecessor, the second report is unusually readable. It abounds in entertaining matter tersely expressed; it is exceptionally instructive by reason of the frequent introduction of quantitative results; it is beautifully illustrated with halftone views, very pertinent to the text; it is full of wise and practical counsel which should lead to conservational measures in the near future; and it rises to a dramatic climax, for its farthest-reaching conclusion is that hydraulic mining débris should be controlled in the mountains because its unrestricted outwash will in time decrease the depth of water on the tidal bar outside the Golden Gate, although the bar is nearly 30 miles from the river mouths at the head branches of San Francisco Bay, and from 100 to 200 miles from the mines; and a decrease of depth on the bar would diminish the volume of San Francisco commerce. Some of the leading topics are here briefly summarized.

GRAVELS OUTWASHED UPON THE VALLEY PLAIN

The report assumes a knowledge of the auriferous gravels on the Sierran uplands: They are heavy, stream-laid piedmont deposits of a former time, which aggraded the lower ground along the western border of what was then the Sierran belt after the mountains of an earlier cycle of erosion had been worn down to moderate or small relief, and before the relatively recent and rapid, slanting uplift of the present range, in response to which the rivers have as yet incised only narrow and steep-sided canyons in the uplifted mass. The gravels have a thickness of from 100 to 300 feet, and to-day constitute high-level "flats" or open upland "valleys," adjoined by rounded hills and drained by comparatively sluggish streams which, on their way to join the incised rivers, cascade through short gorges and so descend into the canyons 500 or 1,000 feet below the uplands. Before the advent of the gold seekers, the rivers flowed down their canyons on beds of solid rock and coarse boulders; their normal load of finer débris was then less than their capacity. But after the introduction of hydraulic mining, chiefly on the tributaries of the Sacramento, the "tailings" were washed from the gravels of the uplands into the canyons, where much of the overload was deposited. The river beds were thereby aggraded and the narrow canyon bottoms were built up in gravelly flood plains, thus slightly diminishing the fall of the upper courses, but increasing the river fall toward the mountain border. On emerging from the mountains the rivers had decreased power and the débris that was not left in the canyons was there in part added to the preexistent piedmont alluvial fans, which constitute a modern analogue to the upland gravels and which grew in some cases to a thickness of 50 or more feet; and in part washed forward to the larger "valley rivers" of the fluviatile plain, and swept down these rivers to the head branches of San Francisco Bay. Aggradation became the order of the day, and as the total 35-year output of the hydraulic washings, or "mines," to streams of the Sacramento system alone is estimated at 1,295,000,000 cubic yards—about seven times the volume of excavation in the Panama Canal—the aggradation had very visible and threatening effects; all the more so because the rivers of the fluviatile plain, following the habits

[1] The convexity of hilltops. Journ. Geol., xvii, 1909, 344–350.

of rivers on such plains, had built up the land surface near their courses to a significantly higher level than that of the shallow "lateral basins" in the plain between the rivers. Farms near the mountains were buried under barren shrouds of sand and gravel; towns near the rivers had to build levees to protect themselves from the advancing deposits; the danger of floods was heightened with the aggradation of the river channels; the deltas in the bay heads were extended and the bays near their heads were shoaled.

The amount of débris artificially added to the Sierran rivers increased rapidly from the beginning of hydraulic mining in 1850 to its peak in 1884. Then with the injunction against hydraulic mining, it decreased still more rapidly, and in 1900 was less than in 1860. As a result of the decrease, the rivers proceeded to scour out their canyons, some of which are now laid bare down to bedrock again; but the scour from the canyons caused aggradation to continue for a time on the piedmont fans; then when the canyon scour was nearly completed, the fans were in turn attacked by their rivers and trenched to depths of 20 or more feet, and thus some of their detritus was washed forward to the "valley rivers"; however, a much larger fraction of the hydraulic débris still remains in the wide sectors of the aggraded fans to the right and left of the new-cut trenches than remains in the narrow canyons within the mountains. The débris from the fan trenches continued the aggradation of the "valley rivers" for a time, but now they also are degrading their middle courses; aggradation continues at present only in the lower courses and on the deltas.

> The downstream movement of a great body of débris is thus analogous to the downstream movement of a great body of storm water, the apex of the flood travelling in the direction of the currents . . . . It travels in a wave, and the wave grows longer and flatter as it goes.

In the case of the Yuba River, an important branch of the Sacramento system—

> the apex of the débris flood, leaving the mines in 1883, passed the mouth of the mountain canyon in about the year 1900 and the mouth of the Yuba River [in Feather River, a main branch of the Sacramento] in about 1905.

The capacity of the "valley rivers" to wash along the débris that they receive has been increased in their middle courses by the construction of levees which restrain them to a moderate width at time of flood, and in their lower courses by the reclamation of bay-head delta marshes across which the rivers are now confined by dikes to relatively narrow channels of rapid flow.

DEPOSITS IN SAN FRANCISCO BAY

Who could have expected, when the investigation of the hydraulic-mining débris was begun, that it would have to include a study of the changes in San Francisco Bay and in the tidal bar outside of the Golden Gate! Yet by following the advance of the débris farther and farther along its course, Gilbert showed most clearly that, contemporaneously with, and therefore presumably in consequence of, the deposition of débris in the head branches of San Francisco Bay, a shoreward shift of the bar has taken place. His argument proceeds as follows: The facts concerning the bar are first assembled. It is semicircular in plan, curving with a radius of 5 miles around a center in Golden Gate; it is formed of detritus detached from the ocean coast, close along which it is drifted by waves and currents, but on reaching the Gate it is held offshore by the ebbing tide. The depth of water on the bar is now 6 fathoms; in the Gate, 50 fathoms. Possible changes in the bar are next inquired into. If there were no ebb tide, the bar would stretch directly across the entrance to the Gate as a shoal or beach, in line with the coast beaches to the north and south, and San Francisco Bay would be closed; hence if the present tidal currents in the Gate are weakened, the curve of the bar ought to diminish in radius and the depth of the bar ought to decrease. The relation of tidal currents to bay-head débris is then examined. In so far as the débris merely shoals the bay it has no effect on tidal currents in the Golden Gate; but in so far as it raises the bay bottom above low-tide level in deltas and mud flats, it must weaken the tidal currents; and in so far as delta marshes are reclaimed for agriculture by building dikes to exclude high-tide overflow, the tidal currents at the Gate must be still further weakened.

The actual conditions are then determined. The quantity of detritus washed into the bay-head branches between 1849 and 1914 from natural sources of supply in all parts of the drainage basin as well as from hydraulic mining in the Sierra is estimated at 1,146,000,000 cubic yards—a quantity almost as great as the total outwash from the mines between 1850 and 1884—and this has sufficed to fill some 13,000 acres of bay-branch heads above low-tide level. The diminuation of the bay area thus brought about is calculated to reduce the tidal volume in the Golden Gate by 2.5 per cent of its measure in 1850, while the reduction due to the reclamation of tide marshes must amount to an additional 1.5 per cent. The total reduction of the tidal currents from their former value must therefore be about 4 per cent, and it is thought that such a reduction should have a recognizable effect in the shifting of the semicircular tidal bar. The argument closes by an examination of the position of the bar as shown by Coast Survey charts in 1855, 1873, and 1900. The discovery is thus made that in each interval between surveys the crest of the bar migrated landward, the total change being nearly or quite 1,000 feet; but no loss of depth was demonstrated. As the expectable loss of depth would be only about half a foot for a shoreward shift of 1,000 feet, the failure to detect so small a change is not held to invalidate the conclusion that the recent inward migration of the bar is actually due in large part to deposition of hydraulic-mining and other débris in the bayheads, and that the remaining part is due to reclamation of the marshlands. Could there be imagined a more beautiful enchainment of argumentation, or a more convincing demonstration of the principle announced in the Henry Mountains report years before, regarding the "interdependence of drainage lines"!

The conclusion thus reached is very properly taken as the basis for an outlook into the future. Further change from hydraulic-mining débris is not expected, because such mining is not likely to be permitted again unless the tailings are impounded in the mountains; but further change in consequence of reclamation of delta marshes is quite another matter, for this process still continues and bids fair to exclude the tides from all the marsh areas; and when that is done, the tidal discharge at the Golden Gate will be reduced to about two-thirds of its original amount. The bar may then be shifted shoreward by a considerable distance with a troublesome decrease of depth. The question is thus raised whether a local community on the delta marshes should make a large addition to agricultural wealth at the cost of a moderate injury to a great commercial harbor; here agriculture, which was successful in its conflict with mining, will become antagonistic to commerce; and on this point Gilbert remarks:

> That agriculture in its entirety is the industry of first importance is recognized by all, but it does not follow that commerce should yield to it at every point of interference. Each particular case of conflicting interest involves an economic problem in relative values and should be adjudged on that basis.

QUANTITATIVE PHYSIOGRAPHY

It must appear from the foregoing paragraphs that the report here summarized represents one of the most remarkable physiographic studies ever accomplished. In its quantitative aspects it is probably unparalleled. The amount of work that it involved was enormous; every step taken was carefully analyzed. Although the observational work on which it was based was in large measure finished before Gilbert's illness in 1909, most of the facts concerning San Francisco Bay appear to have been determined at a later date; all the statements regarding the transporting action of streams were also critically reviewed in the light of the laboratory experiments, after the period of incapacity through illness. One of the principles concerning transportation most frequently quoted is to the effect that although a river fully charged with débris can not continue to carry its load if its slope be decreased while other conditions remain unchanged, it can be so if its width be at the same time artificially reduced; in other words a graded, or even an aggrading stream may be changed into a degrading stream by narrowing its channel.

The quantitative values that appear all through the report were determined with great care. They are not precise, but they may be accepted as close to the truth. For example, the débris deposited by the "valley rivers" since 1850 is considered in two parts, one here called normal and coming from the ordinary processes of erosion; the other here called artificial, coming

from hydraulic mining. The normal supply, coming from all parts of the drainage basin under consideration, was estimated by examining the rate of waste from plowed fields, roads, trails, and grazed lands, as well as from the much larger area of land under natural conditions; the artificial supply was estimated from measurements of the volume of excavation in the abandoned hydraulic workings, reports previously made on this much-studied subject being supplemented by Gilbert's own critical observations. The normal supply was found to be about 420,000,000 cubic yards, or nearly half of the total.

Equal care was taken in estimating the present seat of the deposited débris. It is interesting to note that 11 per cent remains in the mountains, 22 per cent is found in the piedmont deposits, 4 per cent along the "valley rivers," 12 per cent in the river and delta marshes, and 49 per cent in the bays; while only 2 per cent reached the ocean. Although the auriferous gravels on the uplands were in large share of coarse texture, they were triturated so successfully during transportation that the piedmont deposits are largely sand, and practically all of the bay deposits, half of the total, are silt. The trenching of the piedmont fans was carefully studied on the Yuba River, where many quantitative determinations were made. The encroachment upon and the shoaling of the bay were quantitatively determined by comparisons of maps and soundings of different dates; but preliminary to this a critical physiographic study was made of the entire bay, with especial reference to the differential movement of the land on the two sides of the great fracture known as the Hayward fault, by which the inner and outer parts of the bay are divided and on which lay the focus of the earthquake of 1868. Although the evidence thus found of slow movements up to a very recent date was held to be favorable to the view that subsidence is still in progress, it is implied to be very small, for if it were rapid the area and depth of the bay might thereby be increased in spite of inwashed detritus, and as a matter of fact they have been decreased. It would thus appear that a subsidence of a significant geological rate may be practically negligible in comparison with artificially accelerated aggradation.

THE TIDES OF SAN FRANCISCO BAY

In no respect was Gilbert's study made with more scrupulous care than where it touched the tides of San Francisco Bay and the tidal bar outside of it; and in this respect the study is the more to be marveled at, as that section of it was largely accomplished after his illness. The volume of water alternately brought into and carried out of the bay by the tidal currents—the so-called "tidal prism"—might, it would seem, have been fairly well determined by multiplying the area of the bay by the average rise of the tide; but Gilbert was far too cautious and exacting to be satisfied by any such rough-and-ready method. He informed himself minutely on the nature of the local tides, including the well-marked diurnal inequality of successive high and low waters, the displacement of the times of rising and of falling slack water from the times of high and of low water, and the slow progress of high water as well as the decrease of tidal range inland through the irregular bay. The effects of the last two factors on the volume of the effective tidal prism [2] as measured between the water surfaces at the times of rising and falling slack water at the bay mouth was considered with especial care; for curiously enough the departure of tidal oscillation from synchronism in various parts of the bay results in diminishing the volume of the tidal prism, and hence in diminishing also the velocity of the tidal currents at the bay mouth, below the values they would have if the bay tides were everywhere alike in time and range; in other words, as the volume of a high tide in the lower bay is supplied not only by the flood current which runs in through the Golden Gate at the time of that high tide, but also in part by the ebb current from the preceding high tide which is then falling in the bay heads, the volume of the flood at the Gate is thereby somewhat lessened.

[2] There are a few lapses of verbal expression in the discussion of the "effective tidal prism" (pp. 71, 72), which may be ascribed to Gilbert's failing health, for such lapses do not characterize his earlier work. The unqualified statement at the outset: "During the rising of the tide, water flows from the ocean into the bay; while the tide is falling, the flow is toward the ocean," needs a correction that is implied but not stated in the lines next following; for the rising tide begins at the bay mouth while the ebb current is still running out, and falling tide begins there before the flood current has ceased to run in. Farther on, the meaning would be clearer if the "effective tidal prism" were defined as the volume of water which enters the bay "between any time of slack and the time of the following slack" *at the bay mouth.*

WORK UPON SECOND REPORT

Gilbert's letters confirm the impression of his deep interest in this phase of his report that a reader gains in looking it over. He was at work upon it in the winter of 1913–14 in Washington, when a letter to his son tells of his progress:

I find the working up of the tidal-prism problem in the bays mighty interesting—but I lost two days work by starting in with a bad theory. Then when I straitend that out I found I needed more data than hav been publisht. I tho't however that the additional data were in existence & by going over to the Coast Survey was able to get them. I also told my revised theory to the tidal expert—Harris—and was reassured that I was on the right track.

The pleasure afforded by the work on the bay was referred to again later:

Tide problems continue to be fascinating. Hav been especially attractiv this week because I've studied out a tangle that was bothering me.

He wrote in March, 1914:

I've been occupied for several days in measuring areas of lakes and tidal lands, and now am compiling a map to show all tracts affected by the Golden Gate tides. . . . My compilation is very ruf, so as to get the whole thing where I can see it in a bunch, but I mean to hav a careful compilation made for printing. I find the problem of the effectiv tidal prism a fascinating one. Did I ever show you my reticle for estimating areas? It is a plate glass slab, with a half-inch mesh engraved on one side. Laying it over a map I estimate an area by units of the mesh. For the rougher work I count the units which are more than half on the tract to be measured. For finer work I estimate by eye to the tenth of a unit.

Few other subjects were allowed to interrupt these studies of débris and tides; but in March, 1914, a proposed side issue naturally proved irresistible:

There is a temptation on my table—to interrupt my regular work to do an attractiv thing which will take only a few days. I am requested to write a chapter on Lake Bonneville and Great Salt Lake for a railway guide to be issued by the Geological Survey for tourists to the Panama Exposition [in San Francisco, 1915]. I'd like immensely to do it, but feel that I ought to stick to this mining débris report till it is finisht. So I am tempted and hesitate.

But in the end he wrote the desired chapter.[3]

The enlarged understanding of the tides in San Francisco bay that came while Gilbert was still occupied in Washington with the "fascinating study" of the effective tidal prism naturally led to a more critical study of the tides in the bay itself; hence when he went to California in the summer of 1914 he did not as before make the Faculty Club at Berkeley his headquarters, but took a room at the University Club in San Francisco. Soon after arriving there, he wrote to one of his sons:

Yesterday, Mr. ——— and I did some work on the tides . . . spending the day in a boat. Am elaborately sunburnt. Have cald this morning on . . . to arrange for an observer to supplement ——— next Saturday in the Golden Gate. The job will take about 30 hours and I cannot depend on myself—and of course one observer cannot take the whole stunt. . . . I was at the Faculty Club last week and saw a lot of people we know. Had a game of billiards and of bridge.

Half a year later, proof reading became for a time about as great a burden as computation had been before.

The rest of the book proof has come—187 galleys in all, and it will be the middle of the week before I am thru with it. I hav to go slowly—becaus careless reading is sure to leav blunders undetected. Most of the things I find to correct are slips of typists or compositors, but some of them are my own—and that's saddening for I went over the manuscript many times. I can not hope to eliminate all now, but am doing my best.

This fatiguing duty continued to March, 1915:

Not thru with my proof yet. Find it pays to read the whole thing again—keep discovering things I should be mortified to detect after publication. Think of letting "a phenomena" pass me several times in the ms. and twice in proof! And yesterday I discovered a rather important statement from which the word "not" had been omitted.

---

[3] U. S. Geol. Survey, Bull. 612, 94–99.

CHAPTER XXX

THE LAST EIGHT YEARS

GRADUAL RECOVERY FROM ILLNESS

Gilbert continued to make his winter home with the Merriams in Washington after his illness as before, and while there worked as steadily as he could on his débris reports until they were finished. He seemed to enjoy not having to make new arrangements for himself. On returning one year he wrote to a frequent correspondent:

> My niche and my rut were both ready for me and I'm settled in them . . . plodding and loafing in the same old way.

He avoided geological discussions as too disturbing, and although he occasionally lunched with a group of survey members who in a measure perpetuated the Great Basin mess, he absented himself from all scientific meetings and was heard no more from their platforms. He wrote to his elder son in the spring of 1911:

> The [National] Academy of Sciences has come and gone. I have let it alone—and it let me alone. I hardly saw a member.

Although he ordinarily gave four or five hours daily and occasionally as many as eight to his manuscripts, even an informal social gathering tired him. In November, 1911, he wrote of a party he had attended:

> There were ten at the supper and the conversation used me up, so that I fled before the party was half over. However, I got my information on limits easily and shall not be tempted to attend any of the sessions or functions of the Scientific Societies to come here Christmas week.

But he seemed pleased when three members of the Association of American Geographers, of which he had been president three years before, called upon him. When alone he was active-minded enough, for shortly before recording the fatigue caused by personal contacts, he described a "water modulus" of his own devising for the control of the flow into the side canals of an irrigating system, a device which many another would have patented but which he offered "pro bono publico."

> The general notion is as follows—a tapering arm passes thru the opening to supply water from a canal to a distributary. Its position is controlled by a float in such a way that the greater the hed the smaller [the] opening; and the taper is so adjusted that the discharge is constant despite changes in the level of water in the canal.

It is not known whether the device was ever put to practical use.

The care of wordly possessions became a burden as age came on, and many of them were wisely disposed of. In 1910, while the illness of the previous year still weighed upon him, Gilbert gave his books and pamphlets to the geological department of Denison University, at Granville, Ohio, where they form the highly prized nucleus of a valuable working library, easily accessible to the students of the department and much consulted by them. In May, 1911, he placed his medals, a list of which will be given later, in public care and wrote of their disposition to one of his sons in the light vein he frequently adopted in personal correspondence:

> Today I took my medals to the National Museum. They were to be put in a glass case with other objects of the same class near the entrance to the old building. I took a receipt ("on deposit") which I shall put with my papers in the safe deposit box. So if Roy and you get broke you can escape the poor house for a while by recovering and selling them. There's about $300 worth of gold in them.

The same feeling that led to the disposal of these possessions made Gilbert disinclined to receive gifts; he went to the point of discouraging his friends from the habit of giving Christmas presents by giving very few himself. At the end of 1912 he wrote: "My abstinence in the present business for a few years has had a quieting effect on the incoming stream, and this year only two people have given me things I can not use"; and yet at the end of his diary for the year before is a list of birthdays of some 30 friends and relatives.

In his later years, although his salary was no longer supplemented by pay for lectures at colleges or for work on dictionaries and encyclopedias, his income was above his simple needs and he laid aside a part of it every year. A letter that he wrote to his elder son in June, 1912, shows the happy relation existing between the two in financial matters:

It is perhaps well to repeat my point of view. You and I have become a mutual insurance company, in the sense that either of us if embarrast or broke would call on the other for help. It is better for the joint interest that some of my quiet investments be exchanged for more activ investments in your hands. Your investments will have to be on your own judgment, not mine; and I do not care to impose any conditions. I am satisfied that you will not be too speculative.

It may be added that the father carried through his later years an insurance policy which, after his death, should yield an annuity of $500 for the younger son, Roy, and which, if this son died before the father, should add $20,000 to his estate.

SUMMERS AT ANNISQUAM, 1911, 1912

Gilbert spent the summers of 1911 and 1912 in a small hotel at Annisquam, a pleasant resort on the Massachusetts coast, where he did a satisfactory amount of writing. A letter to one of his sons describes the simple comforts and pleasures he enjoyed:

I've been here several days now and am partly set in my room. Have screwed that long box that you drove nails into with its bottom against the wall and am using it as a bookcase—just as at the [Comstock's] Hermitage. And the dinkey stand of the summer hotel room is replaced by a kitchen table—whose unvarnish matches the bookcase. My room is high up in a corner. One window commands the sea, the other the harbor, and the view is fine. For the first time in many years I am living in view of the sunset.

The position of his hotel was described as—

one or two stone thros from the water at north and west. Just north is a nice little bathing beach set among granite rocks, and on the west side is a convenient boat livery. I dout if I bathe, but the boats have already been sampled and approved—hevy, stedy, easy-rowing dories".

In order to prepare for the hours devoted to writing, regular exercise was taken on the water, chiefly before breakfast, and the distance covered was gradually increased from 1 mile to 5; but as rowing alone was found to be too solitary for the best diversion, an experiment was tried and reported upon to the elder son:

I seem to be the only one [to row] besides the fishermen, and it is a bit lonesome. Tried taking a widow along yesterday, but she talkt too much, and I don't think I'll give her another chance.

More enjoyment was found in a few salt-water canoe trips, in which his fellow paddler was the geologist Van Hise, president of the University of Wisconsin; and as he had not tried canoeing for some 20 years, he was pleased to find his paddling muscles in trim without special training.

The second trip was 8 miles . . . and at one point we rode on a full grown Atlantic swell which was making magnificent surf at the shore, a most exhilarating experience.

Exercise appears to have been combined with observation.

I've taken up the study of sand ripples, for which the locality is well adapted. It gives a good theme for boat excursions and is somewhat related to my laboratory data.

And observation appears later to have led to risk:

My curiosity about sand ripples on a bar led among breakers one morning, and to escape a wetting I pull'd pretty hard for a few minutes, which set the wheels going in my hed and I didn't amount to much for a day or two: all right now.

A longer but quiet trip was made near the end of the first summer:

Rowed to Essex river to take some fotos,—especially a drumlin that I wished to get as a type. It rises as a solitary oval hill in the midst of a tidal marsh. My trip was about ten miles long, but I took it in a very leisurely way, being gone over six hours and getting lunch at a farmhouse close to the drumlin.

A letter written the second summer at Annisquam shows that his eye and mind were still keen for physiographic observation and interpretation:

The most notable recent variant [from rowing] has been a motor boat excursion to a sound cald Plum Island river, where the topography had features to interest me. I had supposed it proved that this coast is going down a foot or two a century, but all the evidence coming my way these two summers is in the direction of stability.

PERSONAL INCIDENTS

After the experience on the water at Annisquam Gilbert was led to take up his old exercise of canoeing on the Potomac during the winters in Washington, especially when a congenial companion could be found who paddled well and who did not talk too much or too little. Among others, the topographer, W. D. Johnson, and the petrographer, J. P. Iddings, are both mentioned as possessing the desired measure of dexterity and conversation, but his companions were not always geologists. Cards, dominoes, and billiards continued to be favorite indoor recreations, even to the point of playing in tournaments at the Cosmos Club. Concerning one such competition in March, 1914, he wrote:

> I am in the finals at billiards. My average run in the 14 games of the first round was 1.43. Have played one game and lost it. 4 more to play.

But the competition of tournaments demanded more effort than he could easily give, and in the final round he was defeated. He still enjoyed rhyming and guessing games; one that was called "positive, comparative, superlative" consisted in inventing definitions from which words like urn, earner, earnest, were to be guessed.

Gilbert was always an active reader, not alone of scientific articles but also of magazines and newspapers, and of such books as Crothers's Essays, the delicate humor of which has been well said to "make the mind laugh." He had the habit of marking magazine articles that he liked and sending them to his friends, that they might be enjoyed twice for one printing. In ways like this he was always doing little kindnesses that made for happiness, and so generously as to produce no sense of obligation. To some of his correspondents he had a way of inclosing newspaper clippings; cryptic American jokes especially appealed to him. One such may be here included for future historians to work out; it was taken from a jocose essay on Arizona, published shortly after the Republican presidential convention of 1912: "The Roosevelt Dam is the biggest thing of its kind west of Chicago." Reading aloud was still, as it long had been, a favorite occupation when the hours of writing and exercise were over. In 1912 one of his letters written in the simplified spelling adopted in his later years revealed a characteristic consequence of this form of entertainment:

> We have recently finisht "The Broad Highway" and found it intensely interesting toward the end. At the pathetic places——— and I had to use our handkerchiefs, but the women laft—at us. I suppose they hav red so many novels they are callous. Or do they illustrate the peculiar "dullness" of the female of the species?

In spite of the misogynist-like disrespect shown in the last inquiry, Gilbert was "adored" by many younger members of the half of the species there referred to, and was furthermore a great favorite with the children of certain households where he was a not infrequent inmate. To them he did not take the part of the serious scientist, but showed only the playful and affectionate side of his nature. The small son in a friend's family conceived so great fondness for him that he made a special addition to his evening prayer in behalf of the tall visitor: "O Lord, bless father and mother and Mr. Gilbert and some ladies." It is often recorded that eminent men are fond of children, but it is seldom that testimony so spontaneous as this is presented to show the fondness of children for eminent men. But in spite of his own enjoyment of dancing parties in his early years in Washington, Gilbert did not in his later years find himself in sympathy with the round of gaiety in which the grown-up children of that time were engulfed. In 1914 he wrote:

> The girls [not specifying which girls he referred to] are whirling giddily—dances, teas, card parties, calls, and a charity spectacle of some sort. I'm truly sorry for them, but they don't know enuf to be sorry for themselves.

What is known as "Society" never had any attraction for him, and in his older age it repelled him. It is told that when his evening suit had become somewhat worn, he refused to order another and used the lack of one as a convenient excuse for not attending "functions."

Sometimes, as if unmindful of the advance made by certain young relatives from unconscious girlhood to self-conscious young ladyhood, he failed to realize the enormous importance that the proprieties of life thereby gained in their eyes. Two of these personages would surely be pleased to know that in the "summary" for 1914 in his long-continued series of diaries, he regarded the record: "Effie and Edith visited Washington, Feb. 2–5," as important enough to be entered under the "Events" of the year; just as they must surely have been gratified

at the time of the visit on seeing that he felt a responsibility for their having a "good time"; although they may also have been a bit scandalized by his method of showing them attention, for between the acts of a play to which the three went together he drew forth from his pocket a white paper bag of generous size well-filled with heart-shaped candies of a self-announcing peppermint flavor, and, oh, horrors! he expected the young ladies to eat thereof in public! Their only consolation for this well-meant but ill-directed solicitude on his part was to say to each other afterwards in private: "He thinks we are still girls." But one at least of these grown-up girls must have considered his thoughtfulness touchingly appropriate when, not long afterwards, she received from him as a wedding gift a piece of silver that her mother had given to his wife at his wedding in Cambridge nearly 40 years before. How few geologists, how few men there are of any profession, to whom a wedding gift retains its individuality for so long a time!

Gilbert's seventieth birthday, on May 6, 1913, was spent quietly with the Merriams in Washington. A letter comments briefly on the simple celebration of the occasion:

> Yes, the mile post was the 70th. Four years ago I didn't expect to make it, but now there's no telling how many more. The day's celebration included a cake, and a telegram from Ithaca. Friends here pland a surprise party, but Dr. M. discouraged the notion, so I escaped.

Parts of the summers from 1912 to 1915 were spent by Gilbert with his sister, Mrs. Loomis, at Jackson, Mich. In the first of these vacations he tried his hand at driving an electric automobile, and ran "a rather wobbly mile," but was soon competent to take his sister out for her errands. He combined this exercise with study:

> I have done quite a lot of geological reading while doing the waiting stunts that come in as part of a chauffeur's duty.

During the following late winter in Washington he had occasional practice in driving cars belonging to his friends, and thought he could soon learn to run one without too much strain even in a city; but was discouraged on discovering that after one of his trials he made only 9 at a game of billiards against the 50 of an opponent whom he had beaten the day before. In the summer of 1914 the stay at Jackson was followed by an excursion with his sister to Niagara, where they had the company of his younger son, Roy, and to Ithaca for a visit to the Comstocks. Journeys across the continent were repeated in the following years.

If Gilbert felt that "a march had been stolen on him" in giving his name to the salt-water gulf that for a time occupied the basin of Lake Ontario, it is clear that he would not tolerate an involuntary act of what he called "peak piracy" by which Gilbert Peak in the Uinta Range of Utah was declared in a Survey Bulletin issued in 1915 to be named in his honor, whereas it had in reality been named for an Army officer who had been stationed at Salt Lake City years before. He therefore addressed a formal letter to the director of the survey, setting forth the facts and adding:

> It seems to me undesirable that the Survey of which I am a member perpetuate an error of this sort, and I write to suggest, (1) that the passage be marked for omission when the Bulletin goes to a second edition, and (2) that the fact of the error be brought to the attention of members of the Survey who are likely to write of the Uinta mountains.

Fortunately there was no reason which he could allege for removing his name from the superb mountain in Alaska to which it had been attached by one of his many admirers among the explorers of that region.

The Great War in Europe, the beginning of which he believed was based on a false excuse and the end of which he did not live to see, was to Gilbert so depressing a matter that he did not like to let this thoughts dwell upon it. He hated strife and was essentially a pacifist by nature; yet as the war went on and the United States was drawn into it, he was, like many others, much more in sympathy with our Government's action than he had been when, nearly 20 years before, we declared war against Spain; for Spain had offered to submit our dispute with her to arbitration. He did what he could in buying Liberty bonds and in contributing to the Red Cross and urged others to do the same. The problem of protecting our troop ships against the attack of submarines aroused his inventive faculty, and he found his mind working

upon it to such an extent after going to bed that it became necessary to devise various schemes to divert his thoughts so that he might go to sleep. The economic problems of the war also interested him greatly, and he foresaw that they would continue to trouble us after the war ceased; only a few days before his death he told his elder son that even though the war might end soon, the world would be left so far out of adjustment that it would be necessary to economize expenditures for years to come.

During this period of old age, as well as in his more active maturity, Gilbert never undertook any work in mining geology as a paid expert, never entered upon any organized movement for public welfare, never held any public office apart from geological surveys, and with the exception of brief periods of service as director on the boards of two Washington enterprises of moderate dimensions, never took any official share in business matters. It is not to be doubted that his clear discernment and his calm wisdom would have made him a valued associate in many such relations, but they did not attract him. He followed political campaigns with interest, but took no active part in them. He kept his own accounts accurately and was a cautious investor and a careful spender; but business as such had no allurement for him. He was unreservedly devoted to scientific work, which he always conducted in an absolutely impartial manner, and in which he showed an extraordinary capacity for maintaining a suspended judgment in problems that he did not regard as closed. If there be any such divisions of science as pure and applied, his work belonged almost exclusively to the former.

His face in these later years usually wore a thoughtful and serious yet placid expression, but was often lighted with a smile. His hair was thin and white; his walk was still rapid, but a slight stoop lessened his full height of 6 feet 1½ inches. His manner of speaking was like that of his writing, clear, direct, concise. He had as always an even judgment, and in spite of his scientific eminence he was never in the least arrogant; on the contrary, he showed the gentle serenity and the unaffected simplicity that accompany the wisdom born of a long experience with nature and an untiring search for truth.

LAST WORDS ON ISOSTASY

Gilbert's latest study of isostasy was a short essay published at the end of 1913.[1] This was almost his last work on any subject, for although it was followed in date of publication by his report on the transportation and distribution of hydraulic-mining débris in California, nearly all the work on those subjects had been completed previously. Since his three essays of 1895, great progress had been made in the isostatic problem by the geodosists of the Coast Survey as well as by others in Europe. Stations for the observation of gravity had been much increased in number, and the reduction of the observations had been greatly refined over earlier methods by taking fuller account of the topographic irregularity of continental surfaces and of inferred variations of underground density from place to place. As a result, the existence of isostatic equilibrium in the earth's crust had been shown to be more nearly perfect than was supposed before; but let it be noted, in order that the reader shall not imagine the equilibrium to be more precise than it is, that the average anomaly of gravity in the United States without regard to whether it is in excess or in defect, had been placed at 630 rock-feet, and that several local anomalies of 1,200 rock-feet were found; and those are no small quantities in view of the fact that the mean altitude of the United States is only about 2,500 feet. Furthermore, a general agreement had been reached with regard to the depth—the so-called depth of isostatic compensation—at which the pressure should be uniform because of the inverse relation of rock density and crustal thickness. The existence of such a level surface of uniform pressure had been recognized in Gilbert's earlier work, but he had made no attempt to estimate its distance below sea level. When it was shown to lie probably at a depth of 122 kilometers, and when on the adoption of that depth the calculated gravity anomalies for 124 gravity stations and for 765 deflection-of-the-vertical stations in the United States were made available for plotting on a map, Gilbert carried the discussion of the meaning of the anomalies in some respects even further than it had been carried by the geodesists, and in doing so showed an undiminished

[1] Interpretation of anomalies of gravity. U. S. Geol. Survey, Prof. Paper 85, pt. C., 1913, 29–37.

clearness and keenness of penetration in an involved subject. It is not a little gratifying to see that this able essay, like the two essays on the distribution of mining débris in California, both of which were completed after his serious illness of 1909, are marked by all the intellectual vigor of his earlier years.

The geodesists had, largely as a matter of convenience in their calculations, assumed that the density of the rock columns, which rise with differing heights from the level of compensation to stations at different altitudes on the continents, is inversely proportional to the height of the columns; that the departures from the mean crustal density thus introduced were uniform through the entire height of each column; that the depth of compensation is everywhere the same; and that no variations of density occur below that depth. They recognized that these assumptions were not necessarily correct, but their discussion gave prominence only to the possible incorrectness of the first. Gilbert therefore, while accepting the conclusion that the isostasy of the crust is nearly perfect, looked into the possible incorrectness of the other three assumptions. Space can be here given only for his consideration of one of them. He argued that, inasmuch as the assumption of isostatic compensation by variation of density in each crustal column inversely as its height involves a horizontal change of density from column to column, a vertical irregularity of density distribution in the various columns no greater in degree than that of the horizontal change might be plausibly accepted; and then, still working on the assumption that the average density of each column is in inverse proportion to its height, he showed that if, at a station where gravity is slightly in excess, a slight excess of density be assumed in the lower part of the local column with a compensatory defect of density in the upper part, the gravity excess will disappear; and conversely, that if, where gravity is slightly in defect, a slight excess of density be assumed in the upper part of the local column with a compensatory defect in the lower part, the gravity defect will again disappear. Thus isostatic equilibrium might become exact. But it is evidently not necessary or even expectable that it should be exact; the special assumptions as to the vertical distribution of densities here assumed need not be true. The crust may sustain residual inequalities of pressure by its rigidity.

However, in order to learn whether the facts observable at the surface give support to these special assumptions, Gilbert next examined the distribution of the calculated gravity anomalies, as charted on a map prepared by the geodesists with anomaly contour lines for every 300 rock-feet of excess or defect, and considered the anomalies with reference to facts of geological structure and history. He then concluded, as others had also, that neither the composition of the surface rocks, nor any recent changes due to erosion or deposition, nor any reasonable underground extension of surface structures can account for the anomalies. For example, if the axis of the Allegheny plateau be followed from the Catskills southwestward to central Tennessee—an axis of similar geological structure and history for ages past—there is found a defect of over 900 rock-feet in the Catskills, a small excess in southwestern Pennsylvania, a moderate defect in West Virginia, and an excess of 900 rock-feet in central Tennessee, where the plateau is not so high as in West Virginia. Neither the surface composition, nor the geological history, nor the present altitude of these several districts can account for such differences. Similarly, if one travel from southwestern Alabama, a region of modern deposition, across the Mississippi embayment of the coastal plain, also a region of modern deposition, to western Arkansas, a region of modern erosion, the first and second regions, which are geologically much alike, will be found to have, respectively, a defect of 1,200 rock-feet and an excess of 900 rock-feet; while the first and the third regions, which are geologically unlike, both have the same defect of 1,200 rock-feet. Here again, neither composition, history, nor altitude will explain the anomalies. In view of these and other similar facts, Gilbert reached the conclusion that the correspondences of "visible structure and physiography" with the gravity anomalies of the map "are so slight that they may be regarded as accidental, and the general relation is that of independence and discordance." He thereupon concluded further that, as the anomalies can not be accounted for by the surface facts nor by the extension of those facts to a depth of a few kilometers, they must probably be due not only to the irregularity in the vertical distribution of density in the crustal columns down to the 122-kilometer level of compensation as above assumed but also in part to heterogeneity in the nucleus below that level. This may be regarded as a geological in contrast to a geodetic conclusion.

THE GEOLOGICAL ASPECTS OF ISOSTASY

It may be pointed out in continuation of the last sentence, that there is a still more strictly geological aspect of the problem of isostasy to which Gilbert referred only incidentally, but to which his references are nevertheless illuminating. If the earth's crust is to-day in a state of almost perfect isostatic equilibrium, it has probably been similarly isostatic during the greater part of past geological time; for it would be unreasonable to suppose that an earth crust, which has had so enormously long an opportunity to bring itself into equilibrium, had not attained that state before to-day. The purely geological question then arises: How has the isostatic condition of the crust been maintained through the many deformations and erosions and depositions that it has experienced in past geological time? Gilbert's essays fortunately contain a few passages that bear on this matter.

The first, already quoted, is found in his second essay of 1895, in which the great interior plain is referred to as a region that has been so long exempt from orogenic corrugation that all the topographic effects of the corrugation have been obliterated by erosion; and as therefore being a region in which isostatic equilibrium ought to be well established, if established anywhere. It is furthermore said in the same essay in connection with the gravitative excess of the Rocky Mountain mass:

> The upland may be conceived to have originated from the horizontal compression of some crustal tract and the consequent upthrust of superificial portions—a process which would result in local excess of matter approximately to the full extent of the uplift. Excess would continue until the protuberance was removed by erosion.

The third essay of 1895 rephrases the first of the above statements but with the significant addition of "viscous flow" to other agencies of adjustment: The great central area of the United States "has been exempt for a succession of geological periods from orogenic disturbances, and during that time has had exceptional opportunity for the gradual relief, through viscous flow, degradation and sedimentation, of the strains engendered by gravity in connection with anomalies of density. It seems, therefore, a priori probable that this plateau is in approximate equilibrium," as it was found to be.

Gilbert's idea therefore was that mountains produced by crustal corrugation or by subsurface compression and surface upthrust would thereby lose the isostatic equilibrium that they might previously have had, but that they would gradually regain the equilibrium when their surface excess of matter was removed by erosion, or when the surface excess was counterbalanced by a deep-seated viscous outflow. In other words, although isostasy would be temporarily lost in regions and at times of great crustal disturbance, it would spontaneously come to prevail again in regions and at times of crustal calm; and it may be added that as great crustal disturbance is a relatively rare event in any one region, isostatic equilibrium may be supposed to have been generally prevalent over the earth through geological time.

It is instructive to note that, as to the relative efficiency of surface erosion and of deep-seated underflow in bringing about and in maintaining crustal equilibrium, some pointed arguments impugning the value of the latter process are introduced in the essay of 1913. The most significant comments there made on this phase of the problem concern an old hypothesis regarding underflow, at a depth of mobility, from a region that has been weighted down by deposition of sediments toward an adjacent region that has been simultaneously lightened by the erosion of the surface rocks. It is first shown that, to whatever extent the deep-seated readjustment by underflows lags behind the disturbance of equilibrium by surface erosion and deposition, there should be to that extent an excess of mass in the region of deposition and a defect in the region of erosion; and hence "under the hypothesis of interpretation which correlates excess of mass with plus anomalies and defect of mass with minus anomalies we should expect to find as a general fact of the anomaly map, an anomaly gradient from ocean to land in coastal regions," such as the eastern United States. It is then pointed out that, while such a gradient exists on certain northern and southern stretches of our Atlantic coast, "the gradient is unequivocally oceanward between Delaware and Florida"; but he did not here take account of the expectable oceanward gradient which, as shown by Helmert (1909), should, even on a perfectly isostatic

earth, occur at continental borders, because land gravity is there increased by the excess of attraction of the dense suboceanic crust and ocean gravity is there diminished by the defect of attraction of the lighter subcontinental crust. Attention is next directed to the Appalachian belt south of the area of glaciation, where it "has been practically free from loading during two geologic ages, and has been unloaded to the extent of many thousands of feet of rock." A lagging of readjustment by underflow should here result in a defect of gravity; yet "there are large plus anomalies in that belt, and their existence evidently creates a difficulty in interpreting plus anomalies as due to crustal excess of mass"; for "isostatic adjustment may be supposed to have added mass in compensation for the unloading, or for part of it, but may not plausibly be supposed to have overcompensated so as to create an excess of mass." In the third place, the Delta of the Mississippi is instanced as a district of great loading in recent time; "and if isostatic adjustment by outward underflow has not kept pace with the loading there should be in this district a local excess of mass. The fact that the local anomaly is minus instead of plus calls in question the mode of interpretation which infers defects of mass from minus anomalies"; and this must be accepted because here the Helmert effect is in some way masked.

In review, it is concluded that, although an approximate isostatic equilibrium has been shown to exist and although it is therefore appropriate to search for processes that produce and maintain it, underflow driven by gravity must hold a low rank among these processes, because the gravity anomalies shown on the map are frequently the reverse of those that should occur if underflow held the high rank that has often been given to it. Nevertheless the Laurentian region is noted as having subsided when it was heavily covered with ice in the Glacial period, and as having risen when the ice melted away, as if the disturbance of equilibrium by loading and unloading at the surface had there been rather promptly compensated by outflow and return flow deep beneath the surface; and this would seem to contradict the conclusion just announced, and to show that gravitative underflow ought to hold the relatively high rank as a means of producing and maintaining crustal equilibrium that has commonly been ascribed to it. But in answer to this one may suggest that the Laurentian region, having been long untroubled by forces of deformation, has attained so delicate a balance that it responded quickly to the accumulation and disappearance of the great ice sheet, which appears indeed to have been thick enough to enforce its demands; while recently deformed regions which, like the upthrust highlands of the Rocky Mountains, maintain a considerable overload of rock may be still so constrained by the upthrusting forces that, much as they would like to settle down in response to the overload, they are not allowed to do so. However this may be, Gilbert's conclusion was that the gravity anomalies of regions that have been unloaded and loaded at the surface by erosion and deposition are not such as would result from a lag in the underflow by which new matter would be introduced beneath the unloaded region and withdrawn from the loaded region. Some other cause for such anomalies must be found; and Gilbert's belief was that the cause lies in "irregularities in the vertical density gradient," and that this cause "appears both qualitatively and quantitatively competent." The argument by which this conclusion is reached is an excellent example of Gilbert's method of thinking and deserves close study.

CHAPTER XXXI

LEADING CHARACTERISTICS OF GILBERT'S WORK

GILBERT'S ERA

Gilbert was fortunate in his era, an era in which the rational views already introduced into the discussion of geological problems by his predecessors were enlarged and confirmed. Indeed, during Gilbert's lifetime geology was broadened by a rapid, almost world-wide extension of observation into regions that had before been wildernesses; it was at the same time advanced by a change from a freely speculative to an increasingly critical method of treatment, the reflection of which was seen in the change from polemical dissension toward friendly discussion at geological meetings; and it was exalted by the adoption of a thoroughgoing evolutionary philosophy, which recognized the uninterrupted continuity of natural processes, organic as well as inorganic, through all geological time. In an era of extraordinary geological progress thus conditioned, Gilbert was mentally at home, and he took an active part in leading many of its lines of progress. Endowed with a most reasonable spirit and altogether free from the restraint of artificial conventions, he was predisposed to the development and the acceptance of a rational philosophy of nature; he was never content to take old authority for truth, but always sought new truth for his authority. Even if his first essay, an account of the mastodon, intelligently compiled in a provincial atmosphere before Darwin's Origin of Species had taken root there, presented the traditional view of the distinct creation of every organic species, he appears, immediately on emergence into a larger field of experience and reflection, to have accepted evolution as a guiding principle; and although he was never closely concerned with its organic aspects, its physical aspects made him a confirmed uniformitarian. Unlike certain of his American seniors, he was one of those who found competent explanation for the changes of the past, great and small, only in slow processes like those of the present, and who never made extravagant drafts on forces, natural or supernatural, for the accomplishment of geological changes, organic or inorganic. In case the younger geologist of to-day should ask, in surprise, why Gilbert or anyone else should be praised for never making extravagant drafts on such forces, that would only show how well Gilbert and others of his generation, working against the surviving prejudices of earlier centuries, confirmed the evolutionary philosophy which has come to be the unquestioned basis on which all geologists of to-day advance so confidently.

CHARACTERISTICS OF PUBLISHED WORK

If Gilbert's published work is reviewed with the object of discovering its leading characteristics, it is found to have been largely devoted to problems in physical geology and physiography, to have been always guided by a candid reasonableness, to have been abundantly refreshed by ingenuity and originality, to have been frequently given a quantitative flavor, to have been set forth with the most gratifying clearness and the most appealing fair-mindedness, and on occasion to have exhibited an extraordinarily well-balanced suspension of judgment. His work will be found, furthermore, to be based almost altogether on field observation and mental reflection, seldom supplemented by the collection of specimens and rarely extended by work in laboratories or museums; to represent with few exceptions his own personal studies, for he was not often aided by assistants; the reports and essays that he published represented his individual work to an unusual degree; to be—apart from the affairs of the national survey—seldom concerned with scientific enterprises which led him into intimate association with other men; and to be directed altogether to problems of unapplied science; for although he lived in an age when the practical value of economic geology gained an unprecedented recognition from the business world, he took no part whatever in that phase of earth studies. His occasional mathematical discussions, of which the most elaborate example was his next-to-last published report, are sometimes difficult to follow, not from obscurity on his part but from prevailing mathematical

lameness on the part of the would-be follower. The same next-to-last report is the only one that was based essentially on experimental data; it was indeed as much a study of an engineering as of a geological problem—the transportation of detritus by running water. His many other papers are remarkably free from technicalities; their facts are directly and accurately stated, their discussions are simply, clearly, and convincingly presented. The impression that they create is one of calm reasonableness.

His first independent investigation, a voluntary and very original study of the age of the Mohawk gorge at Cohoes, near Albany, was exceptional in showing at once keenness in obervation and skill in interpretation; although the discussion was of narrowly limited range, it was as strikingly quantitative as his final great study, made nearly half a century later, on the distribution of hydraulic-mining débris in California. His work on the physiography of the Maumee Valley in northwestern Ohio, one of his earliest studies as a member of a geological survey, was marked by the delicate application of simple principles in the explanation of a landscape of almost imperceptible relief; his originality was here shown in the announcement of the then novel idea that, in view of the unevenness of the land surface over which the Pleistocene ice sheet advanced, its margin must have been multilobate; an idea so rational that everyone on learning it must have wondered why it had not been hit upon sooner; indeed, why he had not hit upon it himself.

THE WHEELER SURVEY REPORTS

The two Wheeler survey reports on Gilbert's first three field seasons in what was then the Far West are of uneven quality, and in this they reflect the hurried observations in certain districts in contrast to the more gradual progress in others; but they reflect also a personal dissatisfaction with the conduct of scientific investigation under military control, whether this required the carrying of a carbine which he was glad to lose when his boat upset in the Colorado River, or the postponement of publication which he wished to make promptly on return to Washington. Yet although trammeled by these regulations, he was not trammeled by certain premature geological generalizations of eastern teaching; for he saw that the almost unbroken continuity of deposition from Cambrian to early Tertiary in the West gave direct contradiction to the generally accepted view that the post-Carboniferous revolution of Europe and of eastern North America had a world-wide extension; and more important still he saw that the areas of ancient crystalline rocks, both East and West, had not emerged from the sea to form the first land surfaces around which the deposition of Paleozoic strata began, as was ordinarily taught in colleges in the '70s, but that they represented long enduring and greatly degraded land surfaces of very ancient date, which on finally sinking beneath ocean level provided a sea floor on which Paleozoic strata began their deposition. Thus rationally guided, Gilbert gained from these three years in the West not only a wide acquaintance with the physical features of the Great Basin, but also a broad understanding of the structure, the physical history and the surface forms of the plateau province; and he gave an excellent account of that province as a field for geological study, although it is to Powell that credit is properly given for the earlier and fuller exploration of that wonderful region. On the whole it was experience rather than publicity that Gilbert gained on the Wheeler survey.

His work on the basin ranges is, however, an exception to the last statement, for the theory of their origin as fault-block mountains is widely associated with his name. Yet an adverse fatality seems to have attended all his relations to this fine problem. His first contribution to it, a novel but incomplete conception, was chiefly to the effect that, notwithstanding all the evidence found elsewhere for the uplift of mountains by lateral compression and folding, the basin ranges are best explained as individual vertical uplifts with little compression. Two additional elements of the theory were soon introduced by others; the occurrence of two periods of deformation, one marked by compressional folding, the other by disruptive faulting, was first suggested by King; and the occurrence of a long period of erosion between the two periods of deformation, as proved by the peneplanation of the earlier mountains of compression and folding before the uplift of the later mountains by faulting with little compression, appears to

have been first detected by Powell, who is seldom thought of in that connection. It is to be regretted that, during the discussion which brought out King's and Powell's suggestions, Gilbert remained silent, apparently on principle; he certainly had abundant opportunity to contribute to the discussion while he was in Salt Lake City, before he was overwhelmed with work on his return to Washington in 1881, but took no public part in it. And it is still more to be regretted that, after his first idea was extended by these suggestions from his seniors as well as by new observations and reflections of his own, and thus developed into a more completely reasoned form, it was never published.

The statement of his views which he made as censor of a paper submitted by another author to the Geological Society of America, although now brought out in an earlier section of this memoir, was not made public in his lifetime; and moreover it included no adequate mention of a long period of erosion between two periods of deformation. When he was about ready to break his long silence after a season of field work in the Great Basin in 1901, an unhappy accident discouraged him from reporting on the many excellent observations he had then recorded, and the accident is thus responsible for a serious loss to American science. The new facts that he discovered in support of Powell's erosional element of the basin-range theory remain unpublished in his field notebooks, except in so far as brief extracts of them are here presented on an earlier page; the evidence that he detected in confirmation of the occurrence of master faults along one or both margins of the ranges was presented orally at a meeting of the Geological Society and survives only in the memory of those who were fortunate enough to hear him; the newer inference of the oblique uplift of the ranges and the strong lateral extension of the region at the time of the faulting, by which subsequent observations led him to replace the earlier inference of vertical uplift with little compression, was published only as a brief assertion 13 years later in a paper on isostasy, where few have seen it. Even the essay on the basin ranges that was left unfinished at the time of his death was not, except for the Wasatch Range, as shown in the next chapter, carried far enough beyond a historical review to include an account of his observations in the field seasons of 1901 and of after years on the elevated and dissected peneplains which he recognized in the highlands of certain ranges, or an estimate of the amount of lateral extension indicated by the moderate slope of the fault surfaces; much less a discussion of the mechanism of such faulting. It is as if fate had been here working against him. Would that his life had been spared long enough and with sufficient strength to complete that essay; for his early statement of the basin-range problem, the only one that he published, although it includes one of his most original ideas, is the least complete of all his more important discussions. It is clear that he came to know vastly more about those ranges than he ever put into print.

THE POWELL SURVEY REPORTS

With Gilbert's transfer to the Powell survey in 1874, two years of great opportunity were opened to him for the serious investigation of large problems—the geology of the high plateaus and of the Henry Mountains—but his work on the high plateaus, which for some unexplained reason was allowed to duplicate Dutton's study of the same province at the same time by the same survey, was not published. On the other hand, his work on the Henry Mountains stands out as one of the most notable successes of his career. The problem was novel and interesting; the opportunity for studying it was planned to Gilbert's entire satisfaction; the solution reached was unusually gratifying; the report upon it was prepared with eager promptness and soon published in good form. A comparison of this most convincing discussion of the novel structures exhibited by laccolithic mountains, as prepared for the Powell survey, with the brief and unsatisfactory account of the equally novel structures exhibited by the basin ranges, as submitted in the earlier Wheeler reports, makes it clear that if favorable opportunity, such as was provided for the examination of the Henry Mountains, had been likewise provided for that of a few selected basin ranges, they also might have been treated with convincing clearness and completeness.

As to the Henry Mountains volume itself, it is characteristically free from lithological details and fully charged with critical description and reasoning about intrusive structures and

processes. The conclusions reached as to such structures have been widely accepted; Gilbert's name is more closely associated with laccoliths than with any other one class of geological phenomena; but his views as to the essential processes of laccolithic intrusion have rarely been appreciated. On the other hand, his remarkable analytical discussion in the third chapter of the same report which treats the principles of land sculpture on the basis of observation and reflection during five years of western exploration, has long enjoyed the high appreciation it so fully merits. As to the last three years on the Powell survey, they were not particularly fruitful, except in so far as they happened to give occasion for the study of Bonneville shore lines. The work then done on land classification and the possibilities of irrigation in Utah may be regarded as the nearest approach that Gilbert ever made to economic geology; and that work represented his interest much less than Powell's.

THE NATIONAL SURVEY: BONNEVILLE AND WASHINGTON

On the formation of the national survey, Gilbert again found opportunity widely opened in the study of Lake Bonneville under King, an investigation wholly to his liking, inasmuch as it was concerned essentially with physical geology and physiography; and then for a period of several years he had the aid of a small corps of assistants with headquarters at Salt Lake City. Plans made at the time show that he looked forward to a long-continued study of a large region, but his hopes in that direction were disappointed when, after only two years in the Great Basin, he was transferred to Washington, where for 11 years thereafter he was greatly distracted from the scientific tasks of his preference. His famous Bonneville monograph, slowly brought to a conclusion after many interruptions, was foreshadowed by partial statements in two annual reports of the director and anticipated by Russell's monograph on Lahontan, in the composition of which, however, Gilbert had an influential hand. When the Bonneville volume finally appeared it at once became a classic, and it still remains the most serious study of a large arid continental basin anywhere in the world. Apart from a few chemical analyses of lake sediments, a few experiments on sedimentation, and a short paleontological study, all conducted for Gilbert by others, this matured discussion is another example of Gilbert's preference for investigations that involve only keen field observation and critical mental interpretation.

The geological mapping of the Appalachians was in Gilbert's charge for a time at Washington, and appears to have been conducted on a plan that at least had his approval if it were not his invention; but the conventional subjects of stratigraphy and structure seem never to have absorbed his attention. The one Appalachian problem that did attract him was a novel one, namely, the physiographic interpretation of successive cycles of uplift and erosion on the basis of principles that he had come to understand in the West; but he was so occupied with duties of organization, consultation, administration, and supervision in the Washington office that this problem was largely solved by others before he reached it. The original work that he undertook during this decade of uncongenial indoor duties concerned the physiographic interpretation of Pleistocene lake shore lines in northern Ohio and western New York, and was accomplished chiefly in summer vacations; it may be regarded as an extension of his earlier study of the Maumee Valley in the light of his Bonneville experience. As he was at once engrossed with many other subjects when he returned to Washington, most of his observations were never reported in the detail that they deserved, and such brief articles as summarized them were published in unofficial form elsewhere than in survey volumes. But when the interpretation of lake shore lines led him to study the evolution of Niagara Falls, his most significant conclusions on that grand subject, which became peculiarly his own, were repeatedly presented in popular scientific lectures to delighted audiences; and even if the items of fact were concisely stated, such was not the case with the argumentation to which the facts led: that was searchingly elaborated. It may be here recalled that two essays of apparently paleontological nature—the "Age of the Equus fauna," a chapter in the Bonneville monograph, and the "Age of the Potomac formation," published apart in 1896, are both in reality concerned for the most part with physiographic questions.

LIBERATION FROM WASHINGTON

After 1892, the disaster year of the survey, Gilbert was liberated from administrative duties in the Washington office and was assigned, as now seems unfortunate, for three years, to a real work on geological formations and underground water in Colorado. As such work was largely of a standardized kind, of which any quantity could be done as it were to order by a standard geologist, it contrasted with many of his other studies, which were so novel in ideas and methods that he alone could carry them through. Hence here, as in his work on Bonneville and on the Appalachians, he had the aid of assistants. His report, published in 1896, has more of a perfunctory quality than any other of his publications. This Colorado work will probably be remembered not so much for the Pueblo folio which he and his assistants colored, as for his new interpretation of the fresh-water Tertiaries of the plains as fluviatile instead of as lacustrine deposits; and for his views on "Rhythms and geologic time," which may be long in maturing, but which seems destined to find important application. It was in connection with the field work in Colorado that Gilbert became interested in the chemical composition of shales and fire clays, of which a number of analyses were made at his request; but as usual this line of laboratory research was not carried far.

The physiography of Niagara and the Great Lakes again took his attention as soon as his task in Colorado was completed, and results of increasingly quantitative character were soon reached, along with many novel explanations of local features of glacial origin. Among the latter the cross-spur channels in the district about Syracuse, N. Y., with their dry cataracts and their plunge-pool lakes, deserve special mention as having afforded appropriate exercise for his power of original interpretation; for although they are matters of small magnitude they are of great theoretical import. The ingenuity of their treatment was truly Gilbertian. The appreciation then gained of the importance of glacial erosion in shaping the landscapes of western New York was a good prelude for studies of much more intense glacial erosion in Alaska in 1899, and a few years later in the Sierra Nevada. It is noteworthy that in each of these three regions the evidence discovered in favor of strong glacial erosion was of a purely physiographic kind; that is, it was not based on a study of the physics of ice, or upon the nature of glacier motion, or upon the volume of glacial drift, but directly upon the forms of the glaciated land surface; for to Gilbert's analytical mind every land form must have a responsible cause, and to his expert eye the cause was declared by the form. The geodetic problem of isostasy also engaged his attention in the nineties, and gave play to his spirit of philosophical inquiry as well as to his capacity for mathematical discussion.

It has been remarked that Gilbert was not of a reminiscent nature; he seldom talked of his past experiences and he never wrote of them, and the world is therefore the poorer. Even the contributions that he made to memorials of his chief, Major Powell, tell little about matters that were not relatively well known to the geological public, although it can not be doubted that the long intercourse between Powell and Gilbert would have furnished a large fund of interesting stories concerning the development of the national survey; but narratives of that sort do not seem to have appealed to him. As to his own more personal experiences he left no records; he left untold even so adventurous an excursion as Wheeler's boat party in the Colorado Canyon. It is probable that few persons ever heard him mention it. He was a forward-looking student of nature; his own part in his studies was seldom referred to.

WORK IN CALIFORNIA

Gilbert's brief return to the problem of the basin ranges in 1901 and its melancholy sequel have been told above. For a number of years thereafter Washington remained his winter residence, but California became the scene of his summer labors; and there in addition to subordinate physiographic studies in the Sierra Nevada and to a collateral but illuminating examination of the earthquake of 1906, two of his greatest investigations were undertaken. It was also during these later years, and while his attention was largely given to the work in the West, that he made additions to three problems of earlier date. One concerned the rate of recession of Niagara Falls and added new refinements to his earlier discussions; another explained the

convexity of hilltops, a physiographic problem the solution of which had eluded him long before in the land-sculpture chapter of the Henry Mountains report; and the third treated anomalies of gravity in relation to isostasy. The first two illustrated his usual combination of refined observation in the field with delicate mental interpretation; the third introduced a new and ingeniously devised postulate for geodetic discussions and exhibited his unusual capacity in the treatment of mathematical questions.

As to the two large investigations in California, it should be remembered that both of them were begun before and finished after his illness in 1909. One was the experimental and mathematical study of the transportation of detritus by running water; he had assistance in the experimental work, but appears to have made all the elaborate calculations himself. This study, in spite of the great amount of time and labor bestowed upon it, appears to have been like the much earlier study of a three-station method of measuring heights with the barometer, less fruitful than he had hoped it might be. The other was his truly wonderful study of the distribution of hydraulic-mining débris, from its source in the beds of auriferous gravels on the marginal uplands of the Sierra Nevada, along its devastating course on the valley plain of California, to its final seat in the bay-head marshes and deltas of San Francisco Bay, and even to its far-reaching indirect effect in the displacement of the submarine bar outside of the Golden Gate. Excepting the aid of an assistant for a few days in recording tidal movements in the bay, it is all Gilbert's own accomplishment.

It may be remarked that just as there is a very brief mention in the Henry Mountains report of the low value of that region economically, so here also a faint economic tinge is given in a brief consideration of the relative values of agriculture as developed on diked areas of bay-head marshlands and of commerce as dependent on the depth of water over the bar outside the Golden Gate; and similarly the edifying discussion of the earthquake of 1906 had a certain economic bearing in connection with the possibility of predicting shocks before they come and of decreasing destruction when they come; but these economic touches were in all cases very subordinate to what may be called the purely intellectual phases of his work, and never more so than in the admirable study of hydraulic-mining débris. Its contents have already been analyzed in an earlier chapter, and it was there characterized as a rival of the Bonneville monograph. It must be here praised again, for it is a marvelous combination of patient and laborious observation with critical quantitative interpretation. In spite of the serious illness in 1909, Gilbert's mental powers as here reflected seem undiminished in keenness, although they were of less continuous action than in his maturity 30 years earlier. Great must have been the consolation that he found in their exercise at a time when his capacity in other directions was waning. If any readers of the present memoir, young or old, have not yet read that California masterpiece of his, let them do so without delay. Furthermore, if the older of them have felt that advancing age will either prevent or excuse them from going on with their own labors, let them learn the lesson that Gilbert's last published report teaches, and look forward to their still older years as a time of continued opportunity. Let them remember also that Gilbert, on completing that last published report, at once entered upon another study and kept it in hand up to within a month of his death.

GILBERT'S PRESIDENTIAL ADDRESSES

The addresses that Gilbert delivered before the various societies that elected him to their presidency may be here reviewed in a group by themselves, although some of them have been reviewed on the foregoing pages. They ought to be brought together and published in a single volume, so valuable are they as original contributions to science and so beautifully do they illustrate the breadth of his competency and the wisdom of his methods. The first was the "Inculcation of scientific method by example," delivered before the Society of American Naturalists, at Boston, in December, 1885; it has been analyzed in an earlier chapter as an example of unusual capacity in penetrating and impartial analysis, as well as of a fair-minded spirit of scientific inquiry. The same society, having elected Gilbert as president for a second year,

heard another address on "Special processes of research," at Philadelphia, in December, 1886, in which the use of various graphic devices as a means of furthering investigation was considered. The public lecture on the "History of Niagara River," delivered at the Toronto meeting of the American Association for the Advancement of Science in 1889, also deserves listing here, as it was of high presidential quality. The address on "Continental problems" delivered before the Geological Society of America, at Ottawa, in December, 1892, was unlike the others in dealing chiefly with unsolved problems; one of its phrases, "go behind the postulates," deserves to be here again brought to the attention of every investigator. The address on the "Moon's face," delivered before the Philosophical Society of Washington in December, 1892, and here abstracted in an earlier chapter, was notable for the originality and ingenuity of its speculations in an astronomical problem, as well as for the minute study of lunar topography on which it was based, and for the amount of experiment by which its argument was supported. If this study be regarded as a trespass on the field belonging to astronomers, it should be remembered that Gilbert openly announced himself as "an advocate of the principle of scientific trespass," as quoted in a preceding section. An address before the Geological Society of Washington on the "Origin of hypotheses," in December, 1895, exhibits a remarkable capacity in maintaining a suspended judgment and suggests strongly that the author's interest in the mental methods of investigation was as great as, if not greater than, in the results gained; such was surely the case in this instance, for no positive results were here reached, yet the path followed in the attempt to reach them is described in detail and in such a way as to have delighted a critical audience. "Rhythms and geologic time," a presidential address before the American Association, at New York, in 1900, was a gradually developed exhortation in favor of a new line of research with the object of determining the duration of geologic ages. This was followed nine years later by the seventh and last address, on "Earthquake forecasts," reviewed in a preceding chapter, where it was regarded as one of the finest if not the finest of the series, an opinion that is confirmed after making the present summary.

If any one feature of these addresses deserves praise above the others it is the even-tempered and candid fair-mindedness by which they are all characterized. Next would come competence of treatment, and also such clarity of statement that every paragraph reads as if its thought had been easily conceived. Candor is a quality that always inspires confidence, and when it is coupled with ease of handling the pair are persuasively compelling. It was particularly because of the combination of these two leading qualities that Gilbert's work was so appealing to his associates; and it became all the more so because he replaced the advocacy and special pleading that so often weaken earnestness by a most judicial impartiality. He never overworked his evidence and never wielded an argumentative sledgehammer; his presentation was extraordinarily gentle, and in its gentleness lay much of its strength. That all these fine qualities should characterize the addresses made by one man shows that that man was possessed of true wisdom in a high degree; and it must be remembered that true wisdom far outweighs mere learning.

GILBERT'S PERSONAL INFLUENCE

It would be a serious error to imagine that Gilbert's influence on geology and geologists is to be measured chiefly by the volume of his scientific work. The manner in which his problems were treated and the form in which the results were set forth impressed many a reader of his reports and addresses as deeply as the results themselves. Moreover, his high personal character exerted a most beneficent influence upon all who came into contact with him, and they were many; for although most of his researches were carried on alone, he had abundant associations with a great number of other workers not only when, first as a senior geologist and later as chief geologist of the national survey, he was in official relations with its members, but for years afterwards when his acquaintance was sought unofficially and his advice valued by all who were fortunate enough to receive it. In his work as in his personality, the qualities of candor, fair-mindedness, and impartiality were as manifest as those of patience, carefulness, and unselfishness. He possessed that most enviable combination of force and gentleness which always compels respect; his force or will-power being manifested chiefly in the control of his own actions

and his gentleness in his dealings with others. His views were so reasonable and his suggestions were so persuasively excellent that he never had occasion to use authority to secure their adoption. It has been a great credit to American science that a man of Gilbert's nature should have been one of its acknowledged leaders.

All this is a matter of common repute in geological circles; but it is well attested by the acknowledgments of scientific indebtedness and by the protestations of personal gratitude and affection expressed in a collection of letters which were written by his friends at the suggestion of the director of the national survey which he had served so long and so faithfully, to greet him on his seventy-fifth birthday, May 6, 1918, but which unhappily failed to reach him, as his death occurred five days before. Such an outpouring of sincere feeling as these letters manifest shows that their writers were deeply moved in mind and in heart, perhaps all the more so because many of them knew that at the very time of their writing Gilbert was seriously ill, and because some of them knew also that if he recovered his health it was his intention to give up his residence in Washington, where many of the writers lived, and spend his remaining years in California: they were not only letters of greeting but letters of farewell.

It is difficult to say whether expressions of intellectual obligation or of affectionate admiration are in the majority. If some writers tell of the high appreciation in which they held Gilbert's reports of the 'seventies, where the glamor of a heroic age seemed to be reflected, others recall the sympathetic support and helpful counsel that he gave them at trying junctures. If some rightly regard his early accounts of the plateau province as containing the fundamentals of modern physiography and rejoice in the profound impressions gained from them; others remember with enduring pleasure his patience as a field teacher, his encouraging kindness, his cheerful companionship. Some are still mindful of the awakening instruction they received on first reading his report on the Henry Mountains; others look upon all his reports as models of scientific attack and of lucid exposition; or admire their universal saneness, or marvel at his faculty of hitting upon essentials and his capacity for going to the foundations of things. On the other hand, some treasure most the memory of his kindly smile of welcome, his winning manner, and his joyous laugh, which made those who met him the better for the meeting; and some give assurance that his many colleagues hold him in high esteem because of his generosity in sharing knowledge and his gentleness in imparting it; or report the gratification they felt on hearing expressions of warm regard for him abroad where he was looked upon as the leading American geologist; indeed, one excellent judge there ranked him with Faraday and Darwin. An occasional companion writes of the delightful and instructive hours spent with him, indoors and out; a long-time associate tells of the loving regard felt for the true worth of his character; and many younger men thank him for the aid they derived from his interest in their projects, for the help they received from his suggestions, and for the inspiration they gained from his writings; he clarified their ways of thinking and exemplified to them the true scientific spirit. The high standard of excellence in his work is thought to find its parallel in the forceful, concise, and persuasive way in which the results of the work are set forth; and the advanced position which geology has attained among us is declared to be largely due to the influence that he exerted upon his juniors by the finished quality of his studies. Indebtedness is repeatedly expressed for the completeness and accuracy of his observations, for his mastery of analysis, and for his clear presentation, his calm and restrained judgment; even the few who did not in all points agree with him did not fail for that small reason to recognize his fairness and courtesy in his dealings with them, or to pay tribute to his great scientific attainments and his personal merits. Those who worked under him in the earlier stages of their career treasure the memory of that formative association with him, and do not forget his lively interest in their welfare or his wish to give them opportunity, responsibility, and credit; and they assure him that now that they are older they remember all this and try in their turn to treat their own assistants with equal justice. One who can not claim to have known him closely, nevertheless regards him as the outstanding figure among the geologists of America and expresses the deepest admiration for his work and his nature; another who has known him well lays weight on the fineness of his personality, the unfailing kindliness of his attitude toward his associates, and the steadfastness of his friendships; one who knew him long and intimately saw in him the closest approach to human perfection.

Truly the years of a man are a blessing when, utilized nobly in faithful and constant labors, they are crowned in his old age with the heartfelt admiration and affection of all his friends.

SCIENTIFIC HONORS

When Gilbert was asked in 1904 to prepare a list of the honorary memberships to which Powell had been elected by various scientific societies, he refused to do so "because in the United States the enumeration of such matters is not good form." It is natural therefore that he left no list of his own distinctions of this kind, and it might be argued that, in view of his feeling as to good form, no list of such distinctions should be here presented. But to follow such a course would be to deny a very genuine pleasure to at least some readers of these pages, who will rejoice that their personal admiration of this great American was reflected in the official admiration expressed by various learned societies and universities; and it may be added that to omit mention here of these distinctions would be small courtesy to the societies and universities that conferred them. It is true that, in spite of the habitual indication of membership in the Royal Society of London by the letters F. R. S. commonly seen after the names of learned British scientists, and of the equally prevalent indication of association with one or another of the academies in Paris by the words "Membre de l'Institut" after the names of eminent Frenchmen, membership in the National Academy of Sciences in this country is not similarly indicated by the letters M. N. A. S., the Academy itself having voted against such display. But, on the other hand, it may be recalled that in the brief account of F. V. Hayden which Gilbert contributed to Johnson's Universal Cyclopedia, it is stated that Hayden "was a member of the National Academy of Sciences and of nearly all the other scientific societies of America, and an honorary member of many scientific societies in foreign countries"; and it must also be noted that in the brief paragraph he contributed to the same work on his own life, he submitted to the editorial addition of the four letters, M. N. A. S., after his name; hence it seems permissible that his honors, as far as they are known, should be listed in this memoir; but it may occasion surprise that, in view of his unquestioned eminence as a physiographic geologist, the list is not longer.

Various scientific societies of which Gilbert was a member and in the meetings of which he took part have already been named in connection with his scientific activities in three successive decades. The societies which elected him to office may be here named again. He was president of the American Society of Naturalists for two years, 1885 and 1886. In the American Association for the Advancement of Science, he was secretary of section E, geology, in 1885; chairman of that section in 1887; and president of the Association in 1900. He served the Philosophical Society of Washington as vice president from 1888 to 1892, and as president in 1893. He was one of the board of managers of the National Geographic Society from 1891 to 1900, except while he served as vice president in 1896 and 1897. He was vice president of the Geological Society of America in 1892 and president in 1893; he held the same offices in the Cosmos Club of Washington in 1893 and 1894; and in the Geological Society of Washington in 1894 and 1895. The Washington Academy of Science had his services as secretary in 1898, and as vice president from 1899 to 1904. Then after a period of freedom from office holding, he was elected to the presidency of the Association of American Geographers for 1908, and of the Geological Society of America for the second time in 1909. He was an excellent presiding officer and understood very well the importance of holding scientific meetings to the performance of their business, whether it concerned elections and accounts or the reading and discussion of papers. In spite of the large number of offices that he held, it may be truly said of him that in every instance the office sought the man.

Gilbert was elected to corresponding or honorary membership in various societies in this country and abroad. The first of these was the New York Lyceum of Natural History—later renamed the New York Academy of Sciences—which in 1870 elected him a corresponding member, probably after his winter at Columbia with Newberry; the second society to give him such recognition was the Kirtland Society of Natural History, in Cleveland, Ohio, by which he was made a correspondent in 1871, after his three-year service on the Ohio Geological Survey. Twenty years passed before similar action was taken elsewhere in this country; then he was

elected an associate fellow of the American Academy of Arts and Sciences in Boston in 1893, a corresponding member of the Brooklyn Academy of Arts and Sciences in 1895, a member of the American Philosophical Society of Philadelphia in 1902, and an honorary member of the Sierra Club of San Francisco in 1908. The first European society to discern his exceptional ability was the Geographical Society of Leipzig, which in 1886 elected him to corresponding membership; he was elected a foreign member of the Geological Society of London in 1895, and of the Royal Geographical Society of London in 1896; he was made an honorary member of the Geographical Society of Berlin in 1898, and a corresponding member of the Royal Scottish Geographical Society of Edinburgh in 1901, and of the Geographical Society of Geneva in 1904. It is significant that with the single exception of the Geological Society of London, noted above, these six foreign elections were all to geographical societies; the inaction of foreign geological societies suggests that they were less interested, because less informed, in physiographic geology than in other and older branches of geological science; and the inaction of other geographical societies similarly suggests that they were unconscious of the immense importance of physiography in geographical science. However, Gilbert was made a foreign member of the Royal Academy of the "Lincei" of Rome in 1904, and a corresponding member of the International Commission on Glaciers in 1905 and of the Bavarian Academy of Munich in 1907. But that a man of Gilbert's eminence should have been elected only to these three foreign memberships in addition to one in a geological society and five in geographical societies must make those Americans who have received a larger recognition in Europe ask themselves on what grounds their election and his non-election were based.

The University of Rochester naturally and properly conferred the honorary degree of doctor of laws on Gilbert in 1896, as he was one of its most famous alumni. The University of Wisconsin gave him the same honorary degree at its jubilee in June, 1904; and a geologist being president of that university at the time, Gilbert's masterly formulation of the principle of erosion was mentioned as the especial reason for thus distinguishing him. He was discriminately described as "preeminent in the development of physiography; geologist of the first rank; scientist of balanced judgment; deep interpreter of nature." The same degree was also bestowed upon Gilbert by the University of Pennsylvania in 1908, when he was greeted with the statement:

> You have enriched geology by a series of studies and contributions of striking originality and importance, which have led to new conceptions of the earth's history and opened new fields of investigation. As a pioneer in what may almost be called the American science of physiography, your work is recognized and valued the world over as being of the highest significance.

In 1900 the famous Wollaston gold medal of the Geological Society of London was awarded to Gilbert "in recognition of the value of his researches concerning the mineral structure of the earth and, more particularly, of his important contributions to physical geology, and specially to the geological history of the American continent." The words, "mineral structure of the earth," here used appear to be repeated from the conditions of the endowment for the medal, as they recur from year to year when it is voted; surely they were not literally interpreted in this case, for of all geological subjects, the mineral structure of the earth received very little attention in Gilbert's work. Even the more special subject, "the geological history of the American continent," was seldom the main theme of Gilbert's studies, although the touch that he gave to the early chapter of continental submergence instead of emergence, and the many touches that he gave to the latest or physiographic chapter were of the highest significance. However, on the announcement of the award at the annual meeting of the society the president, William Whitaker, appropriately declared the recipient of the medal to be "a worthy successor of his countrymen, James Hall and J. D. Dana, as our Wollaston medallists, for his work is not only American but appeals to the world at large." It is singular that Gilbert's physiographic work was not specified, for Whitaker had many years before led British geologists to an understanding of the subaerial denudation of the Weald in southeastern England where the chalk scarps had been previously interpreted as sea cliffs. It is singular also that neither on this occasion nor on any other similar occasion was the quantitative element of Gilbert's geological

work selected for mention, although after impartial objectivity that element is its most marked characteristic. It may be added that, although this well-merited distinction from London undoubtedly gave real pleasure to its recipient, it does not seem to have elated him unduly, for after briefly referring to it in a letter to a relative, he added, probably more in jest than in earnest: "But the thing I am proudest of is a prize I won at progressive euchre this week." In 1909 he received the gold medal of the National Geographic Society of Washington "for original investigation and achievements in physiographic research," and in 1910 the American Geographical Society of New York, of which he had been a member since 1889, gave him its Daly medal for "contributions to geographical science."

An exceptional distinction came from the Boston Society of Natural History, when it conferred upon him, in April, 1908, the Walker grand prize, which is "bestowed once in five years on a naturalist of the United States whose services have been so preeminent that they seem to the council deserving of especial recognition." In transmitting the prize, the council declared:

> The great number and remarkably high quality of your contributions to science, the thoroughness and originality of your researches, and the noble generosity with which you have assisted others in their investigations, constitute a claim upon our consideraion so strong that we feel that all the scientific men of the country will welcome the announcement of the award with cordial appreciation.

The grand prize was a check for $1,000; and Gilbert wrote to a friend that on seeing the amount, it literally took his breath away. The letter which inclosed the check was preserved, and attached to it is a memorandum, signed "G. K. G.," "that this money might not be lost to science after its first duty, it was dedicated to research, and given, September, 1909, to ——— ———, to use in the investigation of California rifts." Whether the Boston society ever knew and approved of this overscrupulous dedication of its prize to use by some one else than the scientist of preeminent services whom it selected to bestow it upon does not appear.

The last honor that Gilbert received was election in February, 1918, to foreign membership in the Royal Society of London, an honor that is regarded by those who confer it as the highest scientific distinction British science can award. It is pleasant to record that his nomination was presented to and, with adequate support, carried through the council of the society by the same geologist who, when 30 years younger, had been so disappointed at the Bath meeting of the British Association by Gilbert's indifference to his inquiring interest in the laccolith problem, but who, generously overlooking that small matter, recognized at its full value the worth of Gilbert's lifelong geological work, and who, knowing that the formal announcement of the election would be delayed, at once wrote an informal announcement which Gilbert received about two months before his death.

CHAPTER XXXII

GILBERT'S LAST STUDY

RETURN TO AN OLD THEME

It appears from an earlier chapter that, in spite of the rich opportunity for further study among the mountain ranges of the Great Basin, as shown by his own work on the House Range and by Louderback's excellent paper on the Humboldt Ranges, Gilbert turned his attention for a number of years to other matters, as if the calamity of 1901 had produced a revulsion of feeling and given him a distaste for what had before so greatly attracted him; and it is noteworthy that, as has already been pointed out, chief among other matters then taken up was the quantitative investigation of the transportation of débris, to which he had long before given a penetrating qualitative analysis in the third part of his Henry Mountains report. It is now to be told that, on the completion of the débris investigation, he turned back in the last four years of his life to the structure of the basin ranges, the problem that he had opened up even before his first qualitative analysis of débris transportation. This subject continued to occupy him until within a month of his death in 1918.

Unhappily, by reason of failing health, this final return to the subject of his earliest studies in the West was not carried to completion. It nevertheless included several field excursions, on which a surprising amount of observation was accomplished. On the way to San Francisco in 1914 he stopped for a fortnight or more to examine the western base of the Wasatch Range near Ogden; one of his letters at that time notes:

> My last two days in Ogden were profitable. I struck some geology that will figure well in the next report.

Of more importance was an excursion made in company with a friend from Berkeley in 1916 to the Klamath Falls district in southern Oregon, where some fault block ridges of very recent displacement, as indicated by their exceptionally fresh scarps, had been discovered a few years before. On the way there he noted that the eastern face of the Cascade Range at the point where he crossed it appeared to be defined by a fault with a strong drop to the east after the principal lava eruptions by which the range is largely covered, but before the erosion of the deep canyons which now dissect it. Around Klamath, the relief was seen to be manifestly of tectonic origin; erosional valleys are found only in the upfaulted blocks, the intermediate depressions are all aggraded. One fault scarp, worn away above and covered with talus below, exhibits an intermediate part of its sloping surface over a length of 110 feet and a height of 43; it is scored up and down with slickensides. In another such surface he found the hade of the fault to be 70°, thus indicating a horizontal displacement three times greater than the vertical. The field work was enjoyed, but writing an account of it progressed slowly, for his health was only "holding its own and a little more."

It was in the summer of this year, 1916, that a definite decision was made to return to his old subject, the basin ranges: "I have taken up a new job," he wrote to a correspondent while he was visiting his son in Utah, "to write on the structure of these mountain ranges. It is an old theme for me, but I have data gathered in 1901 and 1914 and shall do a little field work this year. . . . The mountains, and especially their bases, have stories to tell me." He rode about in his son's car, and was surprised as well as pleased to see how readily he could thus cut across country through the sage brush. The Klamath excursion was apparently a part of this new job.

The Wasatch front was revisited in May and June, 1917, when several weeks were spent driving along the foothills between Ogden and Provo. There also slickenslided surfaces were found cutting off the rocks of the mountain front near its base at a relatively low angle and thus indicating that a very considerable forward movement of the depressed block had accompanied its downthrow. During the winters of these later years Gilbert prepared the histori-

cal review of theba sin-range problem and the account of the Wasatch Range to which reference has already been made, and was about to supplement them with the results of his field studies of other ranges in 1901 and later; but his strength was then so far reduced that he could spend only a small part of each day in writing. He fully recognized the uncertainty that attended the completion of his plans and talked freely of the possibility that he might not be able to carry them through.

THE WASATCH RANGE

Extracts have already been made in an earlier chapter from Gilbert's historical review of the basin-range problem; it remains to present here some of the most significant passages from his final account of the Wasatch Range. This occupies about 200 typewritten pages, and it is as keenly critical and analytical and demonstrative as any essay he ever wrote. It is a perfect example of scientific acumen and impartial equanimity; and it shows that his mental capacity was unaffected by his loss of bodily strength. He not only patiently analyzed the various interpretations that have been proposed for the observed facts, but he carefully criticized the analyses by which the interpretations are reached. If there be a fault in the discussion, it is in the overelaborate demonstration that the Wasatch escarpment can not have been produced by such agencies as ocean waves, glacial erosion, or wind action. Gilbert's own opinion, quoted below, that this chapter is "worth publishing" is a most modest estimate of its value. Had other ranges been afterwards treated with equal fullness, the resulting report would have been without question Gilbert's greatest work. It is quite impossible, on reading the Wasatch chapter, not again to feel deep regret that a great part of his time after liberation from Washington in 1893 was not given to a comprehensive examination of the mountain ranges of the Great Basin. He did not believe in the inclosure of scientific preserves, and he would surely have welcomed cooperation of others in his studies; yet the origin of those mountain ranges was peculiarly his problem. Moreover it was a greater problem than any other that he undertook; the history of Lake Bonneville was a simple story in comparison; the evolution of Niagara as a phase in the history of the Great Lakes was more complicated, but it did not approach in complexity the origin of the basin ranges. And there is every reason for believing, from the results that he had already reached, that if he had been able to examine a good number of the basin ranges he would have been led to set forth a theory of mountain making involving not simply vertical uplifts, as he had long before suggested, but strong crustal extension; and that would have been even a greater novelty than his idea of fault-block ranges.

The Wasatch Range, 130 miles in length, rises some 4,000 feet above the adjoining plains and has a dozen peaks which rise from 1,500 to 4,000 feet higher still. It is composed of a great variety of rocks, including formations from Archean to Jurassic in age.

Following the Jurassic deposition was a diastrophic revolution. The formations were folded and faulted on a grand scale and among the faults were extensive overthrusts. The intrusion of the granite bodies is assigned to the same period. The mountains and valleys created by these dislocations were not coincident with the present mountains and valleys. . . . A profound syncline crosses the Wasatch . . . at right angles. At the south the axis of an important anticline enters the present range obliquely; and at the north the upbuilding by overthrust is not expressed by the existing relief.

The topography created by dislocation was so far reduced by erosion in Cretaceous time that a transgression by the sea spread sands of Dakota age over worn edges of Jurassic, Triassic, and Upper Carboniferous strata. The evidence of that transgression has been found only on the eastern flank of the range. After the retreat of the sea were other crustal changes, and then an important period of crustal quiet in which the general movement of detritus was from west to east and a broad area at the east received terrestrial and lacustrine deposits. These deposits, referred to the Eocene, rest, in the Wasatch district, on all formations of earlier date, from Archean to Cretaceous. . . . The diastrophism to which the greater features of the present relief are due is clearly post-Eocene, but the date of its beginning cannot be yet assigned. Its changes have continued to the present time, but only a small fraction are post-Pliocene.

One crustal change of the Tertiary system was of broad or epirogenic character and resulted in a general reversal of the slopes controlling drainage. The field of Eocene deposition became an upland, and a western district which had been a field of Eocene degradation became a lowland. . . . A coordinate crustal change was orogenic, the creation of mountain ridges and intermont troughs. The Wasatch range was separated from the chain of broad valleys that follow its western base by a great fault. . . . Up to the present time only physio-

graphic characters have been adduced in support of the view that the [Wasatch] escarpment was created by faulting. In my judgment the physiographic evidence is adequate, but not all are prepared to accept it, and for this reason I shall begin the discussion of the subject by presenting evidence other than physiographic. Afterward several classes of physiographic evidence will be taken up.

STRUCTURAL EVIDENCE OF FAULTING

After a preparatory statement concerning the nomenclature of faults and their physical effects, a number of localities are given where such inferred effects are duplicated in facts, such as slickensided surfaces and sheets of sheared rock fragments, in a plane on the basal slope of the dissected mountain front. Some of the observed effects appear to have been produced under heavy pressure at a great depth, others at a small depth.

Despite the great contrast between the two groups of facts, they are really harmonious—under the hypothesis that faulting on this plane created the entire escarpment. If the faulting began when the tops of the valley block and mountain block were at about the same level, that part of the footwall which is now visible at the base of the escarpment lay initially at a depth beneath the surface roughly equivalent to the present height of the escarpment, plus the amount of degradation of the mountain block, and the frictional effects in that part of the footwall were conditioned by the corresponding pressure. As faulting progressed the pressure on that part of the footwall gradually diminished, with the result that deformation within the wall rock ceased after a time. . . . That part of the fault rock near what is now the base of the escarpment was worked over, with gradually diminishing pressure. . . . The phenomena are all explicable on the hypothesis that they were produced by faulting and that the western fault block is concealed by valley deposits. So far as I know there is no alternative hypothesis to be discussed.

An important modification of earlier statements regarding the simple pattern of the fault line along the mountain base is then introduced:

In characterizing ranges which have a bold front due to faulting, I wrote in 1890, "the line separating the rock of the mountains from the alluvium of the valley is simple and direct." The statement is perhaps defensible because the context compares such a line with the corresponding line on the back slope of a range consisting of a tilted block; but it is fair to say that my conception of the flexuosity which may affect fault outlines has been materially enlarged by more recent studies of the Wasatch front. . . . The line of the fault is crooked. It not only curves through considerable arcs, but in two places turns sharply at right angles; and in three other places its course runs for miles athwart the general direction of the fault as a whole. Some of the deflections, but not all, are evidently associated with preexistent structural features. . . . One of its greatest deviations is in passing around the granite body north of Alpine. . . . It is evident that great simplicity of basal outline is not an essential characteristic of faulted range fronts. On the other hand, the phrase "simple and direct" may properly be used in a relative sense when basal outlines of faulted fronts are compared with rock basins [bases?] shaped chiefly by erosion. The erosive outline . . . is greatly influenced by the arrangement of drainage lines and by the distribution of strong and weak rocks; the fault-block outline is independent of drainage lines and its adjustment to rock structure is partial and local; and these differences serve to make the erosive outline comparatively tortuous, and the fault outline comparatively direct.

The bearing of these facts and inferences on the basin-range problem is then summarized.

The structural data set forth in the preceding sections as proof that a fault created the western face of the range are mainly new, receiving here their first publication. They were unknown to those who early recognized the fault by means of its physiographic features, and to those who afterward discussed the physiographic evidence. So as a matter of history they constitute supplementary evidence, and their function has been to confirm a conclusion already reached. They are here given first place because they are supposed to appeal to the understanding of those geologists whose opinions are little affected by physiographic evidence and the physiographer's reasoning. In the following pages I propose to present and discuss the physiographic data, not primarily for the purpose of adding evidence of the existence of the fault, but in order to determine if possible the value and also the limitations of physiographic characters as criteria for the discrimination of fault-block mountains.

A number of mountain salients, lower than the main mass of the range, from 5 to 7 miles in length along the range front and 3 or 4 miles in breadth, are then discussed, described under the name of "fault-block spurs," and explained as "the upper parts of crustal blocks that are separated by normal faults from the Wasatch block on one side and from the valley block on the other, so that they stand lower than the one and higher than the other." Three of these spurs "are capped in whole or part by alluvium from the Wasatch range, alluvium that was once part of the piedmont apron of the range. . . . The presence of this alluvium and its dislocation from the general modern alluvial apron of the range show that the spur blocks were once so

associated with the valley block as to receive the same deposit, and have since been lifted with reference to the valley block. . . . It is noteworthy that all four of the fault-block spurs are accompanied by thermal springs, the waters rising along their outer bases. I know of but two localities of thermal springs on the line of the frontal fault of the range."

PHYSIOGRAPHIC EVIDENCE OF FAULTING

Scarps in the piedmont alluvium near the mountain base are given first place among physiographic features indicative of faulting. A review of them is thus closed: "The preceding statement of facts and considerations bearing on the origin of the piedmont scarps has a regrettable prolixity because it is the record of an endeavor to free my own mind from an initial bias in favor of one hypothesis by a thorough examination of its rival," namely, local slips in the alluvium without movement on the mountain fault blocks; and the result of this examination is the discovery of a series of minor features by which the two kinds of displacements may be distinguished. "The scarps created by such surface movements can not be confused by the physiographer with the piedmont scarps" due to true faulting.

The features of the range escarpment itself, especially the ends of many lateral spurs or "ribs" into which the escarpment has been divided by the erosion of ravines and canyons, are next considered. "There are a few angles in the outline [base line], but in the main it swings in free curves. Along this line end the ribs of the range. The base line, or rock base, of each rib end runs somewhat directly from the mouth of a canyon to the mouth of the next canyon. It has indeed some curvature, but the curvature is part of the general curvature of the range base, and has little or no relation to the character or size of the rib. It is in as many places concave toward the valley [piedmont or intermont lowland] as convex. To express this character briefly, we may say that the rib ends are aligned. . . . A row of rib ends . . . seems to be part of a once continuous plane or gently flexed surface, the outer surface of a great original rock body out of which the canyons have been carved. . . . A range front characterized by the terminal facets of truncate ribs is appropriately said to be facetted." Ravines and canyons are found to be truncated at the mountain base as definitely as the mountain ribs; their sides vary with the nature of the inclosing rocks, but their beds are narrow, and whatever the nature of the rocks, "the narrowness of the bed continues to the rock base and there ends abruptly. . . . The canyons hold their characters to the plane of the facets and there lose them. In the same sense in which the ribs are truncate the canyons are truncate."

Truncation of structures is next taken up. The strike of the range strata is occasionally parallel to the range base, but along "not less than seven-eighths of the range front, the strike meets the rock base at a notable angle. The greatest fold of the range, a syncline bringing a great series of Paleozoic and Mesozoic formations in succession to the range base, runs athwart the range in the latitude of Salt Lake City. . . . The obverse of these facts is that the line of the range base crosses the strike lines of the structure at all angles. The range front transects the range structure. The structures that are exhibited in the range front are truncate, just as are the features of relief." Then comes a beautifully deliberate interpretation of truncation such as could have been written only by a geologist of matured wisdom:

> Men are prone to take the familiar as a matter of course and to ask the meaning of the strange only; and by our manifestation of this trait we gelogists are assured of our humanity. To one who lived only in a glaciated region, the forms of landscape peculiar to ice erosion suggest no mystery, and it is only through acquaintance with the dominant topography of aqueous origin that he is able to recognize the strongly marked characteristics of the work of ice. The illustration is drawn from personal experience, but its moral finds its support in the circumstance that those who have undervalued the power and accomplishment of glaciers have been dwellers in lands once overrun by Pleistocene ice sheets. It has occurred to me, as I wrote of the phenomena of truncation, and especially the truncation of canyons and of structures, that such phenomena may be so familiar as features of mountain fronts that not all students of orogeny feel the need of their explanation, and I shall therefore preface a consideration of the meaning of truncation by a brief account of a group of escarpments whose characters stand in contrast.

The escarpments chosen for this lesson are those of the great stairway, "described by Powell in one of the classics of physiography," by which ascent is made from the plateau in which the Colorado Canyon is cut northward to the high plateaus of Utah.

Every canyon broadens downstream, flaring widely as its stream leaves the strong bed. The head of each minor stream is in a reentrant angle of the escarpment. The outline or ground plan of each escarpment is a series of convex scallops, large and small, the larger scallops being spaced by the major drainage and the smaller by the minor drainage. The canyons and gullies of the drainage correspond to the canyons of the Wasatch front, and instead of being truncated at the basal outlines of the escarpments they determine reentrants of the escarpments. The ridges between lines of drainage correspond to the ribs of the Wasatch, and instead of being truncated along the bases of the escarpments they determine the salient scallops of the escarpments and their bases. . . . Such structures as faults and folds, instead of being truncated by the escarpments, serve as important conditions for the control of the courses of the escarpments.

It is therefore concluded that—

the forms of escarpments created by stream erosion are largely controlled by the structure of the eroded terrane and by the pattern of its drainage. Because flowing water is the dominant agency in the sculpture of the land, the form types it produces constitute the norm, and departures from its types call for explanation. When therefore so important a form element as the basal outline of an escarpment is found to be independent of the rock structure, independent of the drainage pattern, and independent of the distribution of inter-canyon ridges (which distribution is of course a function of drainage pattern), so that the basal outline appears to cut off or truncate these features, the phenomenon is one requiring explanation.

In the case of the Wasatch escarpment the explanation is at hand, for the base line of the escarpment is also the line of a great fault, characterized by an abundance of the phenomena created by fault friction and followed through long distances by [alluvial] piedmont scarps. The escarpment is evidently the face of an uplifted tectonic block, and the adjacent depressed block is buried under waste from the sculpturing of the uplifted block. The structure elements of the uplifted block end at the fault line because actually cut off by faulting. The ribs end where the block from which they were carved ends. The canyons also end where the block ends, and they end without flaring because the upfaulting is too rapid to give their streams time for lateral corrosion.

The evanescent character of some of the physiographic evidence for block faulting is clearly stated:

The frontal facets to which attention has thus far been directed are all, in a topographic sense, young. Because the Wasatch block is actively rising, they have not been long exposed to wasting agencies; and because their rocks are strong they have wasted little. The Wasatch facets in weak rocks are comparatively worn and indefinite: and the original facets of [other] block mountains, now old and stagnant, have been worn out and replaced by rib ends of a different type. . . . The Wasatch escarpment is characterized by topographic youth. The fault that has produced it is an active fault, and the fault's activity is vigorous. . . . The youth and strength of the escarpment give it special distinction as the representative of a type. . . . If the activity of the Wasatch fault should cease, the processes of erosion would gradually mutilate and eventually obliterate the characteristic features of the escarpment.

CONFIRMATION OF PHYSIOGRAPHIC BY STRUCTURAL EVIDENCE

It is probable that those who have expressed the most pronounced opposition to the fault-block theory of the basin ranges proper would not take exception to the faulting of the Wasatch Range, as thus demonstrated; and they might indeed fairly enough argue that the faulting of the Wasatch did not prove the faulting of the ranges farther west. But that the faulting of one proved the faulting of the others was not the least Gilbert idea. His idea was that if physiographic evidence of faulting in the Wasatch is shown to be trustworthy by its systematic association with structural evidence of faulting, then physiographic evidence may be accepted as proof of faulting when found in other ranges where structural evidence is wanting.

Because the physiographic characters of the Wasatch did not prove misleading it seems proper to infer that similar characters may be used as criteria for the recognition of faults at the margin of other ranges.

It was evidently his intention to bring forward in later chapters of his report the physiographic evidence that he had found in 1901 for the block faulting of other ranges; but those later chapters were never written.

THE WASATCH FAULT BLOCK

The faulting of the Wasatch being thus doubly assured, important conclusions are announced as to the attitude of the fault surface and the measure of the displacement upon it. The dip of the fault surface near the mountain base at seven localities varies between 29° and 45°, and averages 35°. The throw, or vertical component of the displacement, near Twin

Peaks in the Cottonwood district is estimated to be at the very least 7,000 feet, and more probably as much as 10,000 feet; and this requires a slip or slanting movement down the fault surface of 17,900 feet and a heave or horizontal displacement of 14,800 feet. It is this enormous movement of horizontal extension to which reference has been made in earlier pages. Had similar extension been found to characterize other fault-block mountains, Gilbert would surely have had some interesting generalizations to present as to the origin of such movements if his report had been carried to completion.

In conclusion the Wasatch is regarded as a great crustal block, with an average width of 8 or 9 miles. Its eastern margin is tentatively explained as due to the down-faulting of a narrow slab or slabs, by which a series of "back valleys" was initiated, although the down-faulting there was much less than on the west. Several transverse streams which cut canyons through the range from east to west are regarded as antecedent to the block faulting. As to the pre-faulting form of the region:

> Before the range arose its site—or at least part of its site—was a land tract of low relief. This is shown by remnants and other vestiges of a peneplane. . . . At the north the peneplane was developed on Archean and Paleozoic rocks, at the south on Carboniferous. . . . These imperfectly correlated data . . . serve to show that the period of diastrophism to which its [the range's] growth belongs is distinct from the period of the great post-Jurassic revolution, being separated from it by the period of quiet in which the peneplane was developed.

Thus at the end Gilbert makes perfectly explicit statement of the long period of undisturbed erosion which Powell had, in some almost intuitive manner, detected nearly 40 years earlier, and of which Gilbert himself had found evidence in the summer of 1901. His note-books of that year, as already cited, make it clear that the same long period of erosion was recorded in some of the isolated basin ranges which he then studied.

How unavoidable is the regret that he was not spared with bodily health to carry through to completion this superb product of his unimpaired mentality!

FINAL ILLNESS AND DEATH

The study of the basin ranges was still in progress when, in his seventy-fifth year, Gilbert reached the decision to remove from Washington and make his home in California. On arriving in San Francisco 15 years before, he had spent 30 hours without seeing a soul he knew; it was then "a big, lonesome city." But as the years passed he formed many close friendships there and in Berkeley across the bay, and at last came to wish that his remaining years might be spent in that delightful region. With this object he began the journey westward in the spring of 1918, but stopped at Jackson between Detroit and Chicago to see his sister, as he had done so many times before on his travels across the country. While there his health failed so much that he was removed from her house to a hospital which, curiously enough, had been built on the ground where the sister's residence itself had stood for many years; and it was from the hospital that, in the month before his death, he sent his last official letter, addressed to his then successor in the position of chief geologist of the survey. After briefly mentioning the cause of his detention at Jackson and the small likelihood of his being again as well as he had been, he went on:

> Nevertheless I hope to be able to continue work on the report in hand—the Structure of the Basin Ranges—and shall try to complete it. . . . Chapter I of my report, a historical introduction, is written. So is Chapter II, "Wasatch Range," which contains new material on the range and opens the discussion of the value of physiographic evidence of block-faulting. In my judgment these chapters will be worth publishing, even if I fail to complete the work. There is a chapter on the faults of the Klamath region, written with care, so as to make full record of some observations while they were fresh. It has been my purpose to recast this after other chapters have been written. . . . The chapter on hand is on the Fish Spring and House ranges and is intended as Chapter III. It contains new material gathered in 1901. With a view to continuing the work whenever I have the strength, I am sending west my assembled material, but as the journey will be at my own expense the draft on Survey funds will be small. An outside estimate for the current fiscal year will be $150.

The journey was not continued; the report was never finished. What strength remained was given chiefly to setting affairs in order. On April 7, after much suffering the week before, Gilbert wrote, in a greatly altered hand, to his elder son, Archibald, in San Francisco, asking him to come to Jackson to take charge of certain business matters; he felt it was time to "cut

clear" of those responsibilities but was "not able to do the cutting." The page closed: "My mind will be relieved when I learn you are willing to come. . . . . This letter means a great effort to me." Happily he was more comfortable afterwards. He wrote a little later to his younger son, Roy, and told him of the completion of a fatiguing piece of work: "Your letter came just after I achieved a success with my monthly accounts—making the sum of the credits equal the sum of the debits—but it took a long time in several sessions." The fifty-first of his little pocket diaries contains a few records after he went to the hospital, but they are shorter and shorter as the days pass. April 12: "Unsuccessful attempt to sit up—too weak." April 13: "In wheeled chair a few minutes." April 24: "Emma and I play a rubber of cribbage." April 26: "Arch arrived from Cal." April 27: "Arch and Emma call." April 28: "Arch." Two days later he wrote a farewell letter to a cherished friend, and on May 1 passed quietly away, his sister and his eldest son being with him at the end.

Gilbert's death occurred five days before his seventy-fifth birthday, in celebration of which, as has already been told, a host of his geological colleagues had written him congratulatory letters that he never received. It is sad to think that he could not read the many messages of esteem and affection that these letters contained, and it is sad also to know that, great as was his accomplished work, beneficent as his influence had been, his final wish to review and complete one of his earliest and greatest studies was not realized. Nor was his intention to establish himself near San Francisco brought to completion. All plans had been made for the journey across the continent in the spring; a chosen friend was to join him on the way so that he should not go alone to the new home he was to make in California, a home not so far as Washington from the field in which he hoped to work. It was as if, even at the age of threescore and fifteen, he still looked forward to finding a new life beyond the mountains he had so often crossed before. Alas, the companionable journey across the Great Divide was not made; he crossed the Greater Divide alone.

G. L. Goodale

NATIONAL ACADEMY OF SCIENCES

Volume XXI
SIXTH MEMOIR

BIOGRAPHICAL MEMOIR GEORGE LINCOLN GOODALE

1839-1923

BY

B. L. ROBINSON

Presented to the Academy at the Autumn Meeting, 1924

GEORGE LINCOLN GOODALE

1839–1923

By B. L. Robinson

In reviewing a scientific career, its time and place become factors of the first importance. So rapid has been the advance of knowledge, so swiftly has effort passed to new fields, so intensive has specialization become, that a retrospect even of half a century has grown surprisingly difficult. To form a just estimate of personal achievement it thus becomes necessary to grasp the difficulties of the epoch and to bear carefully in mind the limitations of the environment. Only in this way is it possible to perceive the true nature of the obstacles surmounted, to measure the advance attained, and to appreciate the individual contribution to progress.

George Lincoln Goodale was born at Saco, Me., August 3, 1839, the son of Stephen Lincoln and Prudence Aiken (Nourse) Goodale, and died in Cambridge, Mass., April 12, 1923. His father was a man of energy and rare ability. A pharmacist by early training and profession, he brought his knowledge of chemistry, excellent for the time, to bear upon many problems of economic importance, and his activities in this direction ranged from the preservation of food products to the manufacture of commercial fertilizers. He took a great interest in fruit growing and arboriculture in general. He became a person of importance in his State, and as secretary of the Maine Board of Agriculture for many years edited their copious and well-known reports. In his own publications he dealt with the animal as well as the plant side of agriculture and his Principles of Breeding was a highly regarded treatment of the subject.

His son therefore grew up in an atmosphere charged with intellectual interests and had constantly before him the example of endeavor in the field of applied science. His early attention to chemistry, his choice of medicine as a profession, his attraction to the physiological aspects of plant life, his breadth of scientific interest, and his enduring sympathy with the applied aspects of science, all can be with fair certainty traced to paternal example and training.

In boyhood he spent a year in his father's pharmacy. Then entering Amherst College in 1856 he took the usual prescribed course of the period, coming under the instruction of Prof. Edward Tuckerman in botany and Prof. Edward Hitchcock in geology. He received his A. B. in 1860. For a year thereafter he remained at Amherst as assistant in chemistry. There he began his medical studies, which in the succeeding year were continued in the Portland School for Medical Instruction and later at the Harvard Medical School, where he took his M. D. with distinction in 1863, receiving the same degree the same year from Bowdoin College.

Soon after his graduation at Amherst and during his medical studies he was selected to coöperate in a scientific survey of Maine, an ambitious project which had received the indorsement and support of the State legislature. In this work his duties included botany, chemistry, and some geology, and in 1862 and 1863 he published several reports upon its progress. In these he dealt with the flora of newly explored regions and the chemical analyses of waters from various springs. He held at one time the position of State assayer and also prepared some unpublished geological maps mentioned in the official reports of the survey. These diverse activities were in several ways significant in his career. They brought him into coöperative relations with a notable group of young scientific men destined to make good and become widely known in their respective fields, as, for instance, with C. H. Hitchcock, the geologist, and A. S. Packard, the entomologist, and, what was still more important, his botanical work brought him into correspondence with Dr. Asa Gray. The survey itself lasted but two years and then succumbed to the disturbed conditions of the war time, but even in the few months of its active prosecution attained creditable results and prepared the ground for more intensive cultivation at a later period.

Doctor Goodale's medical practice, though extending through only three years, brought considerable diversity of experience, for, in addition to private practice, he held several official positions, being city physician in Portland, examining surgeon in the Navy, and contract surgeon in the Army. He also taught in the Portland Medical School.

In 1866, being in ill health, he made the first of his longer journeys, going to California by the Panama route, thus gaining his first view of tropical vegetation. His health was soon restored and he extended his journey to several of the Western States.

On his return to Maine he married Miss Henrietta Juell Hobson, to whom he had for some years been engaged and who through a long life was his devoted and sympathetic companion.

Doctor Goodale was in 1868 appointed to the Josiah Little professorship of natural science at Bowdoin College and there taught both in the medical school and in the collegiate work, giving in the latter instruction in chemistry, mineralogy, botany, and zoology, for about four years. In 1872 he was called to Harvard as university lecturer and instructor in botany. In the following year he was advanced to assistant professorship, and in 1878 to full professorship in this subject.

It was his privilege, as well as one of his considerable contributions to science, to relieve Dr. Asa Gray of much routine work in instruction, thus freeing Doctor Gray's time for his long-projected Synoptical Flora of North America, toward which he had been gathering during many years an overwhelming mass of material that only his trained judgment could have treated to equal advantage.

Doctor Goodale's first work at Harvard was his general introductory course in phanerogamic botany, developed chiefly along the lines of gross morphology with ideals similar to those of Eichler, of Sachs's Textbook, and of Gray's lucid Structural Botany. To morphology of such nature he added much of a physiological and an anatomical character dealing effectively with the different vegetable tissues and their component elements, with assimilation, respiration, transpiration, germination, growth, and plant movements, as well as with reproduction, variation, and evolutionary problems.

He was a finished lecturer. Dignified in presence and agreeable in voice, he had an impressive manner which gave weight to the information he imparted. He was never hurried, but chose his words with great care and passed logically from point to point. He was a good ex tempore draftsman, often sketching on the blackboard in a few well-placed strokes the plant structures or anatomical details he was describing. Each drawing was done with a fine definiteness and the minimum of erasure and correction. He never hesitated to employ the now perhaps too generally discarded method of precise definition of terms or structures. Had this been done with less skill it might easily have tended to the dogmatic, but this effect he cleverly avoided. His auditors were carried away by the clearness of his presentation. He was never tedious, never discursive, though, in a dignified way, often humorous. He had his matter well in hand and there was no repetition, no talking against time, no pause to assist memory. Not given to much theorizing nor to philosophic generalization, he stayed close to concrete facts and in stating them never confused his hearers by over-emphasis of doubts or exceptions. Hundreds of students recall his lectures with gratitude and admiration. It is to be remembered too that these students were the same who daily listened to such teachers as Norton, Shaler, Francis Child, William James, Farlow, Palmer, and Royce, all of them men of great originality and force, distinguished stylists in their respective fields.

In the laboratory Doctor Goodale was less successful than on the platform. He was always kindly and exceedingly courteous to his students, but a trifle impersonal, guiding their work with rather general directions, which often left them in some vagueness as to his wishes. Research in the concrete sense in which it had taken form in the laboratories in France and Germany had scarcely been transplanted to American botanical laboratories beyond the field of taxonomy, and student publication of merit was exceptional before the middle eighties. The elective system had not long been in vogue and was still in its youth. Laboratory equipment was as yet scanty and very imperfect. Its use was chiefly to demonstrate known facts and processes, and almost never for student research. Graduate study was rare and little organized. There was much to change and to develop.

Doctor Goodale was keenly alive to the need of improvement in apparatus, increase of equipment, larger and better laboratories, more commodious auditoriums, and vastly increased and diversified collections of illustrative material. Toward these ends he planned and worked patiently for years. He early realized the importance of publicity for scientific work, and with his special gift as a speaker did much by popular lectures to stir the public to interest in and cordiality toward scientific work and ideals.

He gave several courses of lectures, at the Lowell Institute in Boston, at the Cooper Union in New York, and elsewhere. They were well attended and aroused enthusiasm. Vegetable physiology was in the seventies, eighties, and early nineties essentially novel in America and Doctor Goodale had much skill in demonstrating and explaining, even to popular audiences, the leading facts and fundamental principles of this subject. He was ingenious in devising effective illustrations and was one of the first to give successful lantern projection to moving objects such as currents in protoplasm, the escape of oxygen during the assimilation of aquatic plants, etc. Such vital processes, shown in action upon the screen, naturally attracted no small interest in days when the electric lantern was still unknown and the cinema a thing undreamed of.

Doctor Goodale was a very successful teacher in the Harvard summer school, and through its medium exercised a wide influence upon the methods and ideals of many alert teachers, both men and women, young and middle aged, who took back to their own work greatly increased enthusiasm from a few summer weeks thus spent under his stimulating instruction. They spread his fame in remote parts of the country, and others came to seek like opportunity.

In 1879 Doctor Goodale consented, at the solicitation of Dr. Asa Gray, to undertake the oversight of the Harvard Botanic Garden, and was appointed its director by the president and fellows of the college. This was a task which for several reasons had much difficulty. The garden was unendowed. Its expenses were constantly increasing. The rapid deterioration of the then wooden-framed conservatories, the adverse influences of dust and smoke from a growing city; the carelessness and occasional vandalism of visitors; the scarcity of trained gardeners; the often conflicting ideas of the university boards, of botanical and horticultural colleagues, as well as of the visiting public, regarding the appropriate aims for such an establishment and the lines of development it should be given; the proper adjustment of its functions to instruction, research, experimentation, and acclimatization; its relations to the laboratories and museums; the demands upon the available supplies of plants and flowers for decoration on occasions of academic celebration—all these problems were superimposed upon the inherent difficulties of keeping a host of delicate and costly exotics healthy in cramped space and unfriendly climate. Doctor Gray, who had long experienced such trials, once characteristically remarked that he did not wonder that Adam fell if he had to live in a garden.

With great patience, tact, and evenness of temper Doctor Goodale discharged for many years the duties of this exacting position. The garden under his directorship was kept at a high level of efficiency. It functioned notably as an object lesson in the great diversity of plant life. No less than 7,000 perennial species were often in cultivation there at the same time, as well as a varied assortment of annuals. The conservatories were enlarged and improved. The planting was given a variety of horticultural features to attract the public. Drainage and grading were bettered. An endowment was started. A skilled, Kew-trained gardener was secured, and a liberal policy inaugurated in supplying material not only for the diverse botanical activities in the university but often to neighboring institutions as well.

In the later seventies and early eighties Doctor Gray, relieved of his teaching by his energetic younger colleagues Goodale and Farlow, and of his curatorial routine by the extraordinarily industrious and methodical Sereno Watson, projected an ambitious collaborative work which in four volumes was to summarize the science of botany. Viewed in retrospect, this undertaking can be clearly seen to have involved well-nigh impossible difficulties, and there can be no surprise that it remained unfinished. The first volume was a restatement of external plant morphology by Doctor Gray himself. This subject was fairly concrete. It had already been treated several times by Doctor Gray, whose masterly lucidity and good sense of proportion had long been recognized in Europe as well as America. Furthermore the

subject, though by no means completely investigated, had long been pursued and was relatively well matured. Thus, without inordinate difficulty, Doctor Gray was able to bring out his Structural Botany as the first volume of the proposed series.

The second volume, which devolved upon Doctor Goodale, was to give similar summary of the anatomical and physiological aspects of the flowering plants. Here the conditions were very different. Instead of arranging a selective presentation of subject matter which had in a measure become definite, if not actually static, it was necessary to give, so to speak, an instantaneous view of a host of facts, interpretations, processes, and theories, which were at the time themselves moving, changing, being multiplied, and rearranged with astonishing rapidity.

Plant morphology, the subject treated by Doctor Gray, was in great measure an independent one. Not so plant physiology, where investigation is intimately bound up with chemistry and physics, and its success or failure may depend upon concurrent research in some remote field such as optics or crystallography, the perfection of staining reagents, or unexpected discoveries regarding electrical phenomena.

Doctor Goodale wrote his volume and it appeared in 1885. In the space of about 500 pages he compressed a vast amount of matter, summarizing his subject probably as well as its nature permitted at that time.

There can be little doubt that the work proved disappointing to its author. It was rather too compendious for a laboratory guide, and in the rapid advance of plant anatomy and physiology could not long hold its own as a work of reference. Nevertheless it had some years of great usefulness, and there can be no doubt whatever that it was at its date by far the best work on its subject which had appeared in America. Furthermore, its influence upon subsequent educational works in its field has been considerable.

It has been a matter of widely felt regret that the third volume in the series, which was to have been an introduction to cryptogamic botany by Dr. W. G. Farlow, was never completed.

It is less generally known that a fourth volume was also projected, which Doctor Gray described as "a sketch of the Natural Orders of Phænogamous Plants, and of their special Morphology, Classification, Distribution, Products, &c." This, by his own statement, it was his hope rather than his expectation to draw up himself.

On the completion of his Physiological Botany, as his volume was generally called, Doctor Goodale turned his attention largely to certain pressing matters somewhat euphemistically termed "organization." To those who work in institutions supported by governmental, State, or municipal appropriations, it can scarcely be realized what complications arise in partially endowed institutions ambitiously expanding and constantly forced to take on functions and maintain establishments of increasing expense. To finance such undertakings it is necessary to secure the interest of persons of substance inclined to constructive liberality. The matter is one requiring consummate tact. There must be the ability to attract favorable attention, to present specific needs clearly, to command respect, and to inspire confidence.

In all these requirements Doctor Goodale was exceptionally gifted, and he secured the cordial interest of an extended group of persons who repeatedly aided his undertakings with liberality and remained throughout life his devoted friends. His soliciting always had a fine dignity. It was clear that it was impersonal in nature, for high purpose and unselfish ends. His largest single undertaking of this nature was to secure the needful funds to build the botanical section of the university museum. This was to furnish quarters appropriate to the existing needs and immediate expansion of the department of botanical instruction, both as to laboratories and lecture rooms. It was to have rooms also for private offices, library, cryptogamic collections, and ample space for museum exhibits of illustrative botanical material which had long been accumulating at Harvard in a somewhat desultory manner, and which, though already including many objects of rarity and value, had never had proper organization. To this material it was Doctor Goodale's ambition to add objects far more attractive to the public.

This building enterprise, of considerable magnitude for its period, was carried through by Doctor Goodale in a surprisingly short time, and was completed in 1890.

It provided for botany at Harvard quarters coordinate with those previously erected for zoology and ethnology and subsequently added for geology and mineralogy. It thus gave the science its proper place in the comprehensive scheme initiated by the Agassizes. The building is of impressive dimensions and has many excellent features. It is a serious businesslike structure in which architectural embellishment has been completely eliminated and the ends sought have been simplicity, space, and durability. It was "mill built." It was a notable advance upon what had previously existed. It must not be judged by standards of construction only at a later date rendered possible by unexpected advances in steel framing, reinforced concrete, electric devices, or metal furnishing.

Having thus secured for the university the needful housing for a botanical museum, Doctor Goodale set himself seriously about the task of assembling exhibits appropriate to popular illustration of his science.

In this task he encountered difficulties of a general and psychological nature as well as those of concrete detail. He was well aware that previous attempts to make a botanical museum a thing of popular interest had met with little success. Plant life itself lacks much of the human appeal which can be aroused by the clever preparator who stages a pair of nesting birds, a beaver diligently engaged in feats of surprising construction, a serpent charming its prey, or an insect astonishingly obscured by protective mimicry. Nor do dried plants compare as museum objects with varied minerals, precious stones or meteorites, nor yet with models of canyons, volcanoes, atolls, and other surprising geological phenomena. Still less are they comparable in popular interest with archeological exhibits depicting primitive humanity in its homely occupations. It was clear that a botanical section in a general museum, if it was to hold its own, must include objects of much greater esthetic appeal than wood samples, fibers, gums, or grains, and far more immediate interest than dried fruits, nuts, or cones. Models would have to be constructed which would give lifelike representation of the plants themselves with details of form and color. This ambition was not a new one, but the results attained, up to that date, in plaster, wood pulp, or wax had been either extremely crude and clumsy or else of a perishable nature.

While seeking a practicable solution of this problem, Doctor Goodale was attracted about, 1885, by some exceptionally lifelike models in glass of marine invertebrates made by Leopold and Rudolph Blaschka. Conceiving that the unusual talent thus shown might attain the desired ends, if directed to plant structures, Doctor Goodale entered into correspondence with the Blaschkas and not long after visited them at their home in Meissen, near Dresden. They were at first reluctant to undertake subjects so remote from their previous experience, but were soon induced to prepare some sample models of flowers and plants.

These were forwarded to Cambridge, but were shattered in transit. Undiscouraged, Doctor Goodale saw even in the fragments such evidence of ability on the part of the artists that he showed the pieces to several influential friends. Among these were Mrs. Elizabeth C. Ware, of Boston, and her daughter, Miss Mary Lee Ware, who took an immediate and gratifying interest in the undertaking and promised it their support. Business details were arranged and the Blaschkas, father and son, entered upon a contract extending through a number of years and securing to the Harvard Botanical Museum their entire output.

The notable, indeed unique, collection of glass models of plants, flowers, fruits, vegetable structures, and anatomical details is too well known to need description. It was entirely and very liberally financed by Mrs. and Miss Ware as a memorial to their husband and father, Dr. Charles E. Ware. Its success as a popular and drawing exhibit was immediate. Within a few weeks of its installation the attendance at the museum greatly increased and at times was more than doubled.

With a central feature so notable, it was then possible to group attractively and with telling effect in adjacent rooms exhibits of much cleverness to inform the public regarding a great variety of vegetable structures and products. To the most telling disposition, protection, and labeling of these Doctor Goodale gave painstaking attention.

During his professional career Doctor Goodale traveled much. He visited Europe nine times and, for a man who had not been educated there, came to have a very unusual familiarity with the foreign laboratories and museums. He was a linguist of ability and acquired a broad knowledge of foreign literatures, public affairs, trade relations, colonial enterprises, tropical agriculture, and a host of matters contributing much to his powers as a broad administrator of a museum.

In 1890 and 1891, in company with a cousin, Capt. (later brigadier general) Greenleaf Austin Goodale, he made a journey of great length visiting Egypt, Ceylon, Australia, Tasmania, New Zealand, Java, and the Straits Settlements, as well as several points in China and Japan.

His primary object was personally to view the notable botanical establishments at places like Peradeniya, Buitenzorg, Melbourne, Syndey, and Tokyo, to establish friendly relations with their directors, and to secure by purchase or exchange a choice selection of objects suitable to the further development of the Harvard Botanical Museum and Garden. In these matters he met with gratifying success. Among the exhibits obtained many were unusual and several—such as a living specimen of the fern *Todaea barbara* (*T. africana*) and a gigantic rata log, a lignified aerial root some 5 feet in diameter—were doubtless at their time unique in America.

More and more Doctor Goodale turned his attention to the economic side of botany and took much interest in the problems of tropical agriculture. Among these was the improvement of the sugar cane. To further experimental work in this field the Harvard Botanical Garden was able to establish, through the influence of Doctor Goodale and with the generous financial support of Mr. Edwin F. Atkins, of Boston, a tropical garden and experiment station in Cuba, at Soledad, near Cienfuegos.

Here not only many sugar canes, but a variety of other tropical plants of economic importance, were brought together for observation and experimental purposes. Doctor Goodale made several journeys to Cuba in the interests of this enterprise, which is now being further developed and already forms a notable instance of North American scientific effort brought to bear upon tropical economic problems.

Doctor Goodale was a member of many societies both scientific and social. He was a presiding officer of unusual ability, managing business with smoothness, speaking little himself, but directing cleverly the discussions of others. His good judgment, ready and sympathetic interest, and his uniform courtesy made him a valued member on many committees. He was in 1889 vice president of the American Association for the Advancement of Science, and the following year its president. His retiring address, delivered at Washington, was entitled "Useful plants of the future." He was vice president of the Boston Society of Natural History from 1887 to 1890, and its president during the year 1891–92. He was one of the founders of the New England Botanical Club and was its president from 1897 to 1899. He received the honorary degree of A. M. from Bowdoin in 1869, and of LL. D. from Amherst in 1890, from Bowdoin in 1894, and from Princeton in 1896.

With manifold duties of instruction and administration, the care of the botanic garden, financing and development of the museum, with wide professional correspondence and constant attention to the improvement of working conditions in his science, Doctor Goodale had little opportunity for personal investigation. It is probable he was right in judging that his special talents could be most effectively turned to other ends. However, he had a sympathetic interest in the research of others, and many of his publications took the form of appreciative reviews to give wider publicity to their results.

His writings, though numerous, were for the most part brief. Except for his Physiological Botany, already discussed, he published but one work of size, namely, The Wild Flowers of America. His part in this was to supply appropriate letterpress of a popular scientific nature to accompany 50 colored plates painted by Isaac Sprague, the leading American botanical artist of the period. The task had no great scope, but was conscientiously performed, and the resulting work has been highly prized by many flower lovers and, long anticipating the nature-study publications of the present day, gave popular instruction to many readers who could enjoy its clearly written text and striking, colored plates, though they would have been unlikely

to make use of any work of more technical character. The undertaking well illustrates Doctor Goodale's confidence that science, for its own advancement and best interests, must make popular appeal.

His early writings covered a wide range of subject matter—chemical, medical, pharmaceutical, horticultural, and agricultural, as well as botanical. In later years his publications were mostly reviews, synopses for class use, official reports, and addresses.

From 1888 to 1920 he was an associate editor of the American Journal of Science, and to this he contributed notes and reviews in considerable number. Many of his early writings were printed in newspapers or in popular journals of a transient nature, and in later years it is believed that he from time to time contributed by request to the unsigned editorial matter of several periodicals, including the Nation.

For these reasons it would be well-nigh impossible at this date to form a complete bibliography of his writings. Happily Prof. Robert Tracy Jackson, while preparing his excellent sketch of Doctor Goodale for the Harvard Graduates' Magazine, took great pains to familiarize himself with his published output and to this end drew up a very full and as yet unprinted list of his writings. This he has generously contributed for use in the present memoir and, with slight emendations, it is here appended. The portrait accompanying this memoir is here reproduced by courtesy of the editorial board of Rhodora.

In 1888 Doctor Goodale was appointed to the Fisher professorship of natural history, a chair long held by Doctor Gray. In 1909, after 38 years in the service of Harvard University and no less than 36 years as professor, Doctor Goodale resigned and was thereupon appointed, by the president and fellows of Harvard College, Fisher professor emeritus and honorary curator of the botanical museum. To the extent that declining health permitted he continued to give the museum his care and thought for more than a decade thereafter, directing its growth, conducting its correspondence, and exerting influence toward its financial support.

With social gifts of an exceptional nature, Doctor and Mrs. Goodale early gained an acquaintance of unusual extent and were able to make their home one of delightful hospitality, where the guest at once felt breadth of sympathy and where conversation naturally turned to matters of importance and themes of interest. They had five children, of whom only two reached maturity, namely, Joseph Lincoln Goodale, who has for many years been a distinguished surgeon of Boston, and Francis Greenleaf Goodale, a lawyer of Weston, Mass., practicing in Boston.

Doctor Goodale's final illness was of a gradual and at times painful nature, but was borne patiently and referred to, if at all, with characteristic humor.

It would be impossible to summarize in a few words the achievements of a life so full of varied activities. It was one of devoted and patient service to science. Its ends were neither spectacular discovery, nor detailed investigation, nor yet the production of technical treatises. Its guiding motives were to improve the conditions of the student, to extend the opportunities of the investigator, and above all to convey to a wider public important messages of scientific truth.

BIBLIOGRAPHY

Compiled by Robert Tracy Jackson

1862

Botanical report [in the] Scientific Survey [of Maine]. Sixth annual report of the secretary of the Maine Board of Agriculture, pp. 125–129.

Botanical notes on the new lands. Ibid. pp. 361–372, with 3 text figures.

Chemical report [in the] Scientific Survey [of Maine]. Ibid. pp. 443–456.

A catalogue of the flowering plants of Maine. Proc. Portland Soc. Nat. Hist. vol. 1, pp. 37–63. [Continued in part II of the same volume published in 1869. See below.]

1863

Botanical report in the Scientific Survey of Maine. Second annual report upon the natural history and geology of the state of Maine, 1863. pp. 120–128.

[On the geology of Vinal Haven.] Ibid. pp. 264–266.

[Geological observations in Maine.] Ibid. pp. 281–283.

Reconnoissance of the Schoodic Valley. Ibid. pp. 313–323.

[On the Penobscot and St. John Valleys.] Ibid. pp. 333–343.

[On iron.] Ibid. pp. 415–422.

1867

The geysers of California. Amer. Naturalist, vol. 1, pp. 337–342.

1868

Portland catalogue of Maine plants. [A checklist drawn with some changes from the catalogue prepared by Dr. G. L. Goodale and Rev. Joseph Blake and published in Proc. Portland Soc. Nat. Hist. vol. 1, pp. 37–63 (1862) and pp. 127–138 (1869).] Portland. Cover, pp. 1–12.

1869

A catalogue of the flowering plants of Maine. [With the collaboration of the Rev. Joseph Blake.] Proc. Portland Soc. Nat. Hist. vol. 1, pp. 127–138. [Conclusion of the catalogue begun in vol. 1, pp. 37–63, which were published in 1862. See above.]

1870–1872

[Under joint editorship of Cyrus Fogg Brackett and George Lincoln Goodale] Bowdoin Scientific Review. Brunswick, Maine, nos. 1–51, Feb. 15, 1870, to Sept. 17, 1872.

1873

[Review.] On a new method of observing the rate of growth in plants; by E. Askenasy. Am. Journ. Sci. ser. 3, vol. 6, p. 231.

[Review.] On the general occurrence of starch in sieve cells; by Dr. Briosi. Ibid. p. 231.

[Review.] The influence of temperature on the development of *Penicillium glaucum*, blue mould.—Prof. Wiesner. Ibid. p. 231.

[Review.] Normal formation of fatty substances in chlorophyll; by G. Briosi. Ibid. p. 390.

[Review.] Physiological researches in regard to germination; by Ph. Van Tieghem. Ibid. pp. 390–392.

1876

[Review.] Phaenological observations in Giessen; by H. Hoffmann. Ibid. ser. 3, vol. 11, p. 414.

1877

[Note on a monstrosity in apple blossoms.] Proc. Boston Soc. Nat. Hist. vol. 18, p. 354.

[Note on vegetable parasitism.] Ibid. p. 359.

1878

Vegetable histology. Johnson's New universal cyclopædia. New York. pp. 1109–1110.

Vegetable physlology. Ibid. pp. 1110–1117.

[Review.] Transpiration in plants. Wiesner. Am. Jour. Sci. ser. 3, vol. 15, p. 156.

[Review.] Non-sexual outgrowths on fern prothalli. DE BARY. Ibid. p. 403.
[Review.] Absorption compared with transpiration. VESQUE. Ibid. ser. 3, vol. 16, pp. 485–486.
On the causes of the abnormal shapes of plants grown in the dark. Ibid. p. 486.
[Review.] Davenport's notes on *Botrychium simplex*. Amer. Nat. vol. 12, p. 181
[Review.] Ferns, in their homes and ours. JOHN ROBINSON. Ibid. pp. 624–625.
Hybridization in plants. Twenty-fifth Ann. Rep. Mass. Board of Agric. for 1877, pp. 156–159. Boston.

1879

[Published anonymously.] Outline notes [on botany] with titles of works of reference. Cover, pp. 1–16. 16° Cambridge, Mass.
Recent researches in regard to seeds. Twenty-sixth Ann. Rep. Mass. Board of Agric. for 1878, pp. 262–285. Boston.
The floras of different countries. Library of Harvard Univ. Bibliog. Contrib. no. 9. 12 pp. Cambridge, Mass.
Guides for science teaching, no. II. Concerning a few common plants. Published by Boston Soc. Nat. Hist. Cover, pp. 1–61.
[Review.] Cane-sugar in early amber cane.—Professor GOESSMANN. Am. Journ. Sci. ser. 3, vol. 17, p. 488.
[Review.] On the causes of the change in form of etiolated plants; by Professor GODLEWSKI. Ibid. p. 494.
[Review.] The botanical text-book (Sixth ed.), Part I. Structural botany, or organography on the basis of morphology . . . ASA GRAY. Ibid. ser. 3, vol. 18, pp. 73–76.

1880

Outline of a course of four lectures on some relations of botany to horticulture. Delivered by Prof. GEORGE L. GOODALE of the University at Cambridge before the Worcester County Horticultural Society, A. D. 1879. Trans. Worcester Co. Hort. Soc. for year 1879. 12 pp. Worcester, Mass.

1881

Concerning a few common plants. Boston Soc. Nat. Hist. Guides for science teaching. 61 pp. Ginn, Heath & Co., Boston. Second ed. See above under 1879.
The Botanic Garden at Cambridge. Harvard Register, vol. 3, pp. 22–25. [With collaboration of Asa Gray and Alexander Agassiz.] Jan., 1881.
[Review.] Eucalyptographia; a descriptive atlas of the Eucalypts of Australia and the adjoining islands; by Baron FERD. VON MÜLLER. Am. Journ. Sci. ser. 3, vol. 21, pp. 249–251.
[Review.] The flora of Essex County, Mass.; by JOHN ROBINSON. Ibid. p. 251.
[Review.] Botany of California, vol. 2; by SERENO WATSON. Ibid. p. 251.
On the origin of starch grains; by A. F. W. SCHIMPER. Ibid. p. 330.
[Report on the Botanic Garden.] Ann. Rep. of the President and Treasurer of Harvard College, 1879–80. pp. 95–99. Cambridge, Mass.

1882

Wild flowers of America. With fifty colored plates from original drawings by Isaac Sprague. Text by George L. Goodale. pp. 2, 1–210, 50 pl., 4°. S. E. Cassino, Boston.
[Report on the Botanic Garden.] Ann. Rep. of the President and Treasurer of Harvard College, 1880–81, pp. 96–101. Cambridge, Mass.
Report on the Botanic Garden. Ann. Rep. of the President and Treasurer of Harvard College, 1881–82, pp. 103–106. Cambridge, Mass.

1883

Color and assimilation. Science, vol. 1, p. 332.
On the development of chlorophyll and color granules. Ibid. p. 421–422.
[Review.] On the occurrence of formic and acetic acids in plants, and their physiological importance in metastasis; by Dr. E. BERGMANN. Am. Journ. Sci. ser. 3, vol. 25, pp. 161–162.
[Review.] Direct observation of the movement of water in plants. M. JULIEN VESQUE. Ibid. pp. 237–238.
[Review.] Color and assimilation.—TH. W. ENGELMANN. Ibid. pp. 312–313.
[Review.] On the influence of sunny and shaded localities on the development of foliage leaves; by E. STAHL. Ibid. pp. 313–314.
[Review.] Jahrbuch des K. Botanischen Gartens und des Botanischen Museums zu Berlin. Band II, 1883. Ibid. pp. 479–480.
[Report of the] Director of the Botanic Garden. Ann. Rep. of the President and Treasurer of Harvard College, 1882–1883, pp. 116–119. Cambridge, Mass.

1884

[Review.] Handbuch der Botanik . . . N. J. C. MÜLLER; Pflanzenphysiologie . . . W. PFEFFER; Vorlesungen über Pflanzenphysiologie . . . JULIUS SACHS; Traité de botanique . . . PH. VAN TIEGHEM; Handbuch der Botanik . . . A. SCHENK. Am. Jour. Sci. ser. 3, vol. 27, pp. 322–326.

[Review.] Die Pflanzenkrankheiten . . . B. Frank. Am. Jour. Sci. ser. 3, vol. 27, pp. 415–416.
[Review.] The structure and function of lenticels.—Heinrich Klebahn. Am. Jour. Sci. ser. 3, vol. 28, p. 239.
[Review.] The microchemical detection of nitrates and nitrites in plants.—Molisch. Ibid. p. 239.
[Review.] Ou the structure and growth of palms.—Brauner. Ibid. pp. 239–240.
[Review.] On the physiological significance of water glands and nectaries; by Walter Gardiner. Ibid. pp. 240–241.
[Review.] Respiration and transpiration of fungi.—Bonnier and Mangin. Ibid. 241.
[Review.] Das botanische Practicum . . . Strasburger. Ibid. pp. 474–475.
[Review.] Essentials of botany . . . C. E. Bessey. Ibid. p. 475.

1885

Vegetable histology, 8° 194 pp. illus. New York. [Advance reprint from Gray's Botauical Textbook, vol. 2. Physiological Botany.
Practical exercises in histology and physiology. 8° pp. [2] 2–36. Ibid.
Physiological botany. I. Outlines of the histology of phaenogamous plants. II. Vegetable physiology. Practical exercises. 8° pp. xxi. 499 [2], 36, 214 illust. Ivison, Blakeman & Co., New York and Chicago. Gray's Botanical Textbook (sixth edition), vol. II.
[Review.] Reserve carbohydrates in fungi.—Leo Errera. Am. Jour. Sci. ser. 3, vol. 30, p. 489.

1886

Ten lectures on physiological botany, delivered before the Women's Educational Association (1886), reported by E. S. Hatch. New England Journal of Education, Apr.–June, 1886, vol. 23, pp. 196–197; 215; 231; 245; 262; 277; 309; 326; 342; 373.
[Review.] Plasmolytic studies . . . Hugo de Vries. Am. Jour. Sci. ser. 3, vol. 31, pp. 157–158.
Absorption of coloring matters by the living protoplasm of vegetable cells. Am. Jour. Sci. ser. 3, vol. 32, p. 486.

1887

A course of ten lectures on the phenomena presented by ligneous plants. With an account of the probable origin, the classification, and distribution of the shrubs and trees of New England. Delivered in March and April, 1887, before the Women's Educational Association. Reported for the Boston Journal by Miss Hatch.

First lecture. Anatomy of a tree. Boston Semi-weekly Journal, Mar. 18, 1887, vol. 54, no. 22, p. [3].

Second lecture. Physiology of a tree. Boston Journal Suppl. Mar. 19, 1887, vol. 54, no. 17693, p. [3]; [also in] Boston Semi-weekly Journal, Mar. 22, 1887, vol. 54, no. 23, p. [3].

Third lecture. Palms and their allies. Boston Morning Journal, Mar. 22, 1887, vol. 54, no. 17695, p. [1]; [also in] Boston Semi-weekly Journal, Mar. 25, 1887, vol. 54, no. 24, p. [2].

Fourth lecture. Our coniferous trees. Boston Journal Suppl. Mar. 26, 1887, vol. 54, no. 17699, p. [1]; [also in] Boston Semi-weekly Journal, Mar. 29, 1887, vol. 54, no. 25, p. [2].

Fifth lecture. Our apetalous trees. Boston Morning Journal, Mar. 29, 1887, vol. 54, no. 17701, p. [4]; [also in] Boston Semi-weekly Journal, Apr. 1, 1887, vol. 54, no. 26, p. [3].

Sixth lecture. Trees with showy blossoms. Boston Journal Suppl. Apr. 2, 1887, vol. 54, no. 17705, p. [2]; [also in] Boston Semi-weekly Journal, Apr. 5, 1887, vol. 54, no. 27, p. [3].

Seventh lecture. The care of trees. Boston Morning Journal, Apr. 5, 1887, vol. 54, no. 17707, p. [2]; [also in] Boston Semi-weekly Journal, Apr. 9, 1887, vol. 54, no. 28, p. [2].

Eighth lecture. Forests and climate. Boston Morning Journal, Apr. 12, 1887, vol. 54, no. 17713, p. [1]; [also in] Boston Semi-weekly Journal, Apr. 15, 1887, vol. 54, no. 30, p. [2].

Ninth lecture. Forest products. Boston Journal Suppl., Apr. 16, 1887. [Also in] Boston Semi-weekly Journal, Apr. 19, 1887, vol. 54, no. 31, p. [3].

Tenth lecture. Hints as to the management of trees and shrubs in landscape gardening. Boston Morning Journal, Apr. 20, 1887, vol. 54, no. 17720, p. [2]. [Also in] Boston Semi-weekly Journal, Apr. 22, 1887, vol. 54, no. 32, p. [3].

A method of subjecting living protoplasm to the action of different liquids. Am. Jour. Sci. ser. 3, vol. 33, pp. 144–145.
The Botanic Garden [including report on the projected Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1885–86, pp. 119–122. Cambridge, Mass.
Obituary. William Boott. Am. Jour. Sci. ser. 3, vol. 34, p. 160.

1888

Address of Professor Goodale. University of Michigan, 1837–1887. The semicentennial celebration of the organization of the University of Michigan, June 26–30, 1887. pp. 190–191. Ann Arbor.
A course of lectures on forests and forest products, Lowell Institute, Boston, Feb. and Mar., 1888. [Syllabus of 12 lectures in 12 unnumbered sheets.]

On a combination of the auxanometer with the clinostat. Am. Jour. Sci. ser. 3, vol. 35, pp. 258–259.
Recent contributions to our knowledge of the vegetable cell. Ibid. pp. 419–420.
Assimilation by colored and variegated leaves. Am. Jour. Sci. ser. 3, vol. 36, pp. 159–160.
Hough's American Woods, Part I. Ibid. p. 160.
Heather in Townsend, Mass. Ibid. pp. 295–296.
Color granules in flowers and fruits. Ibid. pp. 472–474.
List of writings of Asa Gray chronologically arranged. [Prepared with the collaboration of Sereno Watson and assistance of William Gilson Farlow, Charles Sprague Sargent and William Francis Ganong.] Am. Jour. Sci. ser. 3, vol. 36, Appendix pp. 3–42. Index, prepared by Arthur Bliss Seymour, pp. 1–25.
List of the writings of Asa Gray chronologically arranged with an index. [Reprint of the preceding continuously paged.] cover, pp. 1–68.
Sketch of the life and work of Dr. Asa Gray., Proc. Boston Soc. Nat. Hist. vol. 24, pp. 191–198.
[Review.] Das pflanzenphysiologische Practikum . . . DETMER. Am. Jour. Sci. ser. 3, vol. 35, pp. 87–88.
[Review.] Gray's Elements of Botany. Garden and Forest, vol. 1, pp. 11–12.
[Review.] The Kansas forest trees identified by leaves and fruit, by W. A. KELLERMAN. Ibid. p. 12.
[Review.] Respiratory organs of plants. LUDWIG JOST. Am. Jour. Sci. ser. 3, vol. 35, p. 258.
[Review.] Die natürlichen Pflanzenfamilien, von A. ENGLER und K. PRANTL. Ibid. p. 259.
[Review.] Flora of the Hawaiian Islands; by WILLIAM HILLEBRAND. Ibid. pp. 501–502.
[Review.] Dr. SERENO WATSON's Contributions to American Botany. Am. Jour. Sci. ser. 3, vol. 36, p. 392.
[Review.] Flora of Middlesex County, Mass.; by L. L. DAME and F. S. COLLINS. Ibid. pp. 392–393.
[Review.] A catalogue of plants growing without cultivation in the county of Nantucket, Mass.; by MARIA L. OWEN. Ibid. pp. 393–394.
[Review.] Catalogue of the flora of Vermont; by GEORGE H. PERKINS. Ibid. p. 394.
[Review.] Plants of Rhode Island; by J. H. BENNETT. Ibid. pp. 394–395.
[Review.] Catalogue of the flowering and fern-like plants growing without cultivation in the vicinity of the Falls of Niagara; by DAVID F. DAY. Ibid. p. 395
The Botanic Garden [including a report on the projected Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1886–87. pp. 124–126. Cambridge, Mass.

1889

Herbarium of the late Rev. Dr. Joseph Blake. Am. Jour. Sci. ser. 3, vol. 37, p. 419.
Some recent investigations relative to cell-contents. Address before Am. Assoc. Adv. Sci. at Toronto meeting, Aug. 1889. Salem Press Publ. & Print. Co., p. 16. Salem. [Also in] Proc. Am. Assoc. Adv. Sci. vol. 38, pp. 259–272.
What is a phyllodium? Am. Jour. Sci. ser. 3, vol. 38, pp. 495–497.
Protoplasm and its history. Address delivered . . . as vice president of the Biological Section of the A. A. A. S. at Toronto, Aug. 28, 1889. Bot. Gaz. vol. 14, pp. 235–246.
Principles of physiological botany as applied to horticulture and forestry. Garden and Forest, vol. 2, pp. 8–9, 20–21, 32–33, 44–45, 56, 68–69, 80–81, 92, 104–105, 116–117, 128–129, 140–141, 153, 164–165, 177–178, 188, 201–202, 213–214, 225, 249–250.
[Review.] Das Protoplasm als Fermentorganismus . . . ALBERT WIGAND. Am. Jour. Sci. ser. 3, vol. 37, pp. 77–79.
[Review.] "Ringed trees." W. L. GOODWIN. Ibid. p. 79.
[Review.] A provisional host-index of fungi of the United States, by W. G. FARLOW and A. B. SEYMOUR. Ibid. pp. 79–80.
[Review.] Certain relations of the cell wall.—Dr. KOHL. Ibid. p. 237.
[Review.] The chemical nature of assimilation.—TH. BOKORNY. Ibid. pp. 237–238.
[Review.] Improvement in the "races" of the sugar beet.—C. VIOLETTE and F. DESPREZ. Ibid. p. 238.
[Review.] The primordial leaves of Abietineae.—DAGUILLON. Ibid. pp. 238–239.
[Review.] The descending water-current in plants and its physiological significance.—J. WIESNER. Ibid. pp. 319–320.
[Review.] Certain coloring matters in fungi. W. ZOPF. Ibid. p. 320.
[Review.] The bacterial forms found in normal stomachs.—J. E. ABELOUS. Ibid. pp. 320–321.
[Review.] The Botanic Garden at Buitenzorg, Java.—Dr. TREUB. Ibid. p. 322.
[Review.] The structure of the "crown" of the root.—LÉON FLOT. Ibid. p. 322.
[Review.] Contributions to American botany, XVI; by SERENO WATSON. Ibid. pp. 415–416.
[Review.] Key to the system of Victorian plants, I; by Baron FERD. VON MUELLER. Ibid. pp. 416–417.
[Review.] Revision of North American Umbelliferae; by J. M. COULTER and J. N. ROSE. Ibid. p. 417.
[Review.] Flora Italiana, vol. viii . . . Professor CARUEL. Ibid. p. 417.
[Review.] Diagnoses plantarum novarum asiaticarum, VII.—C. J. MAXIMOWICZ. Ibid. p. 417.
[Review.] The orchids of the Cape Peninsula; by HARRY BOLUS. Ibid. p. 417.
[Review.] Handbook of the Amaryllideae, including the Alstroemerieae and Agaveae; by J. G. BAKER. Ibid. p. 418.

[Review.] Synoptical list of the North American species of Ceanothus; by WM. TRELEASE . . . C. C. PARRY. Ibid. p. 418.
[Review.] On the multiplication of Bryophyllum calycinum.—Dr. B. W. BARTON. Ibid. p. 419. [Unsigned.]
[Review.] Enumeratio plantarum guatemalensium . . . JOHN DONNELL SMITH. Ibid. p. 419.
[Review.] Journal of André Michaux 1787–1796 . . . C. S. SARGENT. Ibid. p. 419.
[Note.] Annals of Botany. Ibid. p. 419.
[Review.] Outlines of lessons in botany. Part I. From seed to leaf; by JANE H. NEWELL. Ibid. p. 419.
[Review.] Ueber Entstehung uud Wachsthum der Zellhaut; E. ZACHARIAS. Am. Jour. Sci. ser. 3, vol. 38, p. 252.
[Review.] Volvox . . . L. KLEIN. Ibid. p. 252.
[Review.] Zur Kenntniss der fixeu Lichtlage der Laubblätter; by G. KRABBE. Ibid. p. 253.
[Review.] Flora, oder Allgemeine botanische Zeitung. [Note on journal and articles by several authors.] Ibid. p. 253.
[Review.] The utilization by plants of free atmospheric nitrogen . . . Prof. FRANK. Ibid. p. 253.
[Review.] Monographiae Phaneroga[ma]rum Prodromi nunc continuatio nunc revisio Editoribus et pro parte auctoribus ALPHONSO et CASIMIR DE CANDOLLE. Vol. Sextum. Andropogoneae, auctore EDUARDO HACKEL Ibid. pp. 253–254.
[Review.] Angewandte Pflanzenanatomie; by A. TSCHIRCH. Ibid. p. 254.
[Review.] Die natürlichen Pflanzenfamilien.—A. ENGLER and K. PRANTL. Ibid. p. 415.
[Review.] Guide practique pour les travaux de micrographie, par BEAUREGARD et GALLIPPE. Ibid. p. 415.
[Review.] Contributions to the physiology of growth.—J. WORTMANN. Ibid. pp. 415–416.
The Botanic Garden [including a report on the building of the Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1887–88. pp. 133–137. Cambridge, Mass.

1890

A suggestion relative to science teaching in the secondary schools, by Professor G. L. Goodale, President of the Am. Soc. Nat. from records of meeting at Columbia College, New York, Dec. 1889. Cover, pp. 1–8. Boston.
On the effects produced on some tropical plants by a temperature of 40 to 34 degrees. Am. Jour. Sci. ser. 3, vol. 39, pp. 77–78.
On an apparatus for easily controlling temperatures at or below freezing, for experiments on the relations of plants to cold. Ibid. pp. 78–79.
The disintegration of woody tissues. Ibid. p. 79.
[Review.] Die natürlichen Pflanzenfamilien, von A. ENGLER und K. PRANTL. Ibid. pp. 75–77.
[Review.] Illustrations of West American Oaks . . . ALBERT KELLOGG. Ibid. pp. 79–80.
[Review.] Hybrids.—G. DE SAPORTA. Ibid. p. 161.
[Review.] The cause of the ascent of sap.—JOSEPH BOEHM. Ibid. p. 162.
[Review.] Memoirs of the Torrey Botanical Club . . . Studies . . . of . . . Carex . . . L. H. BAILEY. Ibid. p. 162.
[Review.] On the nitrification of ammonia.—TH. SCHLOESSING. Ibid. p. 162.
[Review.] On the part which ammonia plays in the nutrition of the higher plants . . . A. MÜNTZ. Ibid. pp. 162–163.
[Review.] Fixation of nitrogen by leguminous plants. E. BRÉAL. Ibid. p. 163.
[Review.] On the roots of the saprophytes.—F. JOHOW. Ibid. p. 243.
[Review.] Hybrids in the genus Ranunculus . . . FREYN. Ibid. p. 325–326.
[Review.] Histology as a basis for classification . . . VESQUE. Ibid. p. 407.
[Review.] On the stem structure of Iodes tomentella; by B. L. ROBINSON. Ibid. p. 407.
[Review.] Garnsey's translation of Sachs's history of botany. Ibid. pp. 407–409.
[Review.] Die natürlichen Pflanzenfamilien, nos. 39 and 40 . . . ENGLER and PRANTL. Am. Jour. Sci. ser. 3, vol. 40, p. 93.
[Review.] Zoe, a biological journal . . . HARKNESS . . . BRANDEGEE . . . PARISH . . . VASLIT. Ibid. pp. 93–94.
[Review.] Catalogue of plants found in New Jersey; by N. L. BRITTON. Ibid. pp. 171–172.
List[s] of plants. Ibid. p. 172.
[Review.] Preparation of sections for the study of the development of organs.—GOETHART. Ibid. p. 172.
[Review.] On the ascent of colored liquids in living plants . . . WIELER. Ibid. p. 173.
[Review.] Analytical key to the genera and species of North American mosses.—C. R. BARNES. Ibid. p. 173.
[Review.] Structural and systematic botany.—D. H. CAMPBELL.
The Botanic Garden [including report on the Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1888–89. pp. 153–156. Cambridge, Mass.

1891

Some of the possibilities of economic botany. Presidential address before the Am. Assoc. Adv. Sci. at Washington, Aug., 1891. Am. Jour. Sci. ser. 3, vol. 42, pp. 271–303.

The same reprinted separately at New Haven, 8° cover, pp. [2] 271–303.

The same published in Proc. 40th meeting Am. Assoc. Adv. Sci. 1892. pp. 1–38 [under title of] Useful plants of the future.—Some possibilities of economic botany.

The same, Pop. Sci. Monthly, 1892, vol. 40, pp. 57–75, 207–220. Nature (London), vol. 44, pp. 469–470; 530–536.

Translations: Zukunftsfragen ueber Nahrungs-und Nützpflanzen. Sonderabdruck aus der Pharmaceutischen Rundschau. New York. 1891. 8° pp. 32. Les plantes utiles de l'avenir. Revue Scientifique, 5 Dec., 1891, 2 Jan., 1892.

Botanic Gardens in the equatorial belt and in the south seas. Am. Jour. Sci. ser. 3, vol. 42, pp. 173–177, 260–264.

[Same continued under title] Some museums and botanical gardens in the equatorial belt and in the south seas. Ibid. pp. 347–352, 434–438.

[Same continued under title] Botanic Gardens in the equatorial belt and in the south seas. Ibid. pp. 517–522.

Garden plants of Ceylon. Garden and Forest, vol. 4, p. 62. [See table of contents, p. 61.]

The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1889–1890, pp. 166–170. Cambridge, Mass.

1892

[Museums of Australia, Tasmania, and New Zealand.] Proc. Boston Soc. Nat. Hist. vol. 25, p. 316.

Remarks on the life of Samuel Dexter. Ibid. pp. 364–365.

[Review.] On specific assimilation in Umbelliferae.—M. GÉNEAU DE LAMARLIÈRE. Am. Jour. Sci. ser. 3, vol. 43, p. 160.

[Review.] Effects of certain poisons on the germination of seeds.—CH. CORNEVIN. Ibid. p. 161.

[Review.] Proofs of land-communication between Europe and America during the "modern" age of the earth.—M. ÉMILE BLANCHARD. Ibid. p. 161.

[Review.] Comparative anatomy of plants.—AD. CHATIN. Ibid. pp. 161–162.

[Review.] M. LUCIEN DANIEL . . . Root-grafting. Ibid. p. 162.

[Review.] Physiology . . . LECHARTIER . . . HENRI BECQUEREL. Ibid. p. 162.

[Review.] BOURQUELOT . . . Boletus. Ibid. p. 162.

[Review.] Die natürlichen Pflanzenfamilien . . . ENGLER and PRANTL. Ibid. p. 162.

[Review.] Concerning principles which accompany chlorophyll in leaves.—ÉTARD. Ibid. p. 436.

[Review.] On the structure of the ovule and the development of the embryo-sac in Vincetoxicum. CHAUVEAUD. Ibid. pp. 436–437.

[Review.] Researches regarding the effect of leaf-removal in the case of grape-vines. A. MÜNTZ. Ibid. p. 437.

[Review.] Muciferous system of Laminariaceae, by GUIGNARD. Am. Jour. Sci. ser. 3, vol. 44, p. 501.

[Review.] Researches on multiple buds, by W. RUSSELL. Ibid. pp. 501–502.

[Review.] Artificial intracellular crystallization; E. BELZUNG. Ibid. p. 502.

[Review.] On the aeration of solid tissues; HENRI DEVAUX. Ibid. pp. 502–504.

The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1890–91. pp. 156–163. Cambridge, Mass.

1893

Botany at the university. Harvard Grad. Mag., vol. 1, pp. 291–293.

The Blaschka glass flower collection. Ibid. pp. 602–606.

Botanic Garden and Botanical Museum. Harvard Grad. Mag., vol. 2, pp. 116–117.

Sereno Watson with a list of his writings by J. A. Allen. Proc. Am. Acad., vol. 27, pp. 403–416, where published anonymously.

Same reprinted with portrait of Watson and with mention of author's name. Cambridge, Mass.

The genus Aster. Jour. Royal Hort. Soc., London, vol. 15, pp. 4–12.

[Review.] The localization of the perfumes of flowers.—MESNARD. Am. Jour. Sci., ser. 3, vol. 45, p. 355.

[Note.] Botanical prizes of the French Academy. Ibid. pp. 355–356.

[Review.] How blanched seedlings can be saved.—CORNU. Ibid. p. 356.

[Review.] Influence of moisture on vegetation.—E. GAIN. Ibid. pp. 356–357.

[Review.] Reports of the Missouri Botanical Garden, Annual Report. Many contributions in volume mentioned and discussed. Ibid. pp. 525–526.

[Obituary.] Henry Elias Seaton. Ibid. pp. 526–527.

[Review.] Plants of Orizaba . . . SEATON. Am. Jour. Sci., ser. 3, vol. 46, p. 76.

[Review.] Die natürlichen Pflanzenfamilien . . . ENGLER. Ibid. pp. 76–77.

[Review.] Rainfall and leaf-form . . . STAHL. Ibid. p. 77.

[Review.] Turgescence and transpiration in fleshy plants, by ÉPHREM AUBERT. Ibid. p. 77.

[Review.] On the origin of endogens from exogens.—Rev. GEORGE HENSLOW. Ibid. pp. 77–78.
[Review.] Amount of water in the soil after a period of severe drought . . . REISET. Ibid. pp. 157–158.
[Review.] Nitrification . . . DEHÉRAIN. Ibid. p. 158.
[Review.] The action of the pyocyanic bacillus on plants.—M. CHARRIN. Ibid. pp. 158–159.
[Obituary.] Alphonse DeCandolle. Ibid. pp. 236–239.
The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1891–92, pp. 163–169. Cambridge, Mass.

1894

The Botanic Garden and Botanical Museum. Harvard Grad. Mag., vol. 3, pp. 234–235.
The Botanic Garden and Museum. Harvard Grad. Mag., vol. 4, pp. 395–396.
Address at the dedication of the Mary Frances Searles Science Building, Bowdoin College, Sept. 20, 1894. 8°, pp. 32. Brunswick, Maine.
[Review.] Flora of Mount Desert . . . RAND and REDFIELD. Am. Jour. Sci., ser. 3, vol. 48, p. 431.
[Review.] Researches in regard to the respiration of the Muscineae.—B. JÖNSSON. Ibid. p. 431.
[Review.] On the constitution of the atmosphere.—T. L. PHIPSON. Ibid. pp. 431–432.
[Review.] Vegetable resources of India . . . Kew Bulletin. Ibid. pp. 511–512.
[Review.] Grafting.—M. LUCIEN DANIEL. Ibid. pp. 512–513.
[Review.] Inflorescence in descriptive botany . . . M. F. HY. Ibid. p. 513.
[Review.] Researches on the structure and affinities of the Terebinthaceae . . . JADIN. Ibid. p. 513.
The Botanic Garden [including a report of the Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1892–93, pp. 183–187. Cambridge, Mass.

1895

The Botanic Garden and Museum, Harvard Grad. Mag. vol. 4, pp. 247–248.
The New York Botanic Garden. Science, n. s., vol. 2, pp. 1–2.
[Review.] Lehrbuch der Botanik.—Dr. FRANK. Am. Jour. Sci. ser. 3, vol. 49, p. 75.
[Review.] Lehrbuch der Botanik.—Dr. K. GIESENHAGEN. Ibid. p. 75.
[Review.] Lehrbuch der Botanik.—Dr. F. PAX. Ibid. p. 75.
[Review.] Lehrbuch der Botanik.—STRASBURGER, NOLL, SCHENK, SCHIMPER. Ibid. pp. 75–76.
[Review.] A student's textbook of botany; by SYDNEY H. VINES. Ibid. pp. 76–77.
[Review.] Practical physiology of plants; by FRANCIS DARWIN and E. HAMILTON ACTON. Ibid. p. 77.
[Review.] A practical flora for schools and colleges; by O. R. WILLIS. Ibid. pp. 77–78.
[Review.] Pflanzen-Teratologie, systematisch geordnet. Dr. O. PENZIG. Ibid. p. 78.
[Review.] Practical botany for beginners; by F. D. BOWER. Ibid. p. 78.
[Review.] Mechanism of the movements of the stamens of Berberis.—CHAUVEAUD. Ibid. pp. 165–166.
The Harvard Botanical Museum. Ibid. p. 166.
[Review.] On the amount of absorption of water by roots.—M. LE COMTE. Ibid. p. 167.
The Ware collection of Blaschka glass models of plants and flowers in the Botanical Museum of Harvard University. Ibid. pp. 242–245.
[Review.] Field, forest, and garden botany. By ASA GRAY, revised by L. H. BAILEY. Ibid. p. 325.
[Review.] A popular treatise on the physiology of plants . . . PAUL SORAUER. Translated by F. E. WEISS. Ibid. pp. 325–326.
[Review.] Cellulose. An outline of the chemistry of the structural elements of plants, with reference to their natural history and industrial uses; by C. F. CROSS, E. J. BEVAN, and C. BEADLE. Ibid. pp. 482–483.
[Review.] An interesting method of dissemination.—DUSÉN. Ibid. p. 483.
[Review.] Australian narcotics . . . J. H. MAIDEN. Ibid. p. 483.
[Review.] Die natürlichen Pflanzenfamilien. ENGLER. Am. Jour. Sci. ser. 3, vol. 50, p. 78.
[Review.] Familiar flowers of field and garden, by F. SCHUYLER MATHEWS. Ibid. p. 78.
[Review.] Synoptical Flora of North America; by ASA GRAY and SERENO WATSON, continued and edited by B. L. ROBINSON. Ibid. pp. 428–429.
[Review.] Another example of chalazogamy.—Dr. S. NAWASCHIN. Ibid. p. 429.
[Review.] The Missouri Botanical Garden. Sixth Annual Report. Ibid. pp. 507–508.
[Review.] Index Kewensis. Ibid. p. 508.
The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1893–94, pp. 183–187. Cambridge, Mass.

1896

The Botanical Garden and Museum. Harvard Grad. Mag., vol. 5, pp. 236–237.
Agricultural problems in plant physiology. Jour. Royal Agric. Soc., ser. 3, vol. 7, pp. 351–369. London.
Pleiotaxy in the androecium of Epidendrum cochleatum, L. Am. Jour. Sci. ser. 4, vol. 1, p. 72.
[Review.] Plant breeding. Being five lectures on the amelioration of domestic plants; by L. H. BAILEY. Am. Jour. Sci. ser. 4, vol. 1, p. 151.

[Review.] Tubercles on the roots of the soja bean . . . Kirchner. Ibid. pp. 151–152.

[Review.] Modifications of the cell-wall . . . Van Wisselingh . . . Mangin. Ibid. p. 152.

[Review.] On the accumulation of sugar in the root of the beet.—L. Maquenne. Ibid. 152–153.

[Review.] Propagation of the sugar-cane . . . J. H. Wakker. Ibid. p. 324.

[Review.] Lehrbuch der ökologischen Pflanzen-geographie . . . Eugen Warming. Am. Jour. Sci. ser. 4, vol. 2, p. 89.

[Review.] Missouri Botanical Garden. Seventh Annual Report. Ibid. p. 89.

[Review.] The Botanical Gazette. Ibid. p. 396.

[Review.] The dualistic theory of descent . . . Sachs. Ibid. pp. 396–397.

[Review.] The timber pines of the southern United States; by Charles Mohr. Ibid. pp. 463–464.

[Review.] Richards on increase of activity in respiration after injury . . . Pfeffer. Ibid. p. 464.

[Obituary.] Baron Ferdinand von Mueller, K. C. M. G., Government Botanist of Victoria. Am. Jour. Sci. ser. 4, vol. 2, p. 464.

The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1894–95, pp. 194–199. Cambridge, Mass.

1897

Botanical Garden and Museum. Harvard Grad. Mag. vol. 6, pp. 237–238.

[Letter.] From Prof. George L. Goodale. [Presented at a memorial meeting for Mr. Thomas Tracy Bouvé.] Proc. Boston Soc. Nat. Hist. vol. 27, p. 241.

[Review.] An illustrated flora of the northern United States, Canada, and the British possessions from Newfoundland to the parallel of the southern boundary of Virginia, and from the Atlantic ocean westward to the 102d meridian; by Nathaniel Lord Britton . . . Addison Brown. Am. Jour. Sci. ser. 4, vol. 3, pp. 76–77.

[Review.] Notes on the flora of Newfoundland; by B. L. Robinson. Ibid. p. 77.

[Review.] The survival of the unlike. A collection of evolution essays suggested by the study of domestic plants; by L. H. Bailey. Ibid. p. 77.

[Review.] A manual and dictionary of the flowering plants and ferns; by J. C. Willis. Ibid. p. 353.

[Review.] A practical botany for beginners. [Notice of this and five other works by the following authors.] F. O. Bower, Volney M. Spalding, Francis Darwin, J. Y. Bergen, William A. Setchell, Thomas H. MacBride. Ibid. pp. 490–491.

[Review.] Professor Van Tieghem's new system of classification of Phænogamia. Am. Jour. Sci. ser. 4, vol. 4, pp. 79–82.

[Review.] The new series of Contributions from the Gray Herbarium of Harvard University, No. XI, by Mr. Greenman. Ibid. p. 249.

[Review.] Synoptical Flora of North America, Vol. 1, Part I, Fascicle II. . . . Asa Gray . . . Benjamin Lincoln Robinson . . . William Trelease . . . John M. Coulter . . . L. H. Bailey. Ibid. p. 249.

[Review.] An illustrated flora of the northern United States, Canada, and the British possessions . . . Nathaniel Lord Britton . . . Addison Brown. Ibid. p. 250.

[Review] Experimental morphology; by C. B. Davenport. Ibid. p. 397.

[Review.] Descriptive catalogue of useful fiber plants of the world, including the structural and economic classifications of fibres; by Charles Richards Dodge. Ibid. pp. 478–479.

The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1895–96. pp. 206–208. Cambridge, Mass.

1898

Tropical horticulture, with illustrations of the principal economic plants of hot climates. Trans. Mass. Hort. Soc. for 1897, pp. 13–19.

[Review.] Synoptical view of the various divisions of Embryophyta siphonogama; by A. Engler. Am. Jour. Sci. ser. 4, vol. 5, p. 156.

[Review.] Pflanzenphysiologie. Ein Handbuch der Lehre vom Stoffwechsel und Kraftwechsel in der Pflanze; von Dr. W. Pfeffer. Ibid. pp. 317–318.

[Review.] Permeability of the bark of tree-trunks to atmospheric gases. M. Henri Devaux. Ibid. p. 318

[Review.] Walter Gardiner . . . Structure of the cell wall. Ibid. pp. 470–471.

[Review.] The phytogeography of Nebraska: I. General survey; by R. Pound. Ibid. p. 471.

[Review.] Text-book of botany; by Drs. Strasburger, Schimper, Noll and Schenk. Ibid. pp. 471–472.

[Review.] Fossil plants: for students of botany and geology; by A. C. Seward. Ibid. p. 472.

[Review.] Contributions to Japanese Characeae; by T. F. Allen. Ibid. p. 472.

The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and Treasurer of Harvard College, 1896–97, pp. 211–214. Cambridge, Mass.

1899

British and colonial army surgeons on the 19th of April 1775. An address delivered on the 19th of April, 1899, before the Middlesex South District Medical Soc. Published at the request of the Society, Cambridge, Mass.

Botanic Garden and Botanical Museum. Harv. Grad. Mag. vol. 7, pp. 419–420.

[Review.] The poisonous effect exerted on living plants by phenols. . . . R. H. TRUE. Am. Jour. Sci. ser. 4, vol. 7, pp. 76–77.

[Review.] On the peculiar mode of formation of pollen-grains in Magnolias. . . . GUIGNARD. Ibid. p. 77.

[Review.] Elements de botanique. P. VAN TIEGHEM. Ibid. pp. 77–78.

[Review.] Die natürlichen Pflanzenfamilien . . . ENGLER. Ibid. p. 169.

[Review.] Experimental morphology, Part II; by C. B. DAVENPORT. Ibid. p. 474.

[Review.] Variations under grafting and the heredity of acquired characters; by L. DANIEL. Am. Jour. Sci. ser. 4, vol. 8, pp. 84–85.

[Review.] Researches concerning certain features in the appropriation of nitrogen by plants; by L. LUTZ. Ibid. pp. 85–86.

The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and the Treasurer of Harvard College, 1897–98., pp. 231–235. Cambridge, Mass.

1900

New England plants seen by the earliest colonists. [Read 15 Jan. 1896.] Trans. Colonial Soc. Mass. vol. 3, pp. 180–194, Boston, Mass.

[His remarks on the death of A. Gould. Read Dec. 1896.] Ibid. pp. 304–307.

[Remarks on the death of General Walker. Read at meeting of January, 1897.] Ibid. p. 315.

[Review.] Praxis und Theorie der Zellen- und Befruchtungslehre. . . . V. HACKER. Am. Jour. Sci. ser. 4, vol. 9, pp. 77–78.

[Review.] The Missouri Botanical Garden. Ibid. p. 233.

The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and the Treasurer of Harvard College, 1898–99, pp. 238–243. Cambridge, Mass.

1901

The Botanical Department. Harvard Grad. Mag. vol. 10, pp. 254–255.

1902

The memorial greenhouses at the Harvard Botanic Garden. Am. Jour. Sci. ser. 4, vol. 13, p. 162.

[Brief note on] The Botanisches Centralblatt. Ibid. p. 327.

[Review.] The Galapagos Islands. Important paper, by Dr. Robinson, dealing with their peculiar vegetation. [Unsigned.] New York Evening Post, Dec. 6, 1902.

[Review.] Researches on cellulose, 1895–1900; by C. F. CROSS and E. J. BEVAN. Am. Jour. Sci. ser. 4, vol. 13, pp. 161–162.

[Review.] Professor van Tieghem's classification of plants. Ibid. pp. 326–327.

The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and the Treasurer of Harvard College, 1900–01, pp. 225–230. Cambridge, Mass.

1903

An experimental garden in Cuba. Am. Jour. Sci. ser. 4, vol. 16, pp. 105–106.

The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and the Treasurer of Harvard College, 1901–02, pp. 242–246. Cambridge, Mass.

1904

Harvard Experiment Station in Cuba. Am. Jour. Sci. ser. 4, vol. 18, pp. 91–95.

The Botanic Garden [including a report on the Botanical Museum]. Ann. Rep. of the President and the Treasurer of Harvard College, 1902–03, pp. 226–229. Cambridge, Mass.

1905

[The same for the following year]. Ibid. 1903–04, pp. 241–244.

1906

[Review.] The microscopy of vegetable foods, with special reference to the detection of adulteration and the diagnosis of mixtures; by ANDREW L. WINTON. Am. Jour. Sci. ser. 4, vol. 21, pp. 335–336.

A new form of "container" for use in museums of economic botany. Ibid. pp. 451–452.

Notes from the Harvard Botanical Station in Cuba. Ibid. pp. 475–476.

[Review.] Plant response as a means of physiological investigation; by JAGADIS CHUNDER BOSE. Ibid. pp. 476–477, and vol. 22, pp. 188–190.

Plaster-plaques for museums. Am. Jour. Sci. ser. 4, vol. 22, pp. 90–92.

Heather in Townsend, Mass. Ibid. p. 190.

The Botanic Garden [including a report on the Botanic Museum]. Rep. of the President and the Treasurer of Harvard College, 1904–05, pp. 241–245.

1907

The Botanic Garden [including a report on the Botanical Museum]. Rep. of the President and the Treasurer of Harvard College, 1905–06, pp. 230–234. Cambridge, Mass.

1908

[Review.] The microscopy of technical products; by DR. T. F. HANAUSEK. Am. Jour. Sci. ser. 4, vol. 25, pp. 87–88.

[Review.] Gray's New Manual of Botany . . . BENJAMIN LINCOLN ROBINSON and MERRITT LYNDON FERNALD. The Nation, vol. 87, p. 366. [Unsigned.]

[Review.] Gray's New manual of botany. A handbook of the flowering plants and ferns of the central and northeastern United States and Canada . . . Revised by BENJAMIN LINCOLN ROBINSON and MERRITT LYNDON FERNALD. Am. Jour. Sci. ser. 4, vol. 26, pp. 518–519.

[Review.] Systematic anatomy of the Dicotyledons. A handbook for laboratories of pure and applied botany. HANS SOLEREDER. Ibid. pp. 585–586.

[Review.] A text-book of botany and pharmacognosy; by HENRY KRAEMER. Ibid. pp. 586–587.

[Review.] Die Gestalts- und Lageveränderung der Pflanzen-Chromatophoren; mit einer Beilage; Die Lightbrechung der lebenden Pflanzenzelle; von GUSTAV SENN. Ibid. p. 587.

The Botanic Garden [including a report on the Botanical Museum]. Rep. of the President and the Treasurer of Harvard College, 1906–07, pp. 227–230. Cambridge, Mass.

1909

The Harvard Botanical Station in Cuba. Am. Jour. Sci. ser. 4, vol. 27, pp. 94–96.

A convenient clearing and mounting reagent. Ibid. p. 96.

[Review.] Handbuch der Blüten-biologie . . . PAUL KNUTH. Translated by J. R. AINSWORTH DAVIS. Ibid. p. 96.

[Review.] The forest flora of New South Wales; by J. H. MAIDEN. Ibid. pp. 191–192.

[Review.] Jaarboek van het Department van Landbouw in Niederlandsch-Indie, 1907. Ibid. p. 192.

[Review.] Notes of a botanist on the Amazon and Andes . . . RICHARD SPRUCE . . . ALFRED RUSSEL WALLACE. Ibid. pp. 266–267.

[Review.] The Commercial products of India; by SIR GEORGE WATT. Ibid. pp. 417–418.

[Review.] The forest flora of New South Wales; by J. H. MAIDEN. Ibid. p. 418.

[Review.] Trees, a handbook of forest botany for the woodlands and the laboratory. H. MARSHALL WARD. Ibid. p. 491.

[Review.] Mendel's Principles of heredity; by W. BATESON. Ibid. pp. 491–492, and vol. 28, pp. 84–85.

[Review.] Contributions from the Gray Herbarium of Harvard University. . . . [On work of several authors.] Am. Jour. Sci. ser. 4, vol. 28, p. 85.

[Review.] Elemente der exakten Erblichkeitslehre . . . . W. JOHANNSEN. Ibid. pp. 85–86.

The Botanic Garden [including a report on the Botanical Museum.] Rep. of the President and the Treasurer of Harvard College, 1907–08, pp. 237–241. Cambridge, Mass.

1910

[The same for the following year.] Ibid. 1908–09, pp. 227–231.

[Review.] Physiologische Pflanzen-anatomie. . . . G. HABERLANDT. Am. Jour. Sci. ser. 4, vol. 29, pp. 195–196.

1911

The Botanical Museum. Rep. of the President and the Treasurer of Harvard College, 1909–10, pp. 193–194. Cambridge, Mass.

1912

[The same for the following year.] Ibid. 1910–1911, pp. 185–186.

Biographical memoire of Alexander Agassiz, 1835–1910. Biogr. Mem. Nat. Acad. Sci. vol. 7, pp. 289–305, with portrait.

1913

The Botanical Museum. Rep. of the President and the Treasurer of Harvard College, 1911–12, pp. 181–183.

1914

[The same for the academic year 1912–13.] Ibid. 1912–13, pp. 187–188.

1915

[The same for the academic year 1913–14.] 1913–14, pp. 198–200.

The Ware Collection of Blaschka Glass-models of plants in flower. Cover [2], pp. 1–10. Cambridge, Mass. [Unsigned.]

1916

The Botanical Museum. Rep. of the President and the Treasurer of Harvard College, 1914–15, pp. 213–216. Cambridge, Mass.

1917

[The same for the academic year 1915–16.] Ibid. 1915–16, pp. 217–219.

1918

[The same for the academic year 1916–17.] Ibid. 1916–17, pp. 205–206.

The development of botany as shown in this Journal [Development of botany since 1818.] Am. Jour. Sci. ser. 4, vol. 46, pp. 399–416.

1919

The Botanical Museum. Rep. of the President and the Treasurer of Harvard College, 1917–18, pp. 205–208.

1920

[The same for the academic year 1918–19.] Ibid. 1918–19, pp. 162–163.

The Ware collection of Blaschka glass-models of plants in flower. Published by the Botanical Museum [of Harvard Univ.] for gratuitous distribution. Cover., pp. [2] 1–11. [A later and slightly altered edition of the pamphlet issued in 1915.]

1921

The Botanical Museum. Rep. of the President and the Treasurer of Harvard College, 1919–20, pp. 205–207. Cambridge, Mass.

1922

[The same for the academic year 1920–21.] Ibid. 1920–21, pp. 242–243.

1923

[The same for the academic year 1921–22.] Ibid. 1921–22, pp. 188–190.

BIOGRAPHICAL AND OBITUARY NOTICES CONCERNING DR. GOODALE

1891

Sketch of George Lincoln Goodale. Popular Sci. Monthly, Sept. 1891, vol. 39, portrait and pp. 691–694. [Unsigned.]

1923

Prof. George L. Goodale dead. [Obituary.] Boston Evening Transcript, Apr. 12, 1923.

[Obituary notice.] Cambridge Chronicle, Apr. 14, 1923.

Professor George Lincoln Goodale. [Obituary notice.] Harvard Univ. Gazette, vol. 18, p. 146, Apr. 14, 1923.

M. D. '63. George Lincoln Goodale. [Unsigned obituary.] Harvard Alumni Bull. vol. 25, p. 277, Apr. 26, 1923.

George Lincoln Goodale. [Note of his death.] Am. Jour. Sci. ser. 5, vol. 5, p. 524, June, 1923.

George Lincoln Goodale. [Obituary by William Trelease. Science, vol. 57, pp. 654–656, June 8, 1923.

Minute on the life and services of Professor George Lincoln Goodale. W. J. V. Osterhout, B. L. Robinson and M. L. Fernald. Harvard Univ. Gazette, vol. 18, p. 198, June 16, 1923.

George Lincoln Goodale. [Obituary.] Reprinted from the preceding. Am. Jour. Sci. ser. 5, vol. 6, pp. 275–276.

George Lincoln Goodale. [Obituary by] L. H. Bailey. Rhodora, vol. 25, portrait and pp. 117–120. Aug. 28, 1923.

George Lincoln Goodale. [Obituary by] Robert Tracy Jackson. Harvard Grad. Mag. vol. 32, portrait and pp. 54–59. Sept. 1923.

Henry Marion Howe

NATIONAL ACADEMY OF SCIENCES

Volume XXI
SEVENTH MEMOIR

BIOGRAPHICAL MEMOIR HENRY MARION HOWE

1848-1922

BY

GEORGE K. BURGESS

Presented to the Academy at the Autumn Meeting, 1923

HENRY MARION HOWE

By George K. Burgess

A very beautiful memorial service was held at the Episcopal Cathedral of St. John the Divine, New York City, at 5 o'clock on the evening of October 25, 1923, in honor of the late Henry Marion Howe, who was a vestryman in an Episcopal church of that diocese. Dr. Michael I. Pupin, professor of electro mechanics, Columbia University, was the speaker on this occasion, his theme being "The power of coordination" as exemplified by Doctor Howe in his life and works. This service coincided with the date of the fall meeting of the American Iron and Steel Institute and there were gathered in that quiet and peaceful corner many of his old friends and associates to do honor to the man whose life was so worthy and fruitful in his endeavor to be of service to mankind.

Henry Marion Howe was born March 2, 1848, in Boston. His death occurred in the seventy-fourth year of his life at his home in Bedford Hills, N. Y., on May 14, 1922, of an illness from which he had been suffering, acutely at times, for some 15 months. He is survived by his wife, Fannie Gay Howe, whom he married in Troy, N. Y., April 9, 1874, and also by two sisters, Mrs. Laura E. Richards, of Gardiner, Me., and Mrs. Maude Elliott, of Newport.

In order to have a proper background for such a life as was that of this eminent scientist and teacher, it is of interest to note that his father, Dr. Samuel Gridley Howe, American philanthropist, and son of Joseph N. Howe, a shipowner and cordage manufacturer, and Patty Gridley, one of the most beautiful women of her day, was educated at Boston and Brown University, taking his degree in medicine in Boston. However, he was no sooner admitted to practice than he abandoned all prospects of following his chosen profession. With Byron as an example, his enthusiasm carried him to Greece, where he joined the army and spent six years in the midst of scenes of warfare. After this, in turn, he established a relief depot near Aegina, and formed another colony of exiles on the Isthmus of Corinth, writing meantime a History of the Greek Revolution, which was published in 1828. After his return to America, in 1831, he began receiving a few blind children at his father's house in Pleasant Street, and thus sowed the seed which grew into the famous Perkins Institution for the Blind. He was the director, heart and soul of the school; he organized a fund for printing for the blind—the first done in America—which gained for him the title, "Cadmus of the Blind."

His mother, Julia Ward Howe, American author and reformer, daughter of Samuel Ward, a New York banker, and Julia Rush (Cutler), a poet of some ability, when 16 years of age began to contribute poems to New York periodicals. Among her many works of art, undoubtedly the most popular is her poem, "The Battle Hymn of the Republic," written to the old folksong associated with the song of "John Brown's Body," when Mrs. Howe was at the front in 1861, published in the Atlantic Monthly. Her children were Julia Romana Anagnos, 1844–1886, who, like her mother, wrote verse and studied philosophy, and who taught in the Perkins Institution, in the charge of which her husband, Michael Anagnos, succeeded her father; Florence Marion Howe Hall; Henry Marion Howe, the well-known metallurgist, and subject of this biography; Laura Elizabeth Richards; and Maude Howe Elliott, wife of John Elliott, the painter of a fine ceiling in the Boston library, both these daughters being contributors to literature.

Henry Marion Howe, the only son of these two illustrious personages, was in his early childhood trained by tutors in the persons of Polish and Greek refugees to whom his father's beneficence was well known to many. With these for teachers, and in the atmosphere of a home such as was his, young Howe was surrounded throughout his childhood by the very

essence of culture and learning which laid the foundation upon which was built the man so fittingly described by Albert Sauveur when conferring upon him the John Fritz medal in 1917:

Lover of justice and humanity;
Public servant and public benefactor;
Master of the English language;
Loyal and devoted friend;
Untiring and unselfish worker in an important field of science;
Stimulating teacher, inspiring investigator and generous collaborator;
Voyager in realms but dimly perceived by fellow workers;
Lone explorer of fields destined to yield rich harvests to future generations;
Man of genius, honored and loved the world over.

As a boy, young Howe attended the famous Boston Latin School, from which he graduated in 1865, then entering Harvard, where he remained until graduation in 1869, at the age of 21. During these years of his early education he seems to have followed his natural inclinations toward sport and gayety. He was debonair, devoted to music and dancing, full of fun and mischief; "he was never known to drink, or smoke, or consort with light company—his fun was of a different order." One characteristic, which followed him all through his life, was his love of playing practical jokes upon his friends and all who came in contact with him. After being rusticated in his sophomore year at Harvard because of a boyish prank "the faculty thought not appropriate," however, he began to realize he was wasting the great opportunity of education, and from then on his time was not lost. In the fall of 1869 he entered the Massachusetts Institute of Technology and there first revealed his capacity for hard work in the field of science in which he was to become so distinguished in his later years. In 1871 he graduated with the degree of "graduate in the department of geology and mining engineering," for which the institute substituted a few years later the title of "bachelor of science."

With this liberal education as a basis, it was necessary for him to lay yet another foundation—that of acquaintance with practice—and so we find him next in Troy, N. Y., a student in the steel works. Here he worked hard, but it was not all drudgery, for his high spirits and sense of humor soon made him a favorite in a place full of gayety such that his presence was always in demand.

When his practical studies in Troy were over, he began the work of his career as superintendent of the Bessemer Steel Works, Joliet, in 1872, and of the Blair Iron & Steel Co., 1873–74. For the next five years he devoted himself to the metallurgy of copper, and improved copper smelting in Chile for the heirs of Augustus Hemenway, and then designed and built the works of the Orford Copper & Nickel Co., at Capeltown and Eustis, in the Province of Quebec and at Bergen Point, N. J., 1879–1882. This latter year he was manager of the Pima Copper-Mining & Smelting Co. of Arizona. As a result of his practical studies during these years in the field of improved copper smelting, there appeared from time to time technical papers on the subject, culminating with his first book, Copper Smelting, published in 1885.

It was while with this last-mentioned company that Doctor Howe made a rather startling decision. With prospects for a singular success financially, for becoming a leader in the industrial world, he deliberately chose, at the age of 35, to turn away from the practical to the theoretical side of his profession. He must have realized, from questions arising out of his practical work, that there was a real need in the metallurgical world—that of bringing cosmos out of apparent chaos of nonrelated facts.

So he made the change by establishing himself in Boston as a consulting metallurgist and at the same time a lecturer on metallurgy at the Massachusetts Institute of Technology, where he remained until 1897, when he was called to the chair of metallurgy at Columbia University in New York City. In 1913 he retired from active work at Columbia and became professor emeritus, when he declined as far as practicable all professional business, devoting himself exclusively to research in Green Peace Laboratory which he established at his home in Bedford Hills and maintained until his death, with the aid of one assistant.

The year 1893 also was marked by his election to the presidency of the American Institute of Mining Engineers, an honor and a task given only to men of distinction and accomplishment

in the mining and metallurgical fields. His presidential address, "Our possibilities," showed him to possess keen analytical powers in showing what we might expect in the future, for many of his predictions have been verified. Howe was one of those who believe in yeoman service to the cause he represents, and he was always ready to serve in the arduous work of directing the policies of the technical societies to which he belonged.

He had a gift for organization which was to stand him in good stead when the call came in 1917, of which more later. He had also a farsighted vision as to the possibilities of future usefulness to the community of societies that had not yet found themselves, as illustrated in the case of the American Society for Testing Materials, of which he accepted the presidency, or chairmanship, as it was then, in the early days of its formation in 1900. As stated by one of the founders, Mr. William R. Webster:

> On looking over the American Society for Testing Materials records you will find that we made very little progress during the first few years of our work, and that it was not until after Doctor Howe was elected president of the society that our work was systematized and real work accomplished.

It was during Doctor Howe's stay in Boston after establishing himself as a consulting metallurgist that he began what was later to become the true mission of his life. Already Howe had established his position as an acute observer and reasoner, imbued with the love of investigation and scientific research, expressing himself with a fine clearness, not only in his writings, but also in his lectures to his students and in technical papers presented at scientific meetings.

In 1891 there appeared The Metallurgy of Steel, the great book which constituted the principal foundation of Doctor Howe's fame. Up to this time steel making was only a practice, not a science. It was a series of operations resulting in the production of a material which was not always satisfactory in quality; nor were the defects understood, nor had the use of the microscope been known generally in the making of steel. Metallography was practically a new science. Therefore, since a quarter of a century had elapsed after the appearance of Percy's classical work on iron and steel, and meanwhile there had appeared merely handbooks, including the really admirable books of Bauerman and of Ledebur and the masterly discussion of Bell in connection with the metallurgy of iron, and especially with the blast-furnace process, it seems to have been a fitting time to offer in accessible form, and more fully than these distinguished authors had, the data which made up our then present knowledge of the metallurgy of steel, and, above all, to discuss these data and seek their true teachings. This book, embodying the results of a comprehensive study of the literature on the subject, marks a new era in the history of steel metallurgy. With astonishing clearness Doctor Howe collected and collated all known facts, either in English, French, or German, in such a comprehensive and full, but concise, manner that it no longer was necessary to consult previous literature on the subject for information relating to the metallurgy of steel, although the author, in seeking to lighten the labor of others who might wish to examine the matter in greater detail, or who might wish to verify his statements, has given many references which it would profit most readers but little to examine. In the words of Dr. Rossiter W. Raymond, this "was an amazing accumulation of reported facts tabulated, verified, and explained as far as was then practicable."

In 1902, beside many professional papers of importance, there appeared Dr. Howe's Metallurgical Laboratory Notes, translated into French, a book containing information on various laboratory practices in the field of metallurgy. Immediately following this, in 1903, came the book entitled, "Iron, Steel, and Other Alloys," translated into Russian. This was a contribution to metallurgical literature for the benefit of not alone the various classes of students, but also to meet the needs of practitioners by giving them a systematic account of the condition of the metallography of iron at that time. Other important works which should at least be mentioned briefly here are the article on "Iron and steel" in the new volumes of the tenth edition of the Encyclopædia Britannica, appearing in 1902, and a later article on the same subject for the eleventh edition of the same work. These are comprehensive but brief statements of the history of iron and steel up to the time of the publication of the treatises.

20154°—26——28

No attempt will be made here to give a summary of the monumental series of contributions to science and technology from Howe's pen. We may mention one or two items in which he maintained an active interest and to which he was constantly reverting in his writings; such as the nomenclature of steel and the soundness of steel. Howe's logical mind rebelled at a commercial classification of steel and he sought persistently to have adopted generally a rational classification, beginning with an early paper published in 1876 and ending with his last book in 1916. He was keenly disappointed that the International Association for Testing Materials would not adopt his views. To the question of the "soundness" of steel he returns again and again in his experimental work and writings, and this subject may be said to be the underlying theme of all his work. His numerous papers on metallography as related to iron and steel, of which science he was one of the creators and most able expounders, constitute a masterly series of monographs.

This brief mention of Howe's principal works brings us up to the publication, in 1916, of the results of his best creative genius, The Metallography of Steel and Cast Iron. While it was not possible, or practicable, in his first book, The Metallurgy of Steel, to more than make a start upon the interpretation of the facts contained therein, he was able, through his own experience and research, and the results of that of others, to include in his last book his personal observations and conclusions, the object being not to state already soundly established principles of a new science, but to open before the workers in that science new fields for thought. He believed the true task of a teacher is to excite thought, and this, I believe, he could not have better accomplished than he did in this masterly book.

This volume, consisting of two distinct parts, an introduction to the new science of microscopic metallography as applied to steel and cast iron and an extended study of the very new branch of that science, the mechanism of plastic deformation, was the first of a series of monographs the completion of which was undoubtedly interrupted by Doctor Howe, then at the age of 70, in order to serve during the World War as an active member and later chairman of the engineering division of the National Research Council, devoting his entire time and thought to the problems under way at such a crisis in the history of the country. For this purpose he spent the winter of 1917–18 in Washington, where his counsel in matters metallurgical was much sought after and valued.

Doctor Howe was made a member of the National Research Council February 26, 1918; made member of the division of engineering March 11, 1919, and chairman April 1, 1919. He was appointed scientific attaché in the American Embassy in Paris April 15, 1919, and delegate to Brussels meeting June 17, 1919. Resigning as chairman August 12, 1919, he was appointed honorary chairman of the division of engineering October 14, 1919, to serve for the year 1919 to 1920, and again appointed honorary chairman on April 25, 1920, for the year 1920 to 1921.

This chronology, however, gives no glimpse of the great work he was really doing in orienting and stimulating effective research looking to the improvement in metallurgical practice and products of military importance and in bringing together groups of men most skillfully chosen who could aid in carrying out much-needed investigations. Thus, under his direction, there was formed a general committee of the research council on metallurgical research with several offshoots, in the work of several of which he took an active part, including committees on pyrometry, alloy steels, body armor, steel ingots, improvement of metals in the "blue heat" range, and heat treatment of carbon steels, which last committee he consulted freely in the work of his own laboratory at Bedford Hills. For he also found time to engage in experimental work himself during 1917 and 1918, especially in relation to helmets, special steels for various purposes, the explanation of serious and puzzling imperfections such as "flaky" steel, and the improvement of open-hearth furnace practice on which technique the quality of our steel output depends. Among the objects of his visit to Europe in 1919 was to see what could be done in reviving and maintaining the international relationships among scientific men, and naturally he was concerned with the fate of the International Association for Testing Materials, of which he had been one of the bulwarks in this country; but he found the time not come, nor has it yet, when this important international body could be revived.

The period immediately following the war was a very fruitful one for Doctor Howe and is reflected in the several important contributions from his pen, usually in association with

others. The last three years of his life he was also working on a treatise relating to steel manufacture which he was not permitted to finish.

In 1918, he accepted the position of consulting metallurgist on the staff of the Bureau of Standards, which he held until his death, and he also had a similar relation with the Bureau of Mines. He wanted no remuneration, but merely facilities for productive work at his laboratory. The product of these connections is given in the last writing to bear his name, published after his death under the title, "Influence of temperature, time and rate of cooling on the physical properties of carbon steels."

It was not, however, merely in Howe's formal writings that he was listened to by all interested in the subjects of which he treated, but if one will examine the printed discussions of the many technical meetings he attended one will be surprised at the wealth of suggestive and clarifying material he would offer spontaneously. It was a delight to all present to hear him in a scientific meeting, clear, concise, witty with homely, trite, and often epigrammatic comment, always courteous, always thorough, and unerringly pointing out the weak spots, yet never forgetting to give commendation where merited and occasionally unstinted praise. No man could better summarize another's work than he.

He gave his advice very freely and completely in correspondence with his friends and professional colleagues, and some of the letters I have seen are in themselves models of composition and exposition, and each evidently formed the substance of a portion of some scientific paper in preparation. By thus communicating his ideas in advance of publication he was able to fortify himself and reap the benefits of preliminary friendly discussion. All his published work gives evidence of the greatest care in preparation; he considered all the alternatives possible; and in a subject such as metallography, singularly prone to awake controversy, he was generally able to put forth facts and conclusions that were not seriously questioned.

One of the most charming of his traits was his interest in and the encouragement of the younger men at scientific gatherings. He never failed to say the right word to stimulate further effort, and well I remember the profound impression he made on the occasion of my first paper before the mining engineers by his statement, "We can hardly overestimate the importance of the entry of the Bureau of Standards into our field." Many others have mentioned this trait, which is well described by Mr. A. A. Stevenson, himself a steel maker and a friend of 30 years or more standing:

> Much may be said about what Doctor Howe has done for the steel industry and what he has written, but so far as I am concerned, I feel the greatest legacy Doctor Howe has left is that of his influence on the younger men with whom he came in contact.

Another characteristic was his indomitable persistence and will to work together with an optimistic outlook and cheerfulness as illustrated by the last year and more of his life, when he writes to his sisters:

> As for me I am getting on after a fashion. I am so thankful to be spared most of the pain which usually goes with this ailment and continue to be of real service, and so busy with my readings, and writings, tho they are all done on the bed, that I have no time to be bored, and I think no inclination to grumble, especially when I think of those who voluntarily brought far greater hardship on themselves in the service of their country.
>
> I have just practically finished an important professional paper, "practically finished," because I am now engaged in revising the section and page numbers. I expect a collaborator here the last of next month to take up another important paper with me, and I am making substantial progress on a book.
>
> So it might be worse, incomparably worse, and we may still hope that it will be better.

Doctor Howe had a fine sense of patriotism intensified by his sense of the Nation's as well as the individual's duty, often expressed when occasion offered, and he even made the occasion, as when receiving the John Fritz medal on May 10, 1917, he stated:

> To prolong thanks is so thankless that I might well now hold my peace were it not for the world crisis, ever in our consciousness. This so presses for our best thought that attention to other matters suggests fiddling while Rome burns. For Rome truly is in flames. We are as a family in a burning home. Democracy itself is at stake.
>
> Our problem at the end of the war will be to prevent future wars, to prevent the actual employment of the enormously enhanced destructive powers sure to evolve, to force the nations to keep the peace as we have forced individuals. To say that we are inherently incapable of preventing our own annihilation; that because the less developed past did not learn to prevent its little wars, killing their tens of thousands, the more developed future must ever remain impotent to prevent its wars with their far higher order of havoc, killing their millions, is to betray a fatalism, a pessimism as unworthy of Americans as it is foreign to our nature. To me it seems an insult to the mothers who bore us, nay, to the God who made us.

Among the many honors conferred upon this greatly honored, yet modest man, a member of almost every technical society having to do with iron and steel both here and abroad, he received the degree of A.M. in 1872, and LL.D. in 1905, from Harvard; the degree of LL.D. also from Lafayette in 1905; and that of Sc.D. from the University of Pittsburgh in 1915. He was made a Knight of the Order of St. Stanislaus (Russian) and a Chevalier of the Legion of Honor of France. For his work he received five gold medals from leading technical societies of the world: The Bessemer medal of the Iron and Steel Institute of Great Britain, as mentioned elsewhere in this biographical sketch; the John Fritz medal of the American Institute of Mining Engineers; the Elliott Cresson medal of the Franklin Institute of Philadelphia; that of the Verein zur Befoerderung des Gewerbfleisses of Berlin; and the gold medal of the Société d'Encouragement pour l'Industrie Nationale of France. He was elected president for one term of the American Institute of Mining Engineers, and the International Association for Testing Materials, and for four terms of the American Society for Testing Materials, being among others an honorary member of the Russian Technical Society, Russian Metallurgical Society and the Société d'Encouragement, American Society for Testing Materials, American Society for Steel Treating.

As a teacher, Doctor Howe gave one of his greatest services to humanity. Not only was he the leader, he was also the comrade and friend. The student never felt restraint in asking advice of this kindly gentleman, who in turn was most generous to the deserving, often helping in a material way as well. In the laboratory, as well as in the classroom, he was most patient even in cases where his suggestions had been taken amiss. Owing to his delightful clearness of thought and expression, and simple, logical presentation, Professor Howe's courses were considered the easiest in school. He was endeared in the hearts of his students, and all who came in contact with him, by his wonderful smile and sympathetic manner, and those receiving his recognition considered themselves honored indeed.

The inspiration of Doctor Howe's leadership, it has been the desire to perpetuate, and two societies with which he was intimately associated have sought to commemorate his name. The American Institute of Mining and Metallurgical Engineers have established "the Henry Marion Howe lectureship," and very appropriately Prof. Albert Sauveur, a life long friend and himself with Howe one of the pioneers and outstanding authorities in the subject of metallography, is the first lecturer under this foundation.

There has also been established, in honor of Doctor Howe, the "Henry Marion Howe gold medal," by the American Society for Steel Treating, the first medal being awarded in October, 1923, to Dr. Emanuel J. Janitzky, a replica in bronze being given to Mrs. Howe, in recognition of her aid as a coworker of Doctor Howe, a most devoted friend and companion, accompanying him in all his travels. It is of interest here to note the rules governing the award of this newly established medal as follows:

The board of directors of the American Society for Steel Treating has established a fund to be known as the Henry Marion Howe medal fund, the proceeds of which shall be used annually to award a gold medal to be known as the Henry Marion Howe medal. The award will be made as follows:

(1) The medal will be awarded to the author of the paper which shall be judged to be of the highest merit. All papers in order to be considered must be published originally in the transactions of the society during the 12 months ending August 1 of the year in which the medal is awarded.

(2) The competition for the Henry Marion Howe medal shall be open to all.

(3) The award shall be made by the board of directors.

(4) The award may be withheld at the discretion of the board of directors.

When Howe was elected to the National Academy, in 1917, he had been recognized for years by his colleagues as the dean of American metallurgy. Howe's preeminent position is strikingly set forth at the presentation to him of the John Fritz medal in 1917 in the addresses of Dr. R. W. Raymond, Pres. I. N. Hollis, and Prof. A. Sauveur. We have already quoted the estimate of Sauveur and we are also indebted to much in Doctor Raymond's account of Howe's accomplishments. We may close by quoting from his student, associate, and successor at Columbia, Prof. William Campbell:

In short, we can say of him that he was a kindly gentleman, thoughtful of others; a great scientist, greatly honored and yet most modest; a remarkably clear writer with a gift of simplicity of thought and diction; and lastly he was undoubtedly the greatest of all the steel metallurgists.

THE RECORD OF HENRY MARION HOWE

I. BIOGRAPHIC DATA

Born March 2, 1848, at Boston, Mass.
Graduated from Boston Latin School, 1865.
Graduated as A. B. from Harvard College, 1869.
Graduated (degree corresponding to B. Sc.) from Massachusetts Institute of Technology, 1871.
Received degree of A. M. from Harvard College, 1872.
Student in steel works at Troy, N. Y., 1871–72.
Superintendent of Bessemer works of Joliet Iron & Steel Co., 1872.
Superintendent of Blair Iron & Steel Co.'s works, Pittsburgh, 1873–74.
Married Miss Fannie Gay, of Troy, N. Y., 1874.
Improvements in copper smelting in Chile for heirs of Augustus Hemenway, 1877–78.
Designed and built the works of the Orford Nickel & Copper Co., at Capelton and Eustis, in the Province of Quebec, Canada, and at Bergen Point, N. J., 1879–82.
Manager of Pima Copper Mining & Smelting Co., Arizona, 1882.
Consulting metallurgist, with office in Boston, and lecturer on metallurgy at the Massachusetts Institute of Technology, 1883–97.
Professor of metallurgy in Columbia University, New York, 1897–13, and professor emeritus from 1913.
Chairman, engineering division, National Research Council, 1919.
Consulting metallurgist, Bureau of Standards; Bureau of Mines.
Deceased, May 14, 1922 at Bedford Hills, N. Y.

II. DISTINCTIONS RECEIVED

HONORARY MEMBERSHIPS

Royal Swedish Academy of Sciences.
Russian Imperial Technical Society.
Russian Metallurgical Society.
Cleveland Institution of Engineers, England.
Société d'Encouragement pour l'Industrie Nationale of France.
Dallas Historical Society, Texas.
Alumni Association of the School of Mines of Columbia University.
American Iron and Steel Institute.
Institution of Mining and Metallurgy, England.
American Society for Steel Treating.
National Research Council, honorary chairman engineering division.
American Society for Testing Materials.

FELLOWSHIPS, ETC.

National Academy of Sciences (Member).
New York Academy of Sciences (Fellow).
Washington Academy of Sciences (Non resident Member).
American Academy of Arts and Sciences (Fellow).
American Philosophical Society (Non resident Member).
American Academy of Engineers (Charter Member).

PRESIDENCIES

American Society for Testing Materials (four terms, 1900–02, 1909–12).
American Institute of Mining Engineers, 1893.
International Association for Testing Materials, 1912.
Alumni Association, Massachusetts Institute of Technology (three terms).
Jury of Awards, Mining and Metallurgy, Chicago Exposition of 1893.

HONORARY VICE PRESIDENCY

Iron and Steel Institute of Great Britain.

LIFE MEMBERSHIP OF COUNCIL

International Association for Testing Materials.

ORDERS

Knight of the Order of St. Stanislaus, Russia (second order, with star of first order).
Chevalier of the Legion of Honor of France.

MEDALS

Bessemer medal, Iron and Steel Institute of Great Britain.
Eliot Cresson medal, Franklin Institute of Philadelphia.
Gold medal of the Verein zur Befoerderung des Gewerbfleisses, Berlin.
Gold medal of Société d'Encouragement pour l'Industrie Nationale of France (1916).
John Fritz medal, American Institute of Mining Engineers, 1917.

HONORARY DEGREES

LL. D., Harvard, 1905.
LL. D., Lafayette, 1905.
Sc. D., University of Pittsburgh, 1915.

III. PUBLICATIONS

In this incomplete list of writings of Henry M. Howe the following abbreviations are used:
A. I. M. E.—Transactions of the American Institute of Mining Engineers.
A. S. M. E.—Transactions of American Society of Mechanical Engineers.
A. S. T. M.—Proceedings of the American Society for Testing Materials.
I. A. T. M.—Proceedings of the International Association for Testing Materials.
J. I. S. I.—Journal of the Iron and Steel Institute.
E. & M. J.—Engineering and Mining Journal.

PRINCIPAL WORKS

The metallurgy of steel. Scientific Publishing Co., New York, 1891. Translated into French.
Metallurgical laboratory notes. 1902. Translated into French.
Iron, steel, and other alloys. 1903. Translated into Russian.
Metallography of steel and cast iron. 1916.
Article on iron and steel in the new volumes of the 10th edition of the Encyclopædia Britannica. 1902.
Article on iron and steel in the 11th edition of the Encyclopædia Britannica, vol. 14, p. 801, 1910.

PROFESSIONAL PAPERS

Improvement in processes and furnaces for reducing and smelting ores; one-half assigned to Mr. Howe by William E. C. Eustis. U. S. Patent No. 209554, November, 1878.
Blast-furnace economy. A. E. M. E., 3, 1874–75.
What is steel. E. & M. J., Aug. 28, Sept. 4, Sept. 11, Sept. 18, 1875.
Thoughts on the thermic curves of blast furnaces. A. I. M. E., 5, 1876–77.
The nomenclature of iron. A. I. M. E., 5, 1876–77.
A direct process of copper smelting. A. I. M. E., 7, 1878–79.
Two new processes for the extraction of nickel from its ores. A. I. M. E., 9, 1880–81.
On comparative efficiency of fans and positive blowers. A. I. M. E., 10, 1881–82.
Contributions to the metallurgy of nickel and copper. A. I. M. E., 10, 1881–82.
A suggested cure for blast-furnace chills. A. I. M. E., 11, 1882–83.
A systematic nomenclature for minerals. A. I. M. E., 12, 1883–84.
The patience of copper and silver as affected by annealing. A. I. M. E., 13, 1884–85.
Note on the contraction of iron under sudden cooling. A. I. M. E., 14, 1885–86.
Attainment of uniformity in the Bessemer process. A. I. M. E., 15, 1886–87.
Smelting cupreous pyrites. E. & M. J., March, 1879.
Bessemerizing sulphides. E. & M. J., May, 1879.
Bessemerizing matte in a reverberatory furnace. E. & M. J., March, 1883.
Bessemerizing copper matte. E. & M. J., April, 1883.
The Bessemerizing of copper matte. E. & M. J., May, 1883.
The Hunt and Douglas copper process. E. & M. J., December, 1885.
Rose Polytechnic Inst., annual scientific address, 1885.
Copper smelting. Bulletin 26, U. S. Geol. Survey, 1885.
The Clapp-Griffiths Bessemer plant. Science, 1885.
Bad rails. E. & M. J., May, 1886.
The attainment of uniformity in the Bessemer process. E. & M. J., June, 1886.
The effect of slag on the fibrousness of Avesta steel. E. & M. J., September, 1886.
The Elizabeth copper mine, Vermont. E. & M. J., November, 1886.
The manufacture and cost of coke. E. & M. J., November, 1886.
The quality of steel for guns. E. & M. J., January, 1887.
Modern manufacture of steel. E. & M. J., 1887.
Two conditions of phosphorus in iron. A. I. M. E., 16, 1887.

Momentary depression of the elastic limit at two critical temperatures. Tech. Quarterly, December, 1888.
Heat treatment of steel. Cornell University lecture. Scientific Amer., 1888–189.
Special report to the U. S. commissioner on mining and metallurgy at the Paris Exposition, 89.
An electric-resistance magnesia crucible furnace for laboratory use. A. I. M. E., 31, 1901.
Thermal properties of slags. A. I. M. E., 18, 1890.
Notes on the Bessemer process. J. I. S. I., February, 1890.
Pyrometers and pyrometric data. E. & M. J., June, 1890.
Is magnetic oxide electro-positive to metallic iron? E. & M. J., June, 1890.
Darby's recarburizing process. E. & M. J., July, 1890.
Plural tests. E. & M. J., July, 1890.
Bull's metal and the breaking-down point. E. & M. J., August 1890.
Aluminum in iron. E. & M. J., August, 1890.
Why do steel-tired wheels wear flat less than chilled cast-iron ones? E. & M. J., July, 1891.
Bessemer process. Revue University des Mines, 1891.
Manganese steel. A. S. M. E., 1891.
Note on Manganese steel. A. I. M. E., 21, 1892.
Manganese steel. Jl. Franklin Inst., February and March, 1893.
The heat treatment of steel. A. I. M. E., 23, 1893.
The physics of steel. A. I. M. E., 24, 1894.
The crystallization of iron. E. & M. J., November, 1894.
Our possibilities. Presidential address. A. I. M. E., 24, 1894.
The relative corrosion of wrought iron and steel. The Mineral Industry, 1895.
The hardening of steel. J. I. S. I., 1895.
A possible explanation of kernel roasting. E. & M. J., March, 1895.
The hardening of steel. E. & M. J., August, 1895.
The relation between temperature and the grain of steel, by A. Sauveur and H. M. Howe. E. & M. J., December, 1895.
The relative strength of wrought-iron and steel pipe. E. & M. J., April, 1898.
Hardening power of low-carbon steel. Metallographist, July, 1898.
Equipment of metallurgical laboratories. A. I. M. E., 29, 1899.
An explanation of the rapidity of the Bertrand-Thiel process. E. & M. J., September, 1899.
The critical ranges in iron and steel. Metallographist, October, 1899.
L'Equipment des laboratoires metallurgiques. Revue Universelle des Mines, 1899.
Rapidite du proceede Bertrand-Thiel. Revue Universelle des Mines, 1899.
The color names for high temperatures. E. & M. J., January, 1900.
Remarks on the constitution of cast iron. A. I. M. E., 31, 1900
Piping and segregation in steel ingots. A. I. M. E., 38, 1907.
The influence of silicon and sulphur on the condition of carbon in cast iron. A. I. M. E., 30, 1900.
The relative corrosion of wrought iron, soft steel, and nickel steel. E. & M. J., August, 1900.
Report on iron and steel metallurgy. Inter. Univ. Exposit. at Paris, 1900.
L'Enseignement du laboratoire de metallurgie. Revue Internat. de L'Enseignement, 1901.
The constitution of cast iron, with remarks on current opinions concerning it. The metallographist, 1901.
Progres realise depuis 1889 dans la metallurgie du fer et de l'acier. Bulletin de la Société de l'Industrie Minerale, 1901.
What is the essence of crystalhood? Metallographist, January, 1902.
Metallurgical laboratories. Science, n. s., May 16, 1902.
Annual address by the retiring president. A. S. T. M., 1902.
On the constitution of cast iron. A. S. T. M., 1902.
The freezing point curve of binary alloys of limited reciprocal solubility when molten. Metallographist, 5, 1902.
Progress in the metallurgy of iron and steel, especially in the open-hearth process since 1889. Cassier's Mag., Oct., 1902.
Roberts-Austen. Biographical sketch. Metallographist, January, 1903.
An introduction to the study of alloys. Engineering Mag., August, 1903.
The technical school and the university. Iron Age, January, 1903.
Organization of higher technical education. Journ. Assn. of Amer. Universities, January, 1905.
Biographical notice of Sir Lowthian Bell, Baronet. A. I. M. E., 36, 1905.
Report on the Buda-Pesth congress. I. A. T. M., September, 1901.
Laboratoires de metallurgie. Revue Universelle des Mines, 1905.
The uniform nomenclature of iron and steel. I. A. T. M., 1906.
Experimental double muffle gas-heating furnace studying the laws of the heat-treatment of steel. A. S. T. M., 1906.
The relative corrosion of wrought iron and steel. A. S. T. M., 1906.
Report of committee F on heat treatment. A. S. T. M., June, 1906.

The roasting of the argentiferous cobalt nickel arsenides of Temiskaming, Ontario, Canada (with Drs. Wm. Campbell and C. W. Knight). A. I. M. E., 38, 1906.
Why do fluid slags cause slopping in the Bessemer converter? Electrochem. & Metallurg. Industry, July, 1906.
The influence of the conditions of casting on piping and segregation, as shown by means of wax ingots. A. I. M. E., 38, 1907.
Does the removal of sulphur and phosphorus lessen the segregation of carbon? A. S. T. M., 1907.
How may the quality of steel rails be improved? E. & M. J., July, 1907.
A new iron-carbon phase, osmondite. Electrochem. & Metallurg. Industry, September, 1907.
Not the dream of a dreamer but the vision of a prophet. E. & M. J., October, 1907.
A further study of segregation in ingots. E. & M. J., November, 1907.
The duplex process for steel making. Electrochem. & Metallurg. Industry, January, 1908.
Segregation in steel ingots. School of Mines Quart., April, 1908.
The shape of the iron blast furnace. E. & M. J., September, 1908.
The relative corrosion of steel and wrought-iron tubing. A. S. T. M., September, 1908.
The air-furnace process of preparing white cast iron for the malleablizing process. A. I. M. E., 39, 1908.
Carbon and the properties of cast iron. E. & M. J., November, 1908.
The carbon-iron diagram. A. I. M. E., 39, 1908.
Notes on the use of the tri-axial diagram and triangular pyramid for graphical illustrations. A. I. M. E., 28, 1898.
Manganese steel mold. U. S. Patent, No. 913,728, March, 1909.
The metallography of iron clarified. Electrochem. & Metallurg. Industry, October, 1909.
On the uniform nomenclature of iron and steel. I. A. T. M., 1909.
The treatment of steel in electric furnaces. E. & M. J., August, 1909.
The closing of blowholes in steel ingots. A. S. T. M., 1909.
Influence of ingot-size on the degree of segregation in steel ingots. A. I. M. E., 40, 1909.
Influence of top-lag on the depth of the pipe in steel ingots. A. I. M. E., 40, 1909.
The welding of blowholes in steel. A. S. T. M., 1910.
Piping in steel ingots. Iron Age, January, 1910.
Memorial of C. B. Dudley. A. S. T. M., 1910.
Allotropy or transmutation? Repts. British Assn., 1910.
Closing and welding of blowholes in steel ingots. Repts. British Assn., 1910.
Process of making manganese steel. U. S. Patent No. 954188, April, 1910.
An optimist's view of the iron ore supply. Atlantic Monthly, June, 1910.
Practice recommended for annealing miscellaneous rolled and forged carbon steel objects. A. S. T. M., 1911.
Nucleus action and grain growth. Met. & Chem. Engineering, February, 1911.
The life history of cells and grains in steel. Int. Zeit. fur Metallographie, 1911.
A. S. T. M. annual Pres. address, June 27, 1911.
Life history of network and ferrite grains in carbon steel. A. S. T. M., 1911.
Nomenclature of the microscopic constituents of iron and steel. Met. & Chem. Engineering, January, 1912.
The upward concentration of kish by flotation. Met. & Chem. Engineering, June, 1912.
Does commercial hyper-eutectic white iron free from manganese exist? J. I. S. I., II, 1912.
Annual address by the retiring president. A. S. T. M., 1912.
The life history of pro-eutectoid cementite, by Howe and Levy. I. A. T. M., 1912.
Why does lag increase with the temperature from which cooling starts? A. I. M. E., 45, 1913.
Presidential address to the VIth Congress of the I. A. T. M. A. S. T. M., 1912.
Belated coalescence vs. balling up as the cause of the degradation of the network structure of hypo-eutectoid steel (with A. G. Levy). Internationale Zeitschrift fur Metallographie, September, 1912.
Notes on Ruff's carbon-iron equilibrium diagram. A. I. M. E., 44, 1912.
The influence of divorcing annealing on the mechanical properties of low-carbon steel. Howe and Levy, A. I. M. E., 44, 1912.
The nomenclature of the microscopic substances and structures of steel and cast iron. I. A. T. M., 1912.
Discussion of Mr. Talbot's paper on prerolled pipeless ingots. Eng. News, November, 1912.
The closing of pipes in steel ingots. E. & M. J., Dec., 1912.
Note on Henry LeChatelier. Met. & Chem. Engineering, 1912.
On uniform nomenclature of iron and steel. I. A. T. M., 1912.
An explanation of the Talbot method of making solid ingots. Iron Age, February, 1913.
The value of expert opinions. Jl. of Industrial & Engineering Chemistry, March, 1913.
In what direction is technical education tending? A. I. S. I., May, 1913.
Ae 1, the equilibrium temperature for A 1 in carbon steel. A. I. M. E., 47, 1913.
Determination of the position of Ae 3 in iron carbon alloys. Howe and Levy. A. I. M. E., 47, 1913.
Discussion of the existing data as to the position of Ae 3. A. I. M. E., 47, 1913.
Notes on the plastic deformation of steel during overstrain, by Howe and Levy. A. I. M. E., 50, 1914.
Are the effects of simple overstrain monotropic? A. S. T. M., 14, No. 2, 1914.
Notes on the divorcing, annealing, and other features of structural coalescence in iron and steel, by Howe and Levy. Cleveland Inst. of Engrs., July, 1914.

Address at the formal opening of the new buildings of the Perkins Inst. and Mass. School for the Blind, June 4, 1914.
Hardening with and without Martensitization. Trans. of the Faraday Soc., 10, 1914.
Are the deformation lines in manganese steel twins or slip bands? Howe and Levy. A. I. M. E., 51, 1915.
General principles of the control of piping and segregations in steel ingots. A. I. S. I., October, 1915.
Control of piping and segregation in ingots. Iron Age, October, 1915.
On the formation of columnar and of free crystals during solidification. Met. & Chem. Engineering, December, 1916.
Notes on the hardening and tempering of eutectoid carbon steel. A. S. T. M., 16, No. 2, 1916.
Notes on pearlite. Howe and Levy. J. I. S. I., II, 1916.
Recrystallization after plastic deformation. A. I. M. E., Bulletin, October, 1916.
On grain growth. A. I. M. E., Bulletin, December, 1916.
The relations between engineering and science. Science, March, 23, 1917.
The erosion of guns. A. I. M. E., No. 134, page 335–390, 1918.
Work of Eng. Division of N. R. C., Washington, U. S. A. Jl. Iron & Steel Inst., No. 1, p. 133, 1919.
Prevention of columnar crystallization of ingots. H. M. Howe and E. C. Groesbeck. A. I. M. E. February, 1919.
Stresses caused by cold-rolling. H. M. Howe and E. C. Groesbeck. A. S. T. M., V. 20, Pt. II, pp. 31–37, 1920.
Acid open-hearth process for manufacture of gun steels and fine steels. A. I. M. E., Feb. 1922; Iron Trade Review, March 16, 30, Apr. 20, 1922, vol. 70, pp. 749–753, 893–895, and 1117–1123. Howe and Barba.
Open-hearth furnaces and processes. Iron Age, V. 105, p. 545–546, February 19, 1920.
Influence of quenching temp. and time of tempering on properties of steel. H. M. Howe and A. G. Levy. A. S. T. M., v. 16, Pt. II, pp. 7–50, 1916.
Structure of steel during solidification. Metallurg. & Chem. Eng., v. 15, p. 623–625, 1916.
Microstructure of steel. A. I. M. E., pp. 1365–1367, 1917.
Influence of manganese on steel. A. S. T. M., Iron Age, v. 100, p. 239, 1917.
Influence of temperature, time and rate of cooling on physical properties of carbon steel. H. M. Howe, Francis B. Foley, and Joseph Winlock. Transactions of the A. I. M. E., 1923.

Alfred G. Mayor

NATIONAL ACADEMY OF SCIENCES

Volume XXI
EIGHTH MEMOIR

BIOGRAPHICAL MEMOIR ALFRED GOLDSBOROUGH MAYOR

1868-1922

BY

CHARLES B. DAVENPORT

Presented to the Academy at the Annual Meeting, 1924

ALFRED GOLDSBOROUGH MAYOR

By C. B. Davenport

INTRODUCTION

This biography of Alfred Goldsborough Mayor is based largely upon a remarkable document entitled "Autobiographical notes, by Alfred Goldsborough Mayor, written in response to the request of the chairman of the committee upon biographical memoirs of the National Academy of Sciences, January, 1917, with notes to 1919, inclusive." This manuscript had been deposited, sealed, with the secretary of the academy. It is, indeed, the most extensive document prepared in response to this call. The fact that Mayor had thus prepared it greatly lightened the task of his biographer. It is to be hoped that other members of the academy will be led by his example to prepare such autobiographics, as a part of the duty that they owe to science. The manuscript of Mayor is often quoted here, and such quotations are usually designated by the symbol (A. G. M., MSS.). Mayor compiled, with Dr. R. S. Woodward, the National Academy's memoir of his father, and this is referred to as Mayer and Woodward (1916). For data concerning the Goldsboroughs I am indebted to several members of the family, and particularly to Mrs. Amelia F. Tyler, of Luray, Va., A. G. Mayor's mother's sister.

BIOGRAPHY

Alfred Goldsborough Mayor [1] was born April 16, 1868, at his mother's father's home, "Sunnyside," near Frederick, Md., son of Dr. Alfred Marshall Mayer, professor of physics at Lehigh University, Bethlehem, Pa., and Katherine Duckett Goldsborough, his wife. His mother died May 2, 1868, at "Sunnyside," of puerperal fever (A. G. M., MSS.). After his mother's death the infant was taken to his father's home at Bethlehem. When Alfred was 16 months old, Professor Mayer married a second time, Miss Maria Snowden, of Anne Arundel County, Md. In 1874 Dr. Alfred M. Mayer became professor of physics at Stevens Institute, and the family was brought to South Orange and later to Maplewood, N. J., where young Alfred passed his boyhood. Alfred entered Stevens Institute at the age of 16 years and pursued the engineering course, in consonance with his father's desire rather than his own tastes. He graduated four years later, M. E., 1889, and was appointed assistant to Professor Michaelson at Clark University, Worcester, Mass. Here he made many friends, but he stayed only one year. He was assistant in physics under Professor Blake at the University of Kansas from 1890 to 1892. He left there rather abruptly in the early spring of 1892 and came to Harvard University to study biology; since his attempts at physics had been failures, due largely to a dislike of the subject.

At Harvard a new world opened to him, an opportunity to study zoology, following a strong, apparently innate, bent. He took advanced courses in the subject and began research on butterflies and moths, their colors, color patterns, and pigmentation in general. During the summer of 1892 he was invited, with some other Harvard students of zoology, to study at Alexander Agassiz's laboratory at Newport. Mr. Agassiz encouraged him to make drawings and to observe the habits of the Medusæ, and he was so successful that, before the summer was over, Mr. Agassiz suggested that they cooperate in the preparation of an illustrated work upon the Medusæ of the Atlantic coast of North America. This quickly led to the close association of Mayor with Agassiz in many of his trips. In the winter of 1892–93 Mayor served as Agassiz's assistant upon his cruise in the chartered yacht *Wild Duck* in the Bahamas and Cuba. In 1896 he went with Agassiz in the *Croyden* to the Barrier Reef of Australia, and

[1] "On August 5, 1918, the family name was changed from Mayer to Mayor by the Court of Common Pleas of Mercer County at Trenton, New Jersey. The family having been loyal American citizens since 1785, it seemed but fitting to repudiate the last link of association which bound us to a nation that had forced our country into the line of its enemies." A. G. M., MSS. See comment of Professor Mendenhall, Science, Aug. 18, 1922.

thence around the world. In 1898 the Fiji Islands were explored in the steamer *Yaralla*, and finally in 1899–1900 the *Albatross* took Agassiz and his party, including Mayor, across the tropical Pacific, visiting the Marquesas, Paumatos, Society, Cook, Nicue, Tonga, Fiji, Ellice, Gilbert, Marshall, and Caroline Islands, and thence to Japan. Mr. Agassiz sent Mayor upon minor expeditions ranging from the Bay of Fundy to Tortugas, Fla.

The prolonged voyages with Agassiz naturally interfered much with his scholastic work. Moreover, an inflammation of the left eye made it necessary for him to spend 1893–94 in a dark room. This episode led him to avoid using the higher powers of the microscope and determined to a considerable degree the character of his future researches. He made a trip to France and Belgium and returned to Harvard in the autumn of 1895. He was given the degree of Sc. D. by Harvard University, 1897.

In 1895 Mayor was appointed assistant in charge of radiates in the Museum of Comparative Zoology at Harvard and retained that position until 1900. In that year he was elected curator of natural science in the new museum of the Brooklyn Institute of Arts and Sciences. He was married on August 27, 1900, to Miss Harriet Randolph Hyatt, a woman of marked artistic gifts, daughter of Prof. Alpheus Hyatt, and they began housekeeping in Brooklyn. Four children were born to them, Alpheus Hyatt in 1901, Katherine Goldsborough in 1903, Branz in 1906, and Barbara in 1910. In 1904 he was elected curator in chief of the museum, a position which enabled him to make certain desirable changes in the management and policy of the institution. While serving the Brooklyn museum he went on scientific expeditions to Florida and the Bahamas, and dredged off the Massachusetts coast in the yacht *Philopena*, lent for the purpose by its owner, H. B. Stearns, Esq. As curator he not only accumulated collections but he inaugurated two publications: Science Bulletin and Memoirs.

At the December, 1903, meeting of the trustees of the Carnegie Institution of Washington it was decided to establish a marine laboratory upon the Tortugas Islands, Fla., and Doctor Mayor, who had urged the laboratory in Science, was appointed its director. He assumed this position in the summer of 1904. Mayor writes:

> A sixty-foot, ketch-rigged yacht having a 20-horsepower auxiliary engine was constructed at East Booth Bay, Maine, during the summer of 1904, and in the meantime two large wooden portable buildings were transported from New York to Tortugas, and landed upon the beach at Loggerhead Key, where they were erected in July, 1904, to serve as a nucleus for the establishment of the laboratory. In less than three weeks the bay cedars had been cut away and the two buildings erected on Loggerhead Key, Tortugas.
>
> Soon after this, the yacht *Physalia* was completed and we sailed out from Booth Bay, Maine, late in August, going slowly down the coast, making frequent surface hauls to study the medusæ, siphonophores, and ctenophores. Thus we put into almost every harbor between northern Maine and Southern Florida, and arrived at Miami in February, 1905. The seaworthy qualities of the yacht were well tested in a hurricane north of Cape Hatteras, which she survived with only the loss of her jib, while all other vessels within sight of us were driven ashore. (A. G. M. Mss.).

While this was the first, it was by no means the worst, storm the *Physalia* weathered during the seven years in which she served the laboratory. In 1911, she was replaced by the twin-screw, 100-horse power, 70-foot vessel *Anton Dohrn*, which was built at Miami, Fla.; being the largest yacht that had hitherto been constructed in southern Florida. Of the work of his department, Mayor states:

> It was the plan of the department to offer to well qualified investigators exceptional opportunities to pursue researches for which the tropical ocean affords peculiar advantages. Thus intensive studies have been conducted in the fields of physiology, ecology, heredity, evolution, animal psychology, variation, the geology and growth of coral reefs, the bacterial precipitation of limestone in tropical seas, the chemical constitution of sea water as a physiological fluid, the coloration of reef fishes in relation to environmental influences and natural selection, the habits of sea gulls, memory and warning coloration in fishes, the systematic description of new and interesting animals, and the ecology and physiology of plants of the region. Indeed, the major part of the researches which our country has produced as a result of studies of the marine life of the West Indies since the laboratory was started has been performed under the auspices or with the cooperation of the department of marine biology.

Mayor's department soon provided at the Tortugas the most thoroughly equipped marine biological station in the tropical world. To this station he returned each spring, with a number of investigators, and here they remained until August, when the hurricane season was apt to begin.

The activity of the Tortugas laboratory is indicated by the nearly 4,500 quarto pages and 500 plates resulting from researches done under its auspices. Just to gather together these papers, to prepare them for and see them through the press was a great task, mostly performed during the autumn and winter. At the same time he was engaged in writing up his own researches. In addition he undertook personal researches in or led parties into other parts of the world. In 1907–8 he studied marine life off the coast of Cornwall, England, and at the Naples Marine Biological Station he spent a delightful winter investigating medusæ. During the period from 1912 to 1916 he visited the various West Indian Islands seeking a site for a permanent laboratory. In 1913 he went with a party consisting of Frank A. Potts, of Cambridge University, and Drs. Clark, Harvey, and Tennant from America to Murray Island in Torres Straits, Australia, and the Island of Papua. Here echinoderms, crustacea, and corals were studied, ecologically and physiologically; later the party traveled eastward via Java and Europe, thus completing the voyage around the world. In 1917, he went to Tutuila, Samoa, to study the problems of coral reefs and growth-rate of corals in the Pacific, and to this island he returned in 1918–19 in company with Prof. R. A. Daly, of Harvard, and Mr. John W. Mills, the engineer of the department. At this time Mayor studied the submerged seaward slope of the coral reefs and planted out, weighed, measured, and photographed corals down to 50 feet of depth. In May–July, 1920, he studied at Samoa and at Fiji, for the last time.

During most of the period of his connection with the Carnegie Institution of Washington Mayor's home was in Princeton. In 1910–11 and again in 1915 to his death he held an honorary appointment of lecturer in biology at Princeton. Among other appointments and honors received by Mayor were the following: In 1904, after retiring from the Brooklyn Institute of Arts and Sciences the trustees gave him the complimentary title of "Honorary curator of Natural sciences." From 1903, he served on the scientific council of the New York Aquarium, and for some years after as a member of the board of trustees of the marine biological laboratory at Woods Hole. He was president of the Cambridge Entomological Club in 1899; president of the eastern branch of the American Society of Zoologists, 1913; vice president of the Washington Academy of Sciences in 1915. In 1916 he was elected a member of the National Academy of Sciences. Other societies of which he was a member and in which he took an active part were: The New York Zoological Society, fellow and patron; the American Society of Naturalists, the Society for Experimental Biology and Medicine, the Academy of Natural Sciences of Philadelphia, and the American Philosophical Society.

During the World War Mayor's activities were for a time directed into war work. The *Anton Dohrn* was leased to the United States Navy from July, 1917, to November, 1918, to serve as a patrol boat, guarding Key West Harbor. In August, 1917, Mayor passed the examinations for a first-class mariner's license for seagoing yachts and then taught navigation to enlisted men of the Navy who, being under age, were permitted to return to their studies at Princeton University. In the autumn of 1918 he taught seamanship in the student's Army training corps at Princeton University up to the time of the armistice. In connection with this work he published a booklet on "Navigation, illustrated by diagrams."

After two years of ill health, he died on June 24, 1922.

PHYSICAL TRAITS

Mayor was about 67 inches tall—slightly under the average stature of Anglo-Saxons. His mother was 2 inches below the average stature of women and his father was also not tall. He remained always lithe and slender.

His eyes were blue, like those of both parents, deep set, and capable of the liveliest expression. His brown hair, even as cut close, had a marked wave, and it is stated that he had ringlets as a small child. His mother's hair was quite straight, but his father's was curly, and the gene for this trait came, doubtless, from the paternal side. Mayor did not have thick hair from early youth, if ever. By 40 it was sparse on top, though there was no baldness. His father had similar sparse hair. His face was rather short and broad, with fairly high cheek bones like his father's. He had a rather large chin, like his mother. His step was quick and short.

ELEMENTS OF SCIENTIFIC MAKE-UP

Fondness for natural history.—Alfred G. Mayor early showed a great interest in living things. Especially butterflies attracted him. As a boy he collected them and made drawings of them and painted them in extraordinarily lifelike fashion, so that it seemed hardly possible that the iridescent and shimmering wings had not been pasted on the page. It can not be doubted that there was an unusually acute capacity for color discrimination and sense of form that lay back of all this behavior. The capacity was so great that its exercise was highly successful and gave great satisfaction. There is no doubt, from his own statements made to the writer, that from an early age there was nothing else gave so keen a delight as animals of striking form and color. And just this capacity of discrimination guided his pen and brush and made him an animal artist of high quality. In his autobiographical notes he stresses a certain love of solitude in his boyhood as contributing to his contact with nature.

I threw myself heart and soul into a world of the imagination wherein I lived apart from man, and sought my playmates among the creatures of the woods and fields. I literally loved individual butterflies I had raised from early larval stages, and exulted in their imagined joy as they flew from my hand to flutter over the clover-laden fields. Only when sorely needed for specimens in my collections did I force myself to kill the beautiful creatures which seemed so wholly to accord with the world of flowers and sunshine I myself adored.

Even at Bethlehem, when not more than three years old, I remember being held spellbound by the operations of wasps building their nest in the window shutter of my nursery. . . . I also pondered over the reason for the roundness and smoothness of the white pebbles that formed the paths around my father's house. The frog pond was a universe of waters; but the climax came when a blue-purple butterfly (*Basilarchea ursula*) flitting in the sunlight filled my little mind with such rapture of delight that I must needs run to my beautiful old grandmother only to learn the miraculous fact that "butterflies come from caterpillars."

He lived at South Orange and Maplewood, N. J., from 1874 to 1889. He continues:

Day after day throughout the summer I wandered forth, butterfly net in hand, and before my teens were passed I had reared, and made colored drawings of, nearly every species of butterfly and many of the moths known from this region.

This intense interest in the beauties of natural form and color was found also in his father, who at an early age "plunged into the pursuit of all things scientific, from collections of insects to the study of the stars" (Mayer and Woodward, 1916). Later in life A. M. Mayer edited books on sport, including articles on fish and game birds. Of his mother it is stated that she was fond of all nature, and so must have been her father, who lived in the country, farmed and enjoyed gunning, and her mother, whose special interest was in her flower garden and in trees.

A. G. Mayor's love of organisms was thus keen and lasting. While his father required him to go through an engineering school, he says:

Almost every spare hour of my college years was given to natural history; and hundreds of colored drawings of turtles, snakes, newts, frogs, and insects had resulted from these charmed hours of exultation. (A. G. M., MSS.)

The three years employed in teaching physics were painful to him, and he was in an "ecstacy of delight and hope" when he entered the zoological laboratory at Harvard University to be started upon that professional career as a zoologist, in fidelity to which he never wavered.

Ability in animal painting and interest in color.—As stated in his autobiography, Mayor desired to record permanently the beauties of the animals whose form and color so moved him. The faithfulness of his reproductions was uncanny, and it was this ability that brought Agassiz's suggestion that he make colored drawings of the jellyfishes. This he did for many years, and these paintings are reproduced in color in Mayer's Medusæ of the World, in three volumes, of which he modestly says:

It has always been a sorrow to me that when (they) were finally published in 1910 Dr. Agassiz had passed away, and he had declined to permit his name to be associated with the work, which was in truth the fruit of his inspiration." (A. G. M., MSS.)

Other applications of physics, chemistry, and mathematics to biology.—After Mayor's appointment as director of the marine laboratory of the Carnegie Institution it was natural for him to continue his studies of the beautiful jellyfishes. Their pulsation had long interested him, and he sought to get light on its causes. He entered upon this novel research with much enthusiasm, and

made the most interesting and ingenious experiments on the matter, by cutting up the muscular disk into strips of various sorts in which contraction waves were trapped and kept going for days. He used his training in chemistry to advantage in many researches. He studied the effect of various solutions on contractions. Thus he found that sodium chloride of the sea water is the chief stimulant to pulsation in Cassiopea, while magnesium is the chief inhibitor. These chemical studies became elaborated in the successive years. They led to a new and greatly improved method of stupefying marine animals by means of a pure magnesium solution isotonic with sea water and to a method of stupefying by carbon dioxide. He found that—

sodium, which is a powerful nervous and muscular stimulant, depresses the movements of the rhythmically beating cilia of trochophore larvæ, ctenophores, etc. On the other hand, these cilia beat at an abnormally rapid rate in magnesium. There is thus a converse relation in the effects of these ions upon the activity of these cilia in comparison with their effects upon nerves and muscles, for which sodium is a stimulant and magnesium a depressant. The explanation is that cilia-bearing cells are very sensitive to pressure and when the surface of the body is bathed in sodium a strong muscular contraction results, thus augmenting the pressure upon ciliated cells and stopping them." (A. G. M., MSS.)

Later, he studied the degree of acidity of the sea water in the zone of the coral reefs.

His researches, similarly, led him to use his training in physics, as already suggested. While he was assistant in physics at Kansas he began a research which showed that leaves are as efficient as a lampblacked surface in their ability to absorb or to radiate heat, but if dew collects upon the leaf its radiation is reduced, becoming that of a water surface. This research involved the use of a highly sensitive thermopile and a reflecting astatic Thomson galvanometer; he had to devise still other apparatus. In his researches on color in Lepidoptera (which is located on the scales) he showed, by pendulum experiments, that scales are not useful to increase the friction between wings and air. At the Tortugas Dr. T. W. Vaughan, who was studying the growth of corals, needed information on the effects of temperature upon Coelenterates and Mayor made the required studies, which are published in Nos. 40, 41, 44, and 54 of his bibliography. These temperature studies were made, inter alia, on the large medusa, *Aurelia aurea*, the only Scyphomedusa that extends from pole to pole. At Tortugas, where at the surface the sea is often 29° C., this temperature is an optimum for Aurelia, which suffers if the temperature is appreciably higher or lower than 29°. At Halifax, on the other hand, Aurelia is killed by a temperature of 29°; but it may be frozen with impunity. There is thus a remarkable adjustment of this organism in different climates to the temperature conditions of the various regions. The results of these temperature studies he applied to the problem of coral reefs. It appears then that Mayor's engineering training was of great use to him in the solution of biological problems. It is obvious that he had some of his father's dexterity in physical manipulation.

Mathematics, Mayor states in his autobiography, was one of the courses at Stevens Institute which attracted him. Accordingly, we find him making much use of it in his researches. He discusses mathematically the degree of retardation of the pendulum if the scales on the wings of Lepidoptera function to increase friction. He discusses mathematically the chances that a given mutation should have occurred independently in the 5 species out of 25 in the genus in which it is found. He finds that the starved jellyfish loses weight in accordance with the formula $y=w\quad(1-a)^x$, where y is the weight at the end of x days, w is the weight at the beginning of the experiment, and a is a constant less than unity. Again, he shows that the rate of nerve conduction in Cassiopea increases in a direct ratio as the electrical conductivity of the sea water increases between salinities of 18 per cent to 40 per cent in accordance with the formula $y=0.945x+4.4$, where y is the rate of nerve conduction (that in normal sea water being 100), x is the degree of dissociation of the cathions of sodium, potassium, magnesium, and calcium in sea water; that of normal sea water of 36.24 salinity and 8.22 PH being 100. Repeatedly he succeeds in expressing relations between environmental conditions and vital response in a simple formula; he was satisfied with nothing less. In this respect Alfred G. Mayor was like his father, whose law of the relation between the time during which the after sensation of a sound does not appear to diminish in intensity and the number of vibrations

20154°—26——29

per second is expressed in a formula known as Mayer's law. Mayor applied his facility in mathematics, especially trigonometry, in his little text-book on Navigation, prepared during the World War.

Mayor's interest in color, even apart from form, is well illustrated in his paper "On the color and color patterns of moths and Lepidoptera"—one of his earliest researches at Harvard. In Plates 6 to 8 of this paper are representations of the color areas of butterflies drawn in a sort of "Mercator's projection" of the wing, which made homologous areas comparable but distorted the pattern of the whole, so as to elicit a strong protest from Alfred R. Wallace, in "Nature," to the effect that the essential significance of the color areas as mimetic, or protective, was lost. Wallace missed the point, namely, that any mimetic pattern is, after all, limited by the physiological developmental capacities of the organism. In this research color itself became a special object of investigation. The relative frequency of the different kinds of colors was plotted; the pigment colors were quantitatively expressed by means of Maxwell's disks and analyzed spectroscopically by a special apparatus. Here Mayor's physical training again stood him in good stead. While he reacted strongly against physics as a subject of research (doubtless because it once threatened to oppose his main interests), yet he readily applied physics to biology. Had he become a physicist he would probably have become a student of light and color, subjects which his versatile father took up again and again. It may be added that Mayor returned to the topic of color and color patterns in Lepidoptera repeatedly. His doctor's thesis considered the development of pigment in the wing; he formulated a new (and most valuble) hypothesis of seasonal dimorphism in color; he gave a Woods Hole lecture on the development of color in Lepidoptera; he discussed the value of color in the mating of these insects; in 1902 he published an extensive research on natural selection versus race tendency in relation to the color patterns; in 1906 he published results of experiments on the reactions of caterpillars and moths. Mayor's interest in the great "color-display" group of insects lasted long into the period when other interests had become strong.

Marine work and travels.—Mayor is best known for his work on marine organisms, which opened up an important part of the field of thallassography. He was 24 years of age before he first visited the seashore to do biological work. This was at Mr. Agassiz's laboratory at Newport. Thus he came to be associated with one of the world's leading thallassographers, and this gave Mayor an opportunity to discover his hereditary fondness for the sea. While curator of the Brooklyn museum he undertook various marine expeditions. As the Tortugas laboratory was usually open only from April to August, Mayor had time to make expeditions to other parts of the world; thus, in 1907–8, to Cornwall, England, and to the Naples station. In 1913 Murray Island and also Papua were visited with a party which circumnavigated the globe.

"The greater number of the West Indian islands were visited in 1912–16 with a view to selecting a site for a permanent laboratory, the work of which might in some measure serve to continue that of the Naples station, which had suffered sadly through the World War. Special expeditions were made also to Kingston, Montego Bay, Jamaica, Guanica, Porto Rico, Pigeon Point, Tobago, and Andus Island, Bahamas," and in 1917 and again in the winter of 1918 voyages were undertaken to Tutuila, Samoa, to study the problems of coral reefs and the growth rate of corals in the Pacific. It is probable that no other biologist of this epoch has had so extensive as well as detailed acquaintance with all the seas and seashores as Mayor.

The results of Mayor's researches on coral reefs are described by him as follows: It was found that, generally speaking, those forms which can withstand high temperature are also correspondingly well able to withstand the smothering due to being buried under mud. Hence those corals can live in the shallow reef flats near shore, where the temperature is high and the silt abundant. On the other hand, the corals which must live in relatively cool water are confined to the seaward parts of the reef where they are surrounded by cool water free from silt. Thus the correlation between temperature reactions and the effects of silt account for the peculiar distribution of the various species of corals over the reef flat.

This fact was discovered in 1913 at Maer Island, in the Murray Islands of Torres Straits, Australia. Here Mayor made the first quantitative ecological study of a coral reef ever undertaken. During his expedition to this region it was found that, despite the fact that the corals of Australia suffer from excessive heat, while those of Florida are correspondingly affected by cold, yet natural selection appears not to have resulted in improving any genus of coral by increasing its ability to withstand temperature conditions. The majority of the *species* of reef corals grow best in the region of the breakers, yet the greatest number of coral heads are found in a zone just shoreward of the inner wash of the breakers, where the water is relatively calm and yet free from silt.

His studies confirm the conclusion that sea water does not dissolve limestone at a rate sufficient to account for the formation of atoll lagoons in this manner. He concludes therefore that Murray and Agassiz were mistaken in assuming that the lagoons of atolls are solution basins. He discovered also that the streams and springs of Samoa and Oahu are alkaline despite the fact that limewater is acid. Thus water pouring outward from the shores of these islands can not dissolve limestone by reason of its acidity, since it is not acid. Other studies, especially at Tutuila, showed that these Pacific corals form limestone at about twice the rate which Vaughan had determined from corresponding genera of corals in the Atlantic.

Finally, in his last trip to Samoa he determined by borings that the reef is underlain by a basaltic rock at a depth of 40 to 50 meters. The modern fringing reefs of this part of Tutuila are not superimposed on the ancient reefs which lie still deeper but are independent structures which have grown out over the submerged basaltic slopes of the island. Mayor consistently entertained doubts as to the universal applicability of the Darwin-Dana theory of coral-reef formation.

There is no question that Mayor enjoyed this life on the sea. Whenever on board the *Physalia* or *Anton Dohrn* he took command; and often assumed grave responsibilities in doing so. The following account of one of his short voyages on the *Physalia* is from his annual report to the Carnegie Institution, 1907.

> Our voyage among the Bahamas proved to be the most adventurous the yacht has yet encountered. On April 1, 1907, a strong southerly gale forced us into a harbor of refuge under the lee of Elbers Cay, about 50 miles southeast of Nassau. The sun was about to set when, on the northern horizon, vast masses of black clouds suddenly arose, driving before them the heavy breakers of the oncoming storm, and in an instant the wind reversed and we found ourselves dragging anchors toward the rocks of a coral reef. With all haste we got the yacht under way. It proved impossible to steam up into the gathering storm, and we had no choice but to scud before it; "jumping" a bar with less depth than our draft, and sailing out between the jagged masses of rock, we reached the open water, where we met the roghest sea the *Physalia* had encountered since she was launched. At midnight the naphtha-tank burst, through the excessive rolling of the vessel, and, with all lights out and only an electric "candle" held close to the binnacle, we went on through the night under storm sails, and when the morning broke we were more than 100 miles away from our former anchorage. A large bark foundered near us in this storm, and a yacht larger than the *Physalia*, which left Miami with us, was never again heard from. However, the *Physalia* returned to Miami in excellent condition, her seaworthy qualities having been thoroughly tested.

What Doctor Mayor does not make clear in this account is that he was at the wheel and his good judgment saved the vessel.

Mayor's long experience in navigation was put by him at the disposal of the Government during the World War. As stated above he taught navigation to naval recruits. There were about 50 men in his first class at Princeton and the course ended in a cruise between New York and Key West in February, 1918, for practice. His success was such that in the autumn he was made lecturer on navigation at Princeton and taught seamanship to 350 men in a training unit there. To facilitate the work of this unit he wrote a small book upon "Navigation Illustrated by Diagrams." A reviewer writes: "Dr. Mayor has the happy faculty of speaking *with* his readers rather than *at* them. . . . The book has a certain charm not often met with in textbooks." This charm, of course, was a reflex of the charming personality of the author. It seems quite certain that Mayor had a strong natural liking for the sea; a genuine thalassophilia; and this was associated with a nomadic tendency. The latter showed itself at an early age in a love of wandering over the country. "In the beautiful country on the

slopes of the Orange Mountains I soon knew every pond and brook, field and forest within five miles of my father's house." Mayor writes further: "Often loneliness has oppressed me in the streets of great cities, but never once upon the sea or in the deep primeval forest, not even when as a boy of thirteen I was lost for a couple of days in the wilds of Maine. I felt angry at the trees that obscured the view but never once *lonely* in their lordly presence." (A. G. M. Mss.) Nomadism is generally a sex-linked trait, found in men and apt to be also in one or more of their mother's male relatives. His mother's brother, Dr. Charles Worthington Goldsborough, married early and had a large family; according to his sister he was "crazy to go to California," had a strong desire to travel, but was tied down by his family of 7 children. These two cases of strong love of travel in mother's brother and nephew are related, as expected, on the assumption that the nomadism was constitutional. However, in the Mayor line are many with an extreme fondness for travel, as Branz Mayer (1878, p. 63) points out.

But in addition to nomadism, Mayor had clearly the trait of thalassophilia; and evidently such a trait should be found in other members of the family on both paternal and maternal sides. On the paternal side such thalassophilia was probably present in Christian Mayer; his father's father's grandfather—a leading East Indian shipowner and merchant of Baltimore, and probably, like most merchants of his day, a voyageur. He organized a marine insurance company. Christian's son Lewis went to India at 8 years (in 1801); was sent as supercargo to Europe at 16 years. Lewis's son, Charles F. Mayer (grandfather of Alfred G. M.) made a voyage to Cape Horn, Valparaiso, and Lima in 1848–1850, and he traveled extensively in the United States and Europe. C. F. Mayer married Eliza C. Blackwell, daughter of Capt. F. Blackwell, a commander in the merchant service. One of their sons (Alfred G. M.'s uncle) was a civil engineer in Brazil, a regular officer of the Engineer Corps, United States Army (1859–1867), who served with special merit under Farragut in the capture of New Orleans.

On the mother's side is Charles Goldsborough of the Confederate Navy, a third cousin of Alfred's mother; also Admiral L. M. Goldsborough, who during the Civil War destroyed the Confederate batteries on Roanoke Island. Thus it is probable that the early trips made by Alfred with Alexander Agassiz awoke an innate love of the sea that determined his career.

Social gifts.—Mayor had social gifts of a high order; among them were companionableness, a love of conversation, a marked sense of humor, and a capacity for administration. His companionableness is testified to by all who traveled with him or worked at the Tortugas laboratory. Thus Dr. Davenport Hooker writes about life at Tortugas:

> "At 6 p. m., after supper, he walked with us along the path to the lighthouse, then to the eastern shore of the island and along the shore to the southwestern tip of the island. Here each one burrowed a comfortable hole in the sand, stretched out and gossiped about books, research people, and things in general, while the sun went down in gorgeous splendor.

At this time, as Prof. E. N. Harvey recalls, they would "listen to Mayor's stories of the chiefs of Fiji, the mutiny of the Bounty, or the narrow policies of the missionaries. Doctor Mayor was one of the most delightful talkers, with an interest in every field under the sun." In his conversational qualities Mayor was like his father, who was "a versatile conversationalist and charming story-teller" (Mayer and Woodward, p. 256). In his love of the bizarre and harmlessly shocking in his stories (like the preferences of cannibals for the flesh of different human races) Doctor Mayor reminded one of his kinsman Edgar Allen Poe—the second cousin of his mother's mother, Amelia Poe, the daughter of Jacob Poe, of Frederick, Md.

Mayor was an excellent administrator. His laboratory at Tortugas and his expeditions to remote seas were planned and conducted in admirable fashion. On the long trips all contingencies were foreseen. At his laboratory menu cards were prepared for a 10-day period so as to provide an adequate rotation of meals. The volumes of reports from his laboratory are evidence of this marked administrative ability.

He had high ideals as to the requirements of courtesy both toward his guests on shore or on the marine equipment; and toward the representative of the Government in the out-of-the-way places that he visited. He always first visited the commandant or chief man of the place where he had occasion to stop. He was punctilious in marine etiquette, deeming it essen-

tial in crises that men and officers should know their places and duties. When a governor or commandant came aboard, the social requirements must be strictly met even if the visitor was a dusky magistrate from one of the Bahamas, who had been invited to dine. But the moment the guests were gone he was the first to remove collar and white coat and plunge into work. So far from being a snob, he was "absolutely democratic at heart and willing to encourage the most insignificant investigator and treat him as a person equal in knowledge and attainments to himself." These high social gifts came from both sides of the house. His father was chosen by the Century Association of New York, on the occasion of the farewell dinner to Professor Tyndall in 1873, to arrange the social details. The Goldsboroughs stood at the top rank socially in Maryland. Two of the family have been governors of the State. Gov. Phillip Lee Goldsboro was a first cousin of Alfred's mother's father; and Gov. Charles Goldsborough, governor in 1818–19, was a first cousin of Alfred's great-grandfather.

Another striking trait of Mayor was his warm-heartedness and his feeling of responsibility for the comfort of those who were associated with him. Two letters of his to Dr. E. N. Harvey are worth printing because they are so characteristic.

S. S. "Houtman,"
At Celebes, Nov. 28th, 1913.

My Dear Harvey: We miss you greatly but hope to find you in Ceylon. The *Dorrego* was a good old roller in the heavy beam sea and we became as smudged as cats in a coal bin but the officers were nice fellows and we enjoyed it hugely. We expected to rough it and give the cannibals and the anopheles a fair chance at us in New Guinea but when I paid my respects to His Excellency the Governor he invited us to be his guests at Government House and provided us with the Government launch to enable us to tow and to see the native villages within 20 miles of Port Moresly, so we saw 40 miles of villages of a very primitive type built out over the water and with natives clothed quite in the old way.

H. E. also provided us with horses but unluckily Potts developed a New Guinea sore on his heel which was quite as bad as the one you had forward so I was glad enough to get him off on the *Houtman* two days before the time we planned for! He is now doing well but it is a slow affair and may necessitate our giving up Java.

I will in any case stay with him.

Everyone in New Guinea has malarial fever badly from the Governor down. 100 white men out of the 1600 in New Guinea died this year but it is a beautiful wild country that calls you in like a siren on the rocks off Cape Sorrento. I wish I could have gone up the Fly River. The cannibals there grab you by the arm and say U–u–u———.

Cordially, Alfred G. Mayer.

Port Said, Egypt, *Dec. 30, 1913.*

My dear Harvey: When Potts and I reached Colombo on Dec. 19th we were distressed to receive your letter telling of the renewed trouble with the New Guinea ulcer.

We confidently hoped to find you in Colombo and our disappointment was very great, especially as I feel sad over having left you. I should not have been so confident that you would continue to improve and should have gone with you.

I will always reproach myself for not having done this, and will not feel content until I hear that you are well and have suffered no permanent injury.

Your spirit in saying that you were glad that the illness came *after* your research was finished is fine, and ought to have been matched by my staying with you as long as you were ill . . . . .

Mayor's regard for the health of his party repeats the qualities of his mother's father, who was the old-fashioned type of country physician, ready to respond to calls at any time of day or night, and to plunge on horseback through rain and mud at the call of "the stork." He was the beloved father confessor for all in physical or mental distress for miles around. He kept slaves and these idolized him. He hated to sell a slave, and on one occasion a slave he had sold acted so badly that he was returned to Doctor Goldsborough, where he remained satisfied and loyal. Mayor recalls that a slave of his grandfather's told him how "in responding to a sick call he found the Monocacy River dangerously swollen and cut one of the horses loose from the carriage and rode on horseback to the opposite shore despite the frightened protest of his devoted servant, who believed his master was plunging to certain death in the rushing torrent." These social traits were found again in Mayor's mother, who wrote delightful letters, was a charming conversationalist, gentle and kind, and a general favorite because always jolly, optimistic, and extraordinarily generous. They appeared again in Mayor's uncle, Charles

Worthington Goldsborough, who reproduced his father's traits as the beloved, unruffled, generous, high-minded country physician. Other members of his fraternity were characterized by a sweet disposition and unselfishness. This Goldsborough family trait of unselfishness Alfred carried into his scientific work, for he readily yielded to others problems toward whose solution he had made some progress, as when he turned over to Prof. H. E. Crampton the studies on Partula he had begun while at the Brooklyn museum. After he wrote a book on Sea-Shore Life he transferred his rights in it to the New York Zoological Society's Aquarium.

Physical health.—Through Mayor lacked a mother's care in early infancy, he developed into an active, tough, slender, somewhat seclusive lad. Always lithe and active, he seemed to have a wiry constitution. Yet he had certain physical limitations characteristic of his family. One of these was a tendency toward inflammation of the eyes. His mother's sister Amelia had to stay in a dark room for six weeks on account of this trouble, and Alfred suffered an apparently similar breakdown after the cruise of the *Wild Duck*, on which he was constantly making drawings. The ciliary muscles of the left eye became inflamed and he had to live for some months in a dark room. He writes characteristically of this period, "my devoted stepmother, who throughout my most hopeless years had not lost faith in me, kept my intellect alive by reading aloud in an adjoining room." (A. G. M., MSS.).

Through his subsequent main period of activity, from 1895 to 1918, Mayor's output of work indicated a man in the prime of health. In the early spring of 1919 and again in 1920 he went to American Samoa and while there examined the submerged seaward slopes of the coral reefs from a diving hood. Returning to America, he fell ill in the autumn of 1920, and it was later decided that tuberculosis had become active. For nearly two years he fought off the disease, just as his first cousin, Henrietta Lee Goldsborough, did. She eventually recovered, but the outcome was not so happy in his case. Despite a warning that it might be fatal to attempt it, he insisted in coming from Arizona to Dry Tortugas to look after the laboratory in 1922 as he had in 1921. There he rapidly grew worse and died in the water, apparently swooning from weakness while bathing on the beach.

As Dr. Asa Schaeffer, who was with him at the Tortugas on that last day, well writes:

> As I think of Mayor's great and absorbing devotion to marine biology, his childlike love of the sea, his passion to get away from the conventional, that peculiar ingredient of wierdness in his personality, but particularly the loving care with which he looked after every detail of the laboratory at Tortugas, even to planting the laboratory grounds with Cocoanut palms, Australian pines, the beautiful scarlet Hibiscus and the delicate Spider Lilies—as these things pass through my mind I can not help but feel that there was a certain appropriatness in his saying his last farewell on the shores of beautiful Tortugas.

SCIENTIFIC PAPERS BY ALFRED G. MAYOR

(Arranged by himself)

1890

1. Habits of the box tortoise. Popular Science Monthly, vol. 38, p. 60–65, 3 figs.

1893

2. Habits of the garter snake. Popular Science Monthly, vol. 42, p. 485–488, 4 figs.
3. The radiation and absorption of heat by leaves. American Journal of Science, vol. 45, p. 340–346, 1 fig.

1894

4. An account of some medusæ obtained in the Bahamas. Bulletin Museum Comp. Zool. at Harvard College, vol. 25, p. 235–241, 3 pls.

1896

5. The development of the wing-scales and their pigment in butterflies and moths. Bulletin Museum Comp. Zool. at Harvard College, vol. 29, p. 209–236, 7 pls.

1897

6. On the color and color patterns of moths and butterflies. Bulletin Museum Comp. Zool. at Harvard College, vol. 30, p. 169–256, 10 pls.
7. A new hypothesis of seasonal dimorphism in lepidoptera. Psyche, vol. 8, p. 47–50; 59–62.
8. On an improved heliostat invented by Alfred M. Mayer. American Journal of Science, vol. 4, ser. 4, p. 306–308, 2 figs.

1898

9. *With* Alexander Agassiz: On some medusæ from Australia. Bulletin Museum Comp. Zool. at Harvard College, vol. 32, p. 15–19, 3 pls.
10. *With* Alexander Agassiz: On Dactylometra. Bulletin Museum Comp. Zool. at Harvard College, vol. 32, p. 1–11, 13 pls.

1899

11. *With* Alexander Agassiz: Acalephs from the Fiji Islands. Bulletin Museum Comp. Zool. at Harvard College, vol. 32, p. 157–189, 17 pls., 146 figs.

1900

12. On the development of color in moths and butterflies. Woods Hole lectures, tenth lecture, p. 157–164.
13. On the mating instinct in moths. Annals and Magazine of Natural History, London, ser. 7, vol. 5, p. 183–190; also in Psyche, 1900, vol. 9, p. 15–20.
14. An Atlantic Palolo, Staurocephalus gregaricus. Bulletin Museum of Comp. Zool. at Harvard College, vol. 36, p. 1–14, 3 pls.
15. Descriptions of new and little-known medusæ from the western Atlantic. Bulletin Museum Comp. Zool. at Harvard College, vol. 37, p. 1–9, 6 pls.
16. Some medusæ from the Tortugas, Florida. Bulletin Museum Comp. Zool. at Harvard College, vol. 37, p. 13–82, 44 pls.

1901

17. The variations of a newly-arisen species of medusa. Science Bulletin, Museum Brooklyn Institute of Arts and Sciences, vol. 1, p. 1–27, 2 pls.

1902

18. Effects of natural selection and race-tendency upon the color-patterns of Lepidoptera. Science Bulletin, Museum Brooklyn Institute of Arts and Sciences, vol. 1, p. 31–86, 2 pls.
19. The Atlantic Palolo. Science Bulletin, Museum Brooklyn Institute of Arts and Sciences, vol. 1, p. 93–103, 1 pl.

1903

20. Some species of Partula from Tahiti, a study in variation. Memoirs Museum Comp. Zool. at Harvard College, vol. 26, p. 117–135, 1 pl.
21. *With* Alexander Agassiz: Medusæ of the tropical Pacific. Memoirs Museum Comp. Zool. at Harvard College, vol. 26, p. 139–175, 13 pls. and 1 map.

1904

22. Medusæ of the Bahamas. Memoirs of Nat. Sci., Museum Brooklyn Institue of Arts and Sciences, vol. 1, p. 1–33, 7 pls.

1906

23. Medusæ of the Hawaiian Islands collected by the steamer *Albatross* in 1902. U. S. Fish Commission Bulletin for 1903, p. 1131–1143, 3 pls.
24. Rhythmical pulsation in Scyphomedusæ, I. Publications of the Carnegie Institution of Washington, No. 47, 62 pp., 36 figs.
25. The annual breeding swarm of the Atlantic Palolo Papers from the Tortugas Laboratory of the Carnegie Institution of Washington, vol. 1, p. 105–112, 1 pl.
26. Rhythmical pulsation in Scyphomedusæ, II. Papers from the Tortugas Laboratory of the Carnegie Institution of Washington, vol. 1, p. 113–131, 13 figs.
27. *With* Caroline G. Soule: Some reactions of caterpillars and moths. Journal of Experimental Zoology, vol. 3, p. 415–433.

1908

28. On the use of carbon dioxide in killing marine animals. Biological Bulletin, Woods Hole, vol. 16, p. 18.

1909

29. On the use of magnesium in stupefying marine animals. Biological Bulletin, vol. 17, p. 341–342.
30. The relation between ciliary and muscular movements. Proc. Soc. Experimental Biol. and Medicine, vol. 7, p. 19–20.
31. The cause of rhythmical pulsation in scyphomedusæ. Proc. Seventh International Zool. Congress, 4 pp.
32. The annual swarming of the Atlantic Palolo. Proc. Seventh International Zool. Congress, 5 pp.

1910

33. Medusæ of the world. 3 vols., 735 pp., 76 pls., 428 text figs. Published by the Carnegie Institution of Washington. Publication No. 109. Quarto.
34. Alexander Agassiz, 1835–1910. Popular Science Monthly, vol. 76, p. 419–458, portrait, 2 figs. Reprinted in Smithsonian Report for 1910, p. 447–472. Issued in 1911.

1911

35. Alpheus Hyatt, 1838–1902. Popular Science Monthly, p. 129–149, portrait, 2 figs.
36. The converse relation between ciliary and neuro-muscular movements. Papers from the Tortugas Laboratory of the Carnegie Institution of Washington, vol. 3, p. 1–25, 8 figs.

1904–1919

37. Annual Reports of the Director of the Department of Marine Biology of the Carnegie Institution of Washington. Year Books of Carnegie Institution of Washington, 1904–1919. (See also No. 69.)

1912

38. Ctenophores of the Atlantic coast of North America. 60 pp., 17 pls., 12 figs. Publication No. 162, Carnegie Institution of Washington.

1913

39. The Tortugas Laboratory of the Carnegie Institute of Washington. Internationale Revue der gesamten Hydrobiologie und Hydrographie, Bd. 5, p. 505–510, Taf. 11.

1914

40. An expedition to the coral reefs of Torres Straits. Popular Science Monthly, vol. 85, p. 209–231, Illustrated.
41. Effects of temperature upon tropical marine animals. Publication No. 183, the Carnegie Institution of Washington, p. 1–24, 12 figs.
42. The relation between the degree of concentration of electrolytes of sea water and the rate of nerve conduction in *Cassiopea*. Ibid., p. 25–54, 13 figs.
43. The law governing the loss of weight in starving *Cassiopea*. Ibid., p. 55–82, 1 pl., 21 figs.

1915

44. Ecology of the Murray Island coral reef. Proc. National Academy of Sciences, vol. 1, pp. 211–214.
45. The nature of nerve conduction in Cassiopea. Proc. National Academy of Sciences, vol. 1, pp. 270–274.
46. Medusae of the Philippines and of Torres Straits. Publication No. 212, the Carnegie Institution of Washington, pp. 157–202, 3 pl., 7 text figs. Reprinted in modified form in Proc. U. S. National Museum, 1917; as a report from the U. S. Bureau of Fisheries.

1915–16

47. A history of Tahiti, A history of Fiji, Papua where the stone age lingers. The men of the mid-Pacific, the Islands of the mid-Pacific, Java the exploited island. Popular Science Monthly, vols. 86–87, and the Scientific Monthly, vols. 1 and 2, 188 pp., figs.

1916

48. Sub-marine solution of limestone in relation to the Murray-Agassiz theory of coral atolls. Proc. National Academy of Sciences, vol. 2, pp. 28–30.
49. A theory of nerve conduction. Proc. National Academy of Sciences, vol. 2, pp. 37–42, 2 figs.
50. Nerve conduction and other reactions in *Cassiopea.* American Journal of Physiology, vol. 39, pp. 375–393, 2 figs.
51. Further studies of nerve conductiou in *Cassiopea.* Proc. National Academy of Sciences, vol. 2, pp. 721–726, 2 figs.
52. *With* Robert S. Woodward: Biography of Alfred Marshall Mayer, 1836–1897. Biographical Memoirs National Academy of Sciences, vol. 8, pp. 243–272, portrait.

1917

53. Further studies of nerve conduction in *Cassiopea.* American Journal of Physiology, vol. 42, pp. 469–475, 2 figs.
54. Nerve conduction in *Cassiopea xamachana.* Publication No. 251, Carnegie Institution of Washington, vol. 11; papers from the Department of Marine Biology, pp. 1–20, 15 figs.
55. Coral reefs of Tutuila with special reference to the Murray-Agassiz solution theory. Proc. National Academy of Sciences, vol. 3, pp. 522–526.
56. Observations upon the alkalinity of the surface water of the tropical Pacific. Ibid., vol. 3, pp. 548–552.
57. On the non-existence of nervous shell shock in fishes and marine invertebrates. Ibid., vol. 3., pp. 597–598.
58. Is death from high temperature due to the accumulation of acid in the tissues? Amer. Jour. Physiol. vol. 44, pp. 581–585.
59. Formula for the rate of nerve conduction in sea water. Ibid., vol. 44, pp. 591–595.
60. Report upon the scyphomedusae collected by the U. S. Bureau of Fisheries steamer *Albatross* in the Philippine Islands and Malay Archipelago. U. S. National Museum, Bulletin No. 100, pp. 175–233, 24 figs.

1918

61. Ecology of the Murray Island coral reef. In Publication No. 213, Carnegie Institution of Washington, 48 pp. 19 pls., 9 text figs. Volume 9 of papers from the Department of Marine Biology.
62. William Stimpson, 1832–1872. Memoirs National Academy of Sciences, vol. 8, pp. 419–433, portrait.
63. Navigation illustrated by diagrams. 207 pp. 97 figs. Lippincott, Philadelphia, Pa.
64. The growth-rate of Samoan coral reefs. Proc. National Academy of Sciences, vol. 4, pp. 390–393.
65. Toxic effects due to high temperature. Papers from the Department of Marine Biology, Carnegie Institution, vol. 12, 6 pp.
66. Nerve conduction in diluted and concentrated sea water. Ibid., 5 pp. 1 fig.

1919

67. Detecting ocean currents by observing their hydrogen-ion concentration. Proc. American Philosophical Society, vol. 58, pp. 150–160, 1 fig.

1920

68. Samuel Hubbard Scudder, 1837–1911. Memoirs National Academy of Sciences, vol. 17, pp. 79, portrait.

1920

69. The effect of diminished oxygen upon the note of nerve-conduction in Cassiopea. American Journal Physiology, vol. 51, pp. 543–549.
70. The reefs of Tutuila, Samoa, in their relation to coral-reef theories. Proc. American Philosophical Society, vol. 19, no. 3, 14 pp., 3 figs.

1922

71. Hydrogen-ion concentration and electrical conductivity of the surface water of the Atlantic and Pacific. In Publication No. 312, Carnegie Institution of Washington, pp. 61–85, 3 charts.
72. Tracking instinct in a Tortugas ant. In Publication No. 312, Carnegie Institutiou of Washington, pp. 101–107, 3 figs.

1924

73. Rose Atoll, American Samoa. In Publication No. 340, Carnegie Institution of Washington, pp. 73–79, 2 pls.

74. Structure and ecology of the Samoan reefs. In Publication No. 340, Carnegie Institution of Washington, pp. 1–25, 6 pls., 3 figs.
75. Inability of stream water to dissolve submarine limestones. In Publication No. 340, Carnegie Institution of Washington, pp. 37–49, 1 fig.
76. Growth rate of Samoan corals. In Publication No. 340, Carnegie Institution of Washington, pp. 51–72, 26 pls.
77. Causes which produce stable conditions in the depth of floors of Pacific fringing reef flats. In Publication No. 340, Carnegie Institution of Washington, pp. 27–36, 2 pls., 1 fig.

---

78. Reports of the director of the department of Marine Biology, Carnegie Institution of Washington in the Year Book of the Institution, as follows: III for 1904, pp. 50–54, pls. 4, 5; IV for 1905, pp. 108–124, pls. 5, 6; V for 1906, pp. 106–118, pl. 7; VI for 1907, pp. 106–123, pl. 1; VII for 1908, pp. 118–138, pl. 5; VIII for 1909, pp. 125–153, pl. 7; IX for 1910, pp. 117–148, pl. 1; X for 1911, pp. 120–156, pls. 3–6; XI for 1912, pp. 118–164, pls. 2, 3; XII for 1913, pp. 163–184; XIII for 1914, pp. 169–233; XIV for 1915, pp. 183–241; XV for 1916, pp. 171–215; XVI for 1917, pp. 161–189; XVII for 1918, pp. 149–172; XVIII for 1919, pp. 185–210; XIX for 1920, pp. 185–200; XX for 1921 pp. 191–205.

DISCUSSIONS, POPULAR ARTICLES, ETC.

1903

Educational efficiency of our museums. North American Review, vol. 177, pp. 564–569.
The status of public museums in the United States. Science, vol. 27, pp. 843–851.
The Tortugas Florida, as a station for research in biology. Science, vol. 17, pp. 190–192.
A tropical marine laboratory for research. Science, vol. 17, pp. 655–660.
A laboratory for the study of marine zoology in the tropical Atlantic. Popular Science Monthly, pp. 41–47, 6 figs.
The Bahamas versus Tortugas as a station for research in marine Zoology. Science, vol. 18, pp. 369–371.

1904

Material versus intellectual development of our universities. Science, vol. 20, pp. 44–47.

1906

Our universities and summer schools. Science, vol. 23, p. 703.

1908

A plan for increasing the efficiency of marine expeditions. Science, vol. 27, pp. 667–671.
Marine laboratories and our Atlantic coast. American Naturalist, vol. 42, pp. 533–536.

1909

Autonomy for the university. Science, vol. 30, pp. 673–675.

1910

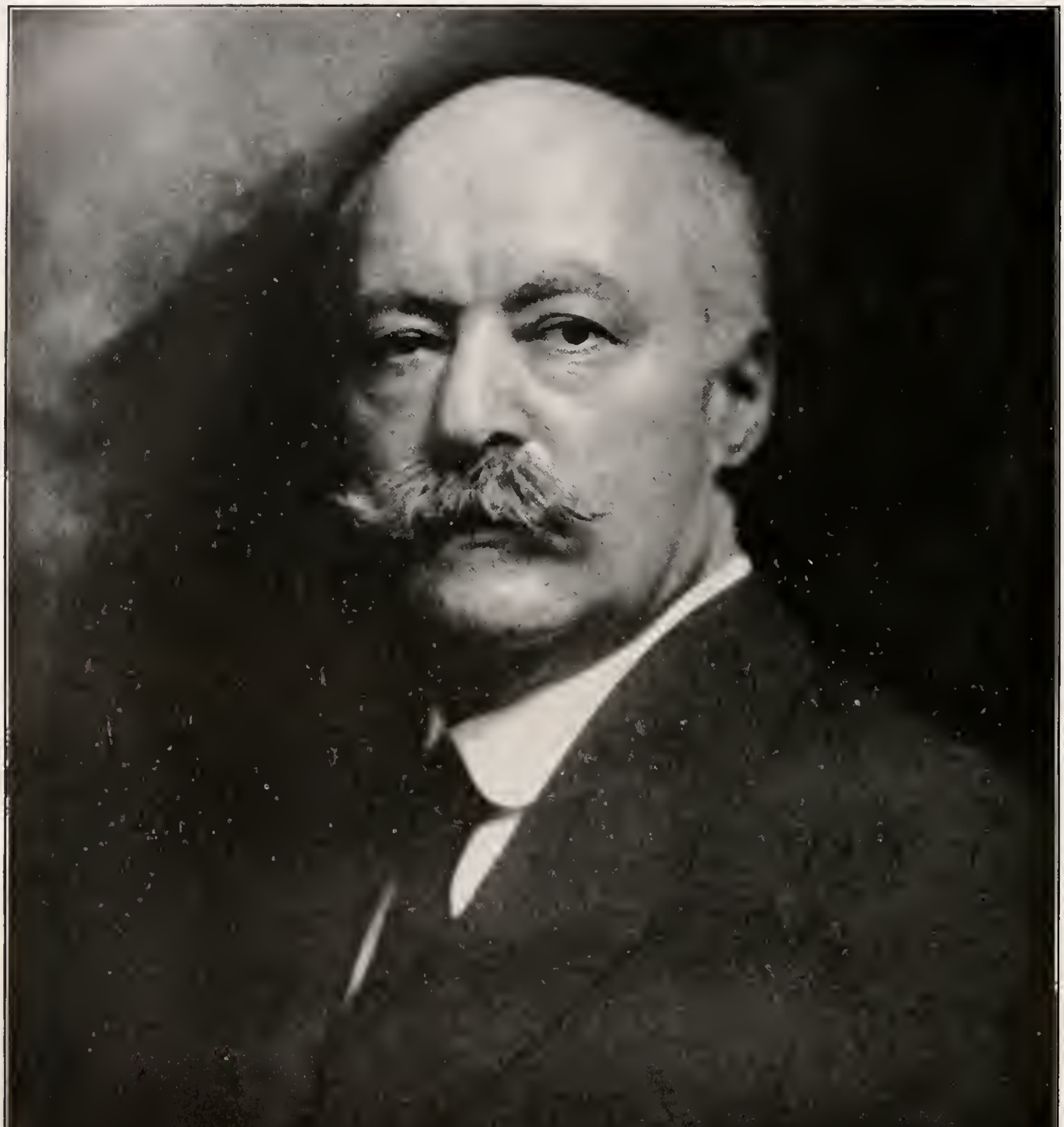
Our universities and research. Science, vol. 32, pp. 257–260.
The research work of the Tortugas laboratory. Popular Science Monthly, pp. 396–411, 12 figs.

1915

Address of the retiring vice president of section F of the American Association for the Advancement of Science. Science, vol. 41, pp. 81–82.

1916

The Gulf Stream off our coast. Bulletin American Museum of Natural History, New York, vol. 16, pp. 501–506. 4 illus.



S. J. Meltzer

NATIONAL ACADEMY OF SCIENCES

Volume XXI
NINTH MEMOIR

BIOGRAPHICAL MEMOIR SAMUEL JAMES MELTZER

1851-1920

BY

WILLIAM H. HOWELL

Presented to the Academy at the Annual Meeting, 1923

SAMUEL JAMES MELTZER

By William H. Howell

Samuel James Meltzer was born of Jewish parents on March 22, 1851, at Ponewjesh, in Curland, northwestern Russia. His parents were poor but belonged to the intellectual class. His father was a teacher, with an intense devotion to his religious faith. He undertook the early education of his son, but, as might be expected, the training was limited almost entirely to a study of Hebrew theological literature. The boy displayed early an eager desire for learning of all kinds, against his father's wishes and commands, with the result that the two came into conflict, the father attempting to limit his son's interests solely to those studies which would prepare him for the career of a rabbi, while the boy borrowed books from neighbors and friends which he was compelled to read in secret under the fear of punishment if discovered. There was constant friction between them and frequent occasions for the exercise of rigid discipline. After one severe punishment following upon this kind of disobedience the boy declared his freedom by leaving home and walking many miles to the house of an aunt in a distant village. Owing to the intercessions of his mother, who sympathized warmly with her son's love of learnng, he was permitted to remain with his aunt for a while and subsequently was sent to a neighboring town where he lived in the temple, was taught by the rabbis, and got his meals from various families in the village. He was a scholar of such ability as to attract the general attention of the community, and it is recorded in the family traditions that at the early age of 16 he had learned all that the rabbis could impart to him. The learned men of the community came to him for help in the interpretation of difficult passages in the Talmud.

At the age of 20, according to the custom of his people, a marriage was arranged for him with Olga T. Levitt, the daughter of a well-to-do merchant. The bride was only 16 years old. She was his devoted wife until the day of his death, during the periods of storm and stress when he was struggling hard to get his start in life, and during that longer period when he had attained to eminence and enjoyed the comforts and luxuries of life. After his marriage young Meltzer announced his fixed determination not to enter the ministry, a decision that caused both anger and sorrow to his father. He betook himself to Konigsberg and with the help of his wife's dowry essayed to become a business man. The business that he selected was the manufacture of soaps. But at the same time he seized the opportunity to enroll in the Real Gymnasium. The result was what might have been expected. The work of the gymnasium interested him, he gave more time and thought to his books than he did to his business, and the latter therefore was not a success. At the end of the year he recognized that business was not his calling. His greatest interest at that time and for some years was in philosophy, but he realized that along that path it might be difficult to earn a living for his family, so he decided to begin the study of medicine. His wife and children returned to her father's house, while he proceeded to Berlin and entered the university in the fall of 1876. He was a student of medicine for five years. The tentamen physicum was absolved in the spring of 1879 and the examen rigorosum in June, 1881. They were very lean years for him from a financial point of view. He was in fact desperately poor. He lived with a humble family in an attic room, spending as little as possible of his meager income on food and raiment, and as much as he could spare in payment for his medical courses at the university.

His love of philosophy was still strong within him, and one of the stories he told of this period was that he surreptitiously attended Professor Steinthal's lectures in this subject, sitting far back in the room for fear that he would be discovered as an interloper and turned out. But on one occasion, during a heated discussion of the meaning of a certain passage in Kant's Critique, his interest got the better of his fears and he suggested that if a certain comma was transposed the meaning would be made clear. This suggestion aroused the interest of the professor. He asked the young man to remain after the lecture, and as a result of the conference that ensued

Meltzer was invited to visit him at his home. This was the more or less accidental beginning of a beautiful and lasting friendship between the two men. However hard the physical conditions of life may have been during this period, there is no doubt that intellectually and spiritually they were gloriously enjoyed and utilized. He threw himself into his work with intense zeal, and taking advantage of his opportunities gave as much serious attention as time would permit to the cultural studies of art and music. In the medical school he sat under great masters—du Bois Reymond, Virchow, Helmholtz, Friedländer, and the like—but the teacher who exercised the greatest influcnce upon his life and came closest to him was Hugo Kronecker. Like Steinthal in philosophy, Kronecker was impressed by the ability and industry of the young student in physiology. He became interested in him personally, invited him to his home, and finally became his warm personal friend as well as his guide and councilor in physiology. It was under Kronecker that he completed his inaugural dissertation for the degree of doctor of medicine. The dissertation was dedicated to Kronecker "in herzlicher Dankbarkeit," and in collaboration with this master and friend he completed some notable investigations, which will be referred to later on. The fact that this poverty-stricken young student enjoyed the personal friendship of two eminent professors in the university and was received into the intimacies of their family life throws an interesting light upon the conditions of university life at that time.

A trivial incident of this phase of his life, which Doctor Meltzer sometimes referred to, indicates quite clearly that the relationship between student and professor was not that of a poor dependent and well-to-do patron, but an intellectual companionship of two scholarly men not influenced by the mere externalities of life. Kronecker, knowing that Meltzer was so poor that he got insufficient food, once in the kindness of his heart sent him a ham, but it was refused with indignation, just as Dr. Sam Johnson, as related by Boswell, spurned the mistaken kindness of a friend in sending him a pair of shoes to replace his shabby footwear when a poor student at Oxford. After graduating in medicine Meltzer could have made his career as a scientific man in Germany. It is stated that he was offered several positions on the condition that he be baptized in the Christian faith. But such a step did not accord with his sturdy sincere character. His thoughts turned to America as the country that had the best form of government and promised the most freedom in speech and action. He did not have sufficient means to purchase his passage; he therefore shipped as surgeon on one of the trans-Atlantic liners and thus arrived in New York. He was provided with letters of introduction to leading men in scientific, medical, and musical circles, but it is not recorded that he obtained any material assistance through this agency. He applied himself to the practice of medicine in order to support himself and to obtain sufficient means to bring over his family. His efforts must have been unusually successful, since in the second year of his residence in New York, in 1885, he felt justified in sending for his family. He became subsequently a successful practitioner, owing no doubt mainly to his intrinsic ability as a physician, but partly also, in all probability, to the fact that he at once began to make himself known to members of the profession through his scientific publications.

Just as soon as his financial position made it at all possible he sought the opportunity to use the facilities then existing in New York to carry on investigative work. He found and utilized such opportunities in Welch's laboratory at Bellevue and in Curtis's and Prudden's laboratories at the College of Physicians and Surgeons. For many years his practice and his investigations were carried on simultaneously. He worked with that extraordinary intensity and devotion which characterized him throughout his life. All his spare time during the day and a large part of the night were given up to laboratory investigations or the reading of current scientific journals. The eagerness for knowledge that he had shown in his early youth and manhood remained with him throughout life. It was a passion with him that seemed to grow stronger as he grew older, so that almost from the beginning of his residence in this country there came from his pen an increasing stream of communications to medical and scientific journals that have served to establish his reputation as a scientific investigator of high rank. His productivity was remarkable. The list of his published papers includes over 240 titles, distributed among some 48 scientific journals of this country, Germany, and England. These

papers contain contributions to the subjects of physiology, pharmacology, pathology, and clinical medicine, in addition to a number of lectures and general addresses. It is a striking demonstration of the breadth of his interests and knowledge that he was recognized as a competent and indeed leading investigator in all of these subjects. Physiology was the subject in which he was most deeply interested and in which he had the most intensive training as an investigator, and his contributions to the other medical sciences were made mostly from the point of view of the physiologist. His practical knowledge of clinical medicine and his extensive and thorough acquaintance with its literature gave him the opportunity to realize how often the results of physiology, as a basal medical science, may be applied with profit to throw light upon the problems of all other branches of the subject.

The professional physiologist is usually out of touch with the problems of the practitioner, and hence he fails often to realize how useful the new results in his science may be to his coworkers on the practical side. Once in a while we have active practitioners who at the same time are competent investigators in one or other of the underlying medical sciences. Such men serve as liaison officers capable of bringing about rapidly a mutually beneficial exchange of knowledge, which otherwise would develop only slowly through many intermediate agencies. Weir Mitchell and Meltzer are perhaps the two most conspicuous examples in this country of this advantageous combination in one individual of practical and scientific knowledge in medicine. In Weir Mitchell's generation it was perhaps less difficult to play this dual rôle, since the medical sciences had not become such highly developed specialties. In Meltzer's generation the task was more difficult, and he was able to carry it through with success only because of his unremitting devotion, his remarkable capacity for work, and his excellent training in the theoretical and practical sides of medicine. While he made important contributions to the fundamental problems of physiology, it may be said with truth that his special field of work lay in the borderland between medical practice and medical science. He was in sympathetic touch with the workers in both fields, he published papers in the theoretical and the practical journals, and he enjoyed the acquaintance and confidence of the leaders on both sides. No one perhaps in the generation now passing was more influential in bringing about a sympathetic understanding and coordination between the laboratory worker and the practitioner. To the latter he preached without ceasing the importance and necessity of scientific investigation, and the former he kept continually reminding of the possible application of scientific results to the explanation or cure of disease. As a consequence of this somewhat unique position which he held in the medical profession of this country we find that he was an active member of many of the important medical societies, both clinical and scientific, and indeed he was immediately concerned in the formation of some, particularly those whose chief function was to bring the results of scientific investigation to bear upon the problems of the clinician. In further confirmation of this view, that he served in a special way to link up the practical and theoretical workers in medicine, attention may be called to the fact that at various times he served as president of the American Physiological Society, the Society for Experimental Biology and Medicine, the American Gastro-enterological Society, the American Society for the Advancement of Clinical Research, the Association of American Physicians, and the American Association for Thoracic Surgery. In this list are represented some of the societies that stand mainly for research, together with associations that are composed mainly of active clinicians. That he was elected to the presidency by the votes of his fellow members is a clear enough demonstration that he stood high in the esteem of both groups of workers in medicine. It is not probable that he will have a successor in this unique position. While his activities covered this large range, his training primarily was that of a physiologist. It was from this standpoint that he worked and investigated, and his contributions to this subject entitle him to be ranked among the foremost physiologists of his generation. It seems appropriate, therefore, to attempt to give a critical estimate of the major contributions at least that he made to physiology. His papers in this field are so numerous and cover such a variety of topics that it is not possible to bring them all under review.

His first appearance as an investigator is recorded in a brief note in the "Verhandlungen der Berliner physiologischen Gesellschaft," printed in the Archiv für Physiologie, 1880, page 299. The communication to the society made by Kronecker in behalf of Herr Cand. Med. Meltzer, was entitled, "Die Bedeutung des M. Mylohyoideus für den ersten Act der Schluckbewegung." Kronecker had become interested in the mechanism of the act of swallowing, and under his direction two of his students, Falk and Meltzer, were carrying on investigations which demonstrated that the older view, namely, that the bolus is carried down by a progressive peristaltic wave of the pharyngeal and œsophageal musculature, is not correct. In the first act of swallowing the bolus is shot into the œsophagus by a contraction of cross-striated muscles, and Meltzer's first paper gave a probable proof that the chief muscle concerned is the mylohyoid. At a later meeting of the society in the same year Kronecker gave a second fuller paper by Meltzer and himself, "Ueber die Vorgänge beim Schlucken," Archiv für Physiologie, 1880, page 446, in which the outlines of their theory of swallowing were presented. This work, which was continued through several years, was published in full first, in the Monatsberichte der Königl. Akademie der Wissenschaften zu Berlin, January 24, 1881, under the title "Ueber den Schluckmechanismus und dessen nervöse Hemmungen," and later in more complete form in the Archiv für Physiologie, supplementary volume, 1883, page 328, with the title "Der Schluckmechanismus, seine Erregung and seine Hemmung." In its essential features their theory of swallowing has been generally accepted in physiology. As stated above, the important new discovery that they made was that the act of swallowing is initiated by a sharp contraction of the mylohyoid and hyoglossi muscles. The effect of these contractions is to shoot the bolus of food, which lies upon the dorsum of the tongue, through the pharynx and for a certain distance, depending on the consistency of the material, into the œsophagus. The contraction of the constrictors of the pharynx and the peristaltic wave along the œsophagus follow later, and in the case of soft foods, at least, constitute a sort of movement in reserve. The whole act of swallowing was studied with great care and many interesting observations were made, partly from experiments upon animals and partly from experiments which Meltzer made upon himself. In later life he developed a very sensitive larynx, which he attributed to the irritations resulting from the long-continued observations made upon his own deglutition. Some of these experiments must have been exceedingly uncomfortable, as they involved the placing of rubber bags in the pharynx and at different levels in the œsophagus to obtain graphic records, by means of connected tambours, of the passage of the swallowed bolus. Those who knew Doctor Meltzer well will realize that the intense earnestness which he threw into all of his work might easily have lead him, in these experiments, to overstep the bounds of prudence, if thereby he could obtain evidence for what he believed to be a true explanation of the process of swallowing.

In connection with this work upon the mechanism of swallowing Meltzer under took, presumably upon Kronecker's advice, the study of a special phase of the action of the deglutition center as the subject for his doctor's thesis. The results were published in his inaugural dissertation for the doctorate in medicine and surgery under the title "Das Schluckcentrum, seine Irradiationen und die allgemeine Bedeutung derselben." This is dated August 12, 1882, and is dedicated to Kronecker "in herzlicher Dankbarkeit." It was published later, under a slightly different title, in the Archiv für Physiologie, 1883, page 209. It is a suggestive and interesting paper on account of the careful analysis which it contains of the far-reaching effects of the act of swallowing upon other centers of the nervous system. He shows that the swallowing reflex influences, by irradiation or overflow, the centers for respiration, the heart, the bloodvessels, the uterus, the erection of the penis, etc., thus making it probable that the act of irradiation, so evident in the case of this relatively unimportant and infrequent movement, is a general accompaniment of the activity of the centers of the nervous system. But the main significance of this paper is to be found in the fact that it suggested to him certain views regarding the importance of inhibition in the reactions of the central nervous system or indeed of living matter in general. Thus early in his career he acquired a point of view in regard to the rôle of inhibition in the life processes which greatly influenced his later work. The idea or theory is outlined in this first paper, but is developed in greater detail in subse-

quent work. In the dissertation he calls attention to the fact that stimulation of certain afferent nerves, such as the superior laryngeal, the splanchnic, and the second branch of the trigeminal causes a contraction of the expiratory muscles and simultaneously an inhibition of the inspiratory muscles, while stimulation of the vagus nerve, on the other hand, causes a reflex contraction of the diaphragm and inspiratory muscle, together with an inhibition of the external oblique muscle of the abdomen, an expiratory muscle. For the muscles of respiration, therefore, the arrangement is such that reflex stimulation of the inspiratory muscles is accompanied by reflex inhibition of expiratory muscles and vice versa. He goes on to assume that a similar reasonable arrangement must exist in the case of antagonistic muscles in other parts of the body, for example, with the flexors and extensors of the limbs, if they are to act most efficiently in locomotion. Some ten years later Sherrington gave the necessary demonstration that this interrelation does hold with the muscular antagonists of the limbs; the contraction of an extensor is accompanied by a relaxation of its opposing flexor and vice versa. He designated this relationship under the term of "reciprocal innervation," without apparently being aware of Meltzer's similar views.

Meltzer meanwhile had been accumulating further data bearing upon his generalization. In his paper upon "The self-regulation of respiration," read before the American Physiological Society in 1889 and published in the New York Medical Journal and under a different title in the Archiv für Physiologie, he describes experiments intended to show that two kinds of afferent fibers exist in the vagus nerve, one exciting and the other inhibiting inspiratory movements. He used this view to modify the Hering-Breuer theory of the self-regulation of the respirations by assuming that the expansion of the lungs stimulates simultaneously both sets of fibers. The resultant effect, as in the case of the simultaneous stimulation of the motor and inhibitory fibers of the heart, is a dominance of the inhibitory action, thus arresting the inspiration and bringing on a passive expiration, but subsequently the excitatory effect, which, like that due to the accelerator fibers, has a long after action, comes into play and starts a new inspiration. In his first general paper on inhibition (New York Medical Journal, 1899) this idea of a combined action of opposing processes is extended by the citation of numerous instances taken from physiological literature and is expanded into a general theory which makes inhibition a universal property of living matter. "I entertain and defend the view that the phenomena of life are not simply the outcome of the single factor of excitation, but they are the result of a compromise between two antagonistic factors, the fundamental forces of life, excitation and inhibition." That is to say, whenever a tissue is stimulated two different processes are aroused, one leading to the functional activity and one to the suppression of this activity. As to the nature of these processes he does not speculate to any extent. He was not satisfied with the Hering-Gaskell conception, according to which excitation follows or is an expression of catabolic changes while inhibition is the accompaniment of processes of an anabolic or assimilative nature. He goes only so far as to assume that the two processes involve the potential and kinetic energies possessed by the system, and that while excitation facilitates the conversion of potential to kinetic energy, inhibition hinders or retards this conversion, the two processes being compared to the turning on and off of a stopcock. Nor was he satisfied with Sherrington's term of reciprocal innervation to describe the general relations he had in mind. Unfortunately, perhaps, he had not proposed any concrete designation in the beginning to express the interplay of these opposing processes, but later, stimulated no doubt by Sherrington's example, he did attempt to find a suitable descriptive phrase.

In his second general paper on inhibition (Medical Record, 1902, page 881), he suggests the term "law of crossed innervation," an expression used by von Basch and Ehrmann to describe the opposed activity of the longitudinal and crossed musculature of the intestines. Later still, in a paper in the Archiv für Verdauungskrankheiten, 1903, Volume IX, page 450, he modifies this to the "law of contrary innervation." But he did not succeed in establishing either of these variations upon Sherrington's nomenclature in current physiological literature. He no doubt felt, and very justly so, that he should have had some of the credit for establishing this general physiological law. His own plea for priority in the matter is given in a footnote to a paper

written in collaboration with his daughter, Mrs. Auer, "On the paradoxical pupil-dilation caused by adrenalin" (American Journal of Physiology, 1904, Vol. XI, page 40). He believed that this process of "contrary innervation" is practically universal in its action; it is "manifest in all the functions of the animal body." As was his custom with all of his theoretical conclusions, he attempted to apply this law to the explanation of some of the pathological phenomena that were presented to him in his practical work as a physician. As he expresses it, "a disturbance of this law is a factor of more or less importance in the pathogenesis of many disorders and diseases of the animal body," and he illustrates his meaning by a specific application to gastric and intestinal colic. If we could imagine that the orderly sequence of a peristaltic wave is disturbed so that the advancing wave of contraction meets a constricted instead of an inhibited area, then evidently conditions are present which may cause distension and give rise to the pains of colic. Other instances of a similar character, cardio spasms, biliary obstructions, etc., are cited to show that known pathological results may follow from a disturbance of or a disharmony in the normal process of contrary innervation. How far Doctor Meltzer was correct in these concrete applications of his theory it is not possible to say. Some of the cases he uses are undoubtedly amenable to other explanations; but in his general conception of the importance of the inhibitory processes in the functional activity of the organs he was in advance of most of his contemporaries, and he deserves credit for keeping this more or less hidden side of vital activity in the forefront of physiological discussions. The whole story is far from being told, and it may be that later work will demonstrate that he saw deeper into the processes of life than his fellow workers have done. Certainly so far as the effect upon himself was concerned it was a rewarding and stimulating theory. It played, as he expressed it, a dominating part in all of his researches. Thus his explanation of the mysterious condition of surgical shock, to which he held tenaciously in the face of all opposing criticisms, was that "the various injuries which are capable of bringing on shock do so by favoring the development of the inhibitory side of all the functions of the body," or to express it in a figurative way, the normal balance of the opposing processes is shifted toward the side of inhibition.

In all of his experimental work Meltzer was very exact and objective in describing his results. In fact he entered into the minute details of his observations with a thoroughness that was probably derived from his German training. On the other hand, it is evident from what has been said of his views upon inhibition that he had a marked tendency to theorize or speculate upon the basis of his findings. He was not content to simply catalogue his observations and publish them as isolated facts which somehow would find their place in the growing structure of science. They were for him the subject of much reflection. He regarded them as revelations of the processes of the body which he must try to interpret, and his genuinely scientific mind was constantly seeking to formulate general theories to fit or to embrace the facts that were disclosed by his experiments. Nothing seemed to give him greater pleasure than to discuss these theoretical possibilities with his fellow workers, and from many conversations of this kind which it was my good fortune to enjoy I got always the impression of a mind constantly on the alert to understand and interpret his results and full of a certain eager expectancy of discoveries of importance. His theories and generalizations in turn supplied him with the interest and energy to devise new work, so that throughout his long career there was never any lack of problems to be attacked nor any diminution in enthusiasm for research. Young scientific workers are sometimes warned by their older associates against the dangers of speculation; and while this advice may be wise as regards the written word, there can be little doubt that an original and constructive mind must react in this way. Reflection and speculation are essential in keeping alive a spirit of investigation. When the proper time came Meltzer did not hesitate to announce his theoretical views in published papers or addresses. Some of them were effective in guiding and stimulating other workers beside himself, and some were so bold and far-reaching that only the future can determine whether they will be fruitful or barren. The work, for instance, which he began with Welch in 1884 upon the effect of mechanical vibration upon bacteria, red corpuscles, and other cells, and to which he returned from time to time in his laboratory investigations, led him to believe and announce that a certain rate

of vibration is one of the normal and essential conditions of life—like temperature, for example. Furthermore, he conceived that the rate of the heart beat is adjusted, on the one hand, to furnish the optimum vibratory condition for the cells of the organism, and, on the other, to give a mechanical vibration which is destructive toward foreign cells, such as invading bacteria. He considered that this constitutes one of the defensive mechanisms of the body, an idea which I fancy none of his fellow workers has adopted.

In one of the last, if not the very last paper that he wrote, a paper published only two months before his death, he announced a most interesting discovery, namely, that removal of both superior cervical ganglia in rabbits is followed by death in about 90 per cent of the cases. His experiments, as usual, are described with care and exactness, but he does not hesitate to speculate with some boldness upon the possible explanations of this remarkable result, and it is to be noted that the two chief hypotheses that he advanced are of a kind that probably do not at once commend themselves to his fellow physiologists. He suggests, in the first place, that these ganglia "contain a principle which is essential for the maintenance of life"—that is, they produce an essential internal secretion. In the second place, to account for the pulmonary lesions exhibited by the operated animals, he assumes that the ganglia normally send impulses to the respiratory centers through which the orderly play of the antagonistic (abductor and adductor) muscles of the larynx are controlled. In the absence of this controlling influence there is a disharmonious action of the muscles, a disorder of the law of contrary innervation, which, as in the case of section of both vagi, leads in some way, not clearly understood, to an infection of the lungs. These and like examples indicate clearly how experimental results tended to stimulate his imagination, as they must do with every worker whose soul is in his work. He held hard to his facts, and insisted that other workers should do the same; but on the basis of these facts his intense and eager mind sought to discover the larger principles that control the life of the organism. Some of his theoretical speculations were more fortunate in finding a prompt acceptance and application among his fellow workers. Thus, in his Harvey lecture, 1906, on "The factors of safety in animal structure and animal economy," he made a most happy application of this term, used in engineering, to describe the reserve powers exhibited by the mechanisms of the body. While the general conception that he developed had no doubt occurred to others, no one before him, so far as I know, had clearly visualized the great importance of these reserves in the adaptation of the organism to the changing conditions of its environment. The apt phrase that he selected to designate this property found an immediate acceptance in scientific medical circles the world over. One meets the expression now constantly in current literature, accompanied usually by a grateful acknowledgment to the author who first suggested its use. The mere term itself, "factor of safety," has proved most useful as a convenient and suggestive form of expression, but much more valuable than this is the emphasis it has given to a great general biological law expressing the way in which an organism is adapted to meet environmental stresses.

In his paper on "Bronchial asthma as a phenomenon of anaphylaxis" (Journal of American Medical Association, September 17, 1910) Meltzer made a theoretical suggestion as to the nature or cause of true bronchial asthma which not only attracted wide attention in medical circles, but has proved to be a real assistance in the understanding and treatment of a troublesome disease.

The most important of his contributions in later years are contained in three series of researches: One dealing with the action of adrenalin upon the blood vessels and the muscles of the iris; one with the inhibitory action of magnesium sulphate and the antagonistic effects of calcium salts; and one with his method of artificial respiration by pharyngeal and intratracheal insufflation. The first series consists of eight or nine papers, mostly in collaboration with his daughter. They showed in this work that the temporary action of adrenalin upon the blood vessels may be converted into a long-lasting effect, in the case of the ear vessels, if these vessels are first denervated by section of the vaso-motor fibers in the sympathetic and the third cervical nerve. A more striking result still was obtained for the iris. In the mammal subcutaneous injections of adrenalin in moderate doses have no effect upon the size of the pupil, but if the

superior cervical ganglion is first excised, then, after a certain interval, subcutaneous injections bring on a marked and long-lasting dilatation. His explanation of these phenomena was made in terms of his theory of inhibition. Whether or not his views in regard to the relations of the cervical ganglion to pupillary dilatation will stand the test of future experimental work, it is to be noted that the observation itself constitutes a significant instance of a kind of independent physiological activity on the part of a peripheral ganglion. The bearing of these facts upon the prevalent conception of the rapid destruction of epinephrin in the tissues was brought out especially in a paper with Auer, in which it was shown that if adrenalin is injected into a ligated limb and an hour or so afterward the ligature is removed the dilatation of the pupil quickly follows, thus demonstrating that for this long period the adrenalin had remained unaffected by the tissues. It is interesting to note that he made a second confirmatory contribution to this phase of the adrenalin effect in the last year of his life, in work done again with Auer and not published until after his death. Two incidental results that came out of this series of experiments have proved to be of value in physiological work. One was the discovery that the isolated eye of the frog shows dilatation of the pupil when exposed to small concentrations of adrenalin. A convenient biological reagent was thus furnished for the detection of minute amounts of adrenalin in the body liquids. The other was the fact that absorption takes place much more rapidly in intramuscular than in subcutaneous injections. The marked physiological effects of adrenalin furnished a positive indication of the rapidity of absorption, and the discovery that injections made intramuscularly are absorbed with great promptness has since been utilized to advantage by other workers.

The work upon the inhibitory and anesthetic effects of magnesium salts gave rise to no less than 25 papers, most of them published in collaboration with one or another of his associates, but chiefly with Doctor Auer. The peculiar inhibitory action of magnesium sulphate had attracted his attention as far back as 1899, and he reported upon it incidentally in a communication to the American Physiological Society. But in 1904–5, influenced again by his general conception of the importance of the inhibitory processes, he took up with Auer a careful physiological study of its action. The results were most interesting and important. When given subcutaneously in certain doses the magnesium sulphate produces a condition of complete unconsciousness and muscular paralysis or relaxation, which is reversible, in the sense that when the animal is given proper care it recovers. Later he was able to show that out of this condition of profound depression or inhibition the animal may be restored to complete consciousness and motility with miraculous suddenness by the intravascular injection of small amounts of calcium chloride. No one who was fortunate enough to see this demonstration as given by Doctor Meltzer will forget its dramatic effect upon his audience. A healthy vigorous rabbit was brought quickly to a condition of complete immobility and apparent death by the magnesium sulphate and then even more suddenly raised from the dead and restored to its normal tranquil existence by the injection of some calcium chloride. Meltzer and his collaborators investigated various phases of this action of magnesium sulphate, and all of the results obtained tended to strengthen in his mind the conviction that in magnesium he had discovered the element in the body that is especially concerned in the processes of inhibition. The antagonistic action of the calcium, although exhibited in such a striking way, was not in his opinion specific. His own experiments in connection with the results reported by other observers led him to the general view that calcium serves to balance the abnormal activity of the other kations, potassium, sodium, and magnesium, whether this abnormal action is in the direction of excitation or of inhibition. Modern work upon the physiological significance of the inorganic constituents of the body fluids which was begun in Ludwig's laboratory, but was given its main impetus by the striking contributions of Ringer, had concerned itself chiefly with the salts of potassium, sodium, and calcium, which alone seemed to be sufficient to maintain normal conditions of irritability. Meltzer's work has shown that magnesium also has its place in this ancient balance of powers through which the functional activity of protoplasm is controlled. One can understand that in arriving at thees results he must have felt that he was approximating at least a demonstration of the correctness of his general conception of the role of inhibition in functional activity. In this,

as in all of his experimental work, Meltzer was eager to give his results a practical application to the art of medicine. The possibilities of the use of magnesium salts as an anesthetic agent in surgical operations were tested with some success on human beings and more important still, their efficacy in controlling the spasms of tetanus has been attempted and gives promise of being useful under certain conditions. One outcome of this work which he did not foresee, but which has been exceedingly useful to the diagnostician, is its application to the procedure of obtaining specimens of bladder bile for bacteriological examination. By the local application of solutions of magnesium sulphate to Vater's papilla in the duodenum the sphincter of the bile duct is inhibited so that bile is emptied into the intestine and can be aspirated off for examination.

His last extensive series of researches dealt with anesthetization and artificial respiration through pharyngeal and intratracheal insufflation. Something like 28 papers, most of them in collaboration with pupils or assistants, were devoted to this subject. His interest in this topic seems to have been stimulated by the fact that in his use of magnesium sulphate for anesthetic purposes the chief danger lay in the inhibition of the activity of the respiratory center. To meet this difficulty he undertook a study of the methods of artificial respiration. The initial paper in 1909 by Meltzer and Auer described a method of artificial respiration by continuous insufflation of the lungs through a tracheal catheter. It was found that by this means not only could an animal be kept alive without the action of the respiratory movements to fill and empty the lungs, but that it furnished also a convenient and efficient method for anesthetization. The use of this method in animal experimentation, and especially its use in human surgery of the thorax and facial region, was apparent, and on many occasions Meltzer sought to make known its advantages and to ask for an adequate trial of its merits at the hands of practical surgeons. The method has found some acceptance, and the application of the principle involved will no doubt be extended in the future as the technique of thoracic surgery improves. It was in recognition of the importance of this work that the American Association for Thoracic Surgery asked him, a physician and laboratory worker, to serve as their first president. It was natural that this work should have led him to consider the whole matter of artificial respiration in its relations to resuscitation after accidents of various sorts. His general paper in the Medical Record for 1917, giving a history and critical analysis of the methods of resuscitation, is an interesting and valuable contribution. He gives experimental data to prove that his device of intratracheal insufflation is the most efficient method of artificial respiration both for man and animals. But he realized that it is a method which requires special knowledge and training for its successful execution, and his broadening acquaintance with and interest in the practical aspects of resuscitation led him to experiment with the less efficient and less safe method of pharyngeal insufflation. He was a member of the three national commissions on resuscitation and served as chairman of the third commission. In connection with the duties of this service he devised a simple portable form of apparatus for pharyngeal insufflation which can be used with very little previous instruction, and he demonstrated, with entire success I believe, that this form of apparatus is much more efficient than any of the so-called manual methods of resuscitation, or than any of the special machines for this purpose, pulmotors and lung motors, which have been exploited commercially during the past few years. It was, I imagine, a sore disappointment to him that he was not able to convince his colleagues on the third commission that this apparatus met all the requirements for industrial and military use. It is probably the simplest and best instrument yet devised for artificial respiration as applied to man, and in institutions or industrial establishments where the need for artificial respiration may arise frequently and where special individuals may be instructed in its use it can be employed to great advantage. But it does require some little amount of training to use it properly. The average uninstructed man or woman can not be trusted to apply it intelligently, and for this reason the commission felt constrained to recommend the adoption of a manual method as the form of first aid which may be used most successfully under ordinary conditions.

It will be evident even from this incomplete review that Doctor Meltzer's work constituted an important contribution to physiological science, and, as has been stated above, his contact with the practice of medicine and his frequent use of medical journals for his publications

enabled him to influence directly the thought and tendencies in medical circles. That he was a strong factor in the development of American medicine is recognized freely and gratefully by his contemporaries. It happens sometimes that men may pose as authorities in medical science among practitioners although they have little honor among the workers in science, or vice versa, but it was Doctor Meltzer's deserved good fortune to be most highly respected and honored by both the practitioners and the laboratory workers. While this reputation rested primarily upon the character of his investigative work, his personal influence was augmented greatly by his constant active participation in the meetings of the numerous societies to which he belonged. He believed strongly in the benefits to be derived from personal contact among workers, and he was especially interested in getting together the men who were doing scientific work in clinical medicine. He was the founder and first president of the Society for Experimental Biology and Medicine, and at the memorial meeting held by this society shortly after his death the speakers all emphasized the great importance of his personality in developing this important center of scientific activity. It was at his invitation that in 1903 the workers in the biological sciences who were resident in New York met to form an organization whose express purpose was to stimulate experimental work in biology and medicine. He gave to this society such devoted personal service and identified himself so completely with its activities that for a long period it was commonly designated as the "Meltzer Verein." In like manner he was the founder and first president, 1909, of the "Society of Clinical Investigation," the main purpose of which was to bring together the men, mostly young men, who were engaged in investigations bearing upon internal medicine. His fellow members give unanimous testimony to the influence that he exerted upon this group of men, and to the inspiration that they derived from the fine and high ideals that he impressed upon the society through his addresses and his example. He served also as president of the Association of American Physicians, the American Association for Thoracic Surgery, the American Gastroenterological Society, the Federation of American Biological Societies, and the American Physiological Society, and was in addition a faithful and active member in many of the other national societies, such as the Society of Biological Chemists, the Society of Pharmacology and Experimental Therapeutics, the Society for Experimental Pathology, the American Philosophical Society, the National Academy of Sciences, etc.

In most of these societies his membership was far from being simply nominal. He attended the meetings, he read papers and took an active part in the discussions and the business sessions. In all of them it may be said that he was a prominent member, influential in his contribution and personally known to the other active members. This fact in itself, when we consider the range of subjects covered and remember that these societies are composed of specialists and trained investigators, enables us to understand how his influence was so widespread. He came into contact, as a colleague, with the workers in all branches of medical sciences. He knew the kind of work that each was doing, so that to a really remarkable extent he was in sympathetic touch with the progress being made in all departments. To maintain this relationship meant of course that he kept himself informed in the current literature of all these various branches of medicine. He was in fact an indefatigable student in all sides of modern medicine. Members of his family inform me that he subscribed to some 35 medical and scientific periodicals and read them all faithfully in spite of the fact that very poor eyesight made his reading slow and difficult. But he had a retentive memory and was quick to grasp the essentials of a paper and, besides, the effort necessary to keep pace with medical and scientific progress was not for him a task, it was his greatest pleasure. It was this sustained and absorbed devotion that made it possible for him to participate actively and creditably in so many fields of medical research. He was really well informed on many sides, and this broad knowledge, supported, as it was, by an excellent memory for details, enabled him to appear to great advantage in society meetings in the discussions of papers. It would seem as though he was prepared to make some contribution to almost any paper that was presented, although the subject might be far outside his special fields of work. Some phase of the subject would appeal to him or remind him of things that he had read or that he knew from his own experience. Whatever it was, even if somewhat remote or trivial, he was likely to speak it out, as a sort of expression of interest, or to stir up

further discussion. He got the same kind of pleasure in listening to the presentation of scientific papers that he did from following the literature, so that he was an ideal society member, punctilious in attendance and in following the program, an absorbed listener, and willing and able to help in the discussion.

As physiology was his first love so the American Physiological Society was probably the organization in which he was most interested. He was elected to membership in this society at its Philadelphia meeting in December, 1888. From that time until his death he was perhaps its most faithful member in attendance, in the presentation of papers, in participation in the discussions, and in promoting social intercourse among the membership. It was evident that he enjoyed thoroughly these gatherings with his fellow workers. He believed in their importance as a means of promoting the advance of physiological science, and he gave the best that was in him to make the meetings profitable and to maintain the high standards of the society as an organization devoted primarily to the encouragement of research. It is a great good fortune for any society to have a member of this type, one who attends its meetings not from a sense of duty or for reasons of personal advancement, but because he thoroughly enjoys and believes in them. It may be said that Doctor Meltzer listened eagerly to every paper presented and had something to say about most of them. What he said was not always important, but it showed his interest. No one was more appreciative of good or new work and no one was more frank in expressing doubt or criticism when the work was not to his taste. Meltzer was not a good speaker. In spite of his long residence in this country he spoke with a marked accent and was far from being fluent or happy in his choice of words. But he was always sincere and earnest and in the discussions at least made his points so that they could not be misunderstood. In the presentation of his own papers, however, he was not particularly skillful. It was something of a strain to follow him closely, and very frequently he elaborated details at such length that the main points they were intended to demonstrate or illustrate were lost or obscured. But the constant interest that he manifested in all that was going on was singularly effective in the long run. One such member, provided he is free from suspicion of self-seeking, can contribute powerfully to the vitality and interest of an organization. Doctor Meltzer was just as much interested in the business meetings of the society as he was in the scientific sessions. The members of the society as a rule were not greatly concerned in such matters as the choice of officers. As is the case probably in most of such scientific societies, they voted for whatever ticket was presented or was nominated in open meeting. But Doctor Meltzer, because of his genuine belief in the influence of the organization upon the advancement of physiological research, took such matters very much to heart. He had quite decided opinions and expressed them with entire frankness. He looked upon the presidency of the society as an honor and recognition and was anxious that it should go to the men who in his opinion were most deserving. It so happened therefore that for a number of years he was a sort of king-maker. When he thought that a president had served long enough he said so plainly in the open meeting and proposed the name of his successor. And because he had given thought to the matter and was obviously sincere and disinterested, he usually carried the meeting with him. When in the course of time the honor of the presidency came to him it was evident that he appreciated it greatly, and he magnified as much as possible the importance of the office. He was in fact a bit autocratic in exercising its functions, but all of his actions were obviously meant to promote the efficiency and importance of the society. One of his reforms caused some consternation when it was first sprung upon the society. Like other presiding officers he was very anxious to finish the program in good time and for this purpose he brought with him an alarm clock which could be set to ring for any given interval. When a member arose to give a paper for which 15 minutes had been allowed on the program, Doctor Meltzer very carefully wound and set the clock and promptly at the appointed moment the alarm went off. This in itself was sufficient to bring most speakers to a precipitous and, usually, incoherent conclusion. Less sensitive members who made an effort to proceed after the noise was over found that the audience was not with them, the faces of the members reflected their somewhat amused conviction that the speaker's time had struck and it was his duty to sit down. Some of the members

in fact were so much interested in observing the reactions of the absorbed speaker when the alarm broke upon him that I fear their attention was seriously diverted from the substance of his remarks. But Doctor Meltzer, whose sense of humor was not highly developed, took the whole matter very solemnly and conscientiously and ran his program off on schedule time.

In the meetings of the National Academy of Sciences Doctor Meltzer exhibited the same faithfulness in attendance and the same keen desire to participate fully in all the activities of the society. From the time of his election he made it a point to attend all of the spring meetings in Washington and most of the fall meetings in addition. Very frequently he was down on the program for a paper. Sometimes his papers were on quite technical subjects, and it is probable that most of the members present were not able to follow him satisfactorily. His method of presentation, as was stated before, was not well suited for a general audience; he did not possess the art of lucid expression, which depends chiefly no doubt on the ability of the speaker to appreciate the state of mind of his audience in relation to the subject under discussion. His own interest and enthusiasm made him overlook the fact that those whose minds are primarily occupied in other directions need to be led up to a new subject through an introductory incline of explanatory statements. His method was rather to plunge at once in medias res. This, together with his faulty pronunciation of English, made his papers difficult to follow, especially when given, as they usually were, in the large hall of the National Museum with its annoying reverberations. Meltzer himself was impressed with the dignity of this gathering of eminent scientists, and he was much more impressed by the importance and dignity of his subject. Whenever, therefore, he had any new results to communicate he felt it incumbent upon him to bring them forward. This was his idea of what such a gathering of scientists was for—to present and discuss new facts and theories. In the same sense he was an excellent listener to the papers of other members; not at all from any sense of duty but because of his genuine and enthusiastic interest in the advancement of scientific knowledge of all kinds. He was faithful also in his attendance upon the business meetings of the academy. When it came to the election of new members no one was more alert in insisting that the highest standards of scholarship should be applied. Eminence of other kinds he respected, but in his opinion membership in the academy should go only to those who are distinguished for their contributions to science; and when a candidate in his opinion did not measure up to this standard, when there was no evidence of scientific productivity of a high order, he was very frank in expressing his conviction that the candidate should not receive election. The directness with which he expressed himself in such matters was sometimes a bit startling, but no one could fail to appreciate his sincerity and to recognize that he was guided in what he said by what he conceived to be the principles involved.

Meltzer was eminent as a physician and as a scientist. Whether or not he can be called a great scientist depends upon the meaning attached to the word. Properly speaking that designation belongs only to those few workers who by reason of exceptional intellectual ability or by good fortune have added something of such outstanding importance to scientific knowledge that it marks a distinct advance, or constitutes an epoch-making discovery, influencing in a large way the subsequent development of some line of scientific inquiry. Meltzer was not so fortunate as to make a contribution of this kind, but he was a consistent, tireless, and devoted worker in science, holding always to the highest ideals, which he preached and exemplified at all times. He was never faint-hearted nor skeptical about the value of scientific methods and scientific ideals as applied to medicine. When he first came to America medical education and medical standards were at a low level, compared with what had gone before as well as with what has since developed, and this unhappy state of affairs must have made a deep impression upon him. He seems to have set himself the task of improving these conditions. He was a clinician and it was on the clinical side that conditions were the worst. In the scientific branches of medicine a new spirit had arisen. Laboratories of physiology and pathology were being established, and it was recognized for these subjects that special training is required and that men who give themselves to such work are a class apart from the general practitioner. But in clinical medicine the scientific spirit had not penetrated deeply. Meltzer believed with

all his heart that the future of medicine depends upon the applications of science to diagnosis and treatment. He had a perfectly clear vision of the good results to be expected in medical practice from the application of laboratory methods to the study of the normal and pathological physiology of the organism. But this was not the opinion of the great body of practicing physicians at that time. There was marked criticism of the elaborate laboratory methods that were coming into vogue and which seemed at first sight to be so far away from the pressing problems of disease. Meltzer, however, was thoroughly au courant with the modern point of view and he was at all times the vigorous defender and propagandist of experimental methods in medicine. In a very real way he became a prominent and influential representative of scientific medicine. Through his writings and his numerous societies he was constantly in evidence as an advocate of the importance of research, and, as was natural, his influence was felt first upon the younger men. The older men were more or less set in their ways, but the younger, better trained men who were then entering medicine were the hopeful element to be considered. Many of these who have since become prominent have expressed their appreciation of the stimulating effect of Meltzer's sympathy and criticism. The fact that he was recognized as an able practitioner, as well as a successful laboratory worker, gave him a position of advantage which he utilized to the fullest extent in bringing his influence to bear upon the younger men to stimulate them to undertake experimental work, and upon the older men to impress them with the value and necessity of such work. As far as I can ascertain his distinguished services did not bring him any academic calls. Whether or not he would have made a successful professor is perhaps open to question. Most likely his qualities were not such as would have made a good instructor for beginners, although for the few really interested individuals he would no doubt have been a constant strong source of inspiration. His work as an investigator did, however, bring him a call of a most complimentary kind from the Rockefeller Institute of Medical Research.

He was asked in 1904 to take charge of a department of experimental physiology and pharmacology. This invitation presented a serious dilemma. On the one hand, it offered the opportunities that he craved of abundant facilities for research, but, on the other, it meant the suspension of his work as a practitioner. He did not hesitate in making his decision. At a very considerable financial sacrifice he accepted the post, and the long series of important investigations which he published from this laboratory is abundant proof that the selection was a wise one. He retained this position for 15 years, resigning in May, 1919. Shortly before his withdrawal, in 1917, the institute had his portrait painted by Adolphe Borie to commemorate his services. During his latter years at the institute and for the remainder of his life he was in poor health. He had long been a sufferer from diabetes. His own careful supervision of his diet enabled him to control this condition successfully for many years, but during the last 15 years he was restricted to an almost entirely carbohydrate-free dietary. During the last year or two of his life he was a very sick-looking man, wan and feeble, but he did not surrender to his condition. He continued his reading and experimenting and attended faithfully his scientific meetings. Death came to him finally while at work in his study at night. It was his custom apparently when he could not sleep to go into his study to read, and it was on an occasion of this kind that the end came, let us hope suddenly and painlessly. He had long realized that his life was precarious, but his only expressed regret was that he might not have time to do some of the important work that he had planned.

The latter years of his life were saddened, not only by ill health and the consequent limitation of his power to work, but also to a considerable extent by the conditions created by the Great War. Meltzer's education had been obtained in Germany and he had always a feeling of gratitude and appreciation for the opportunities opened to him at that period of his life. He had besides a sincere admiration for the great contributions made to medicine by German physicians and professors, with many of whom he was personally acquainted. When the war started and the tide of anti-German sentiment began to rise rapidly in this country it was directed not only against the policies of the German Government, but oftentimes in an unreasoning way against German science and German scientists. This feeling was augmented

no doubt by the brutally frank expressions of policy contained in the so-called manifesto of the intellectuals and signed by many eminent German professors. Feeling was intense and intolerance was exhibited in academic as well as nonacademic circles to an extent that is scarcely comprehensible now, although only a few years have elapsed. Meltzer, with his philosophic training, his warm affection for the Germany that he knew and admired, his loyal love for his adopted country, and his great belief in the cosmopolitanism of science was greatly distressed for fear that the bitterness engendered by the War might injure permanently the spirit of cooperation and fraternity among scientific workers of different nationalities. Indeed there was much reason at that time to fear that such a result might occur.

On the occasion of the fourth annual dinner of the Biochemical Association of Columbia University he delivered a fine address on "The deplorable contrast between intranational and international ethics and the mission of medical science and medical men." This address was published subsequently in the Biochemical Bulletin (Vol. IV, 1915) and in Science (Vol. XII, p. 515, 1915). In it he drew a vivid picture of the inhumanity created by war, and emphasized the contrast in this respect of the generous medical service rendered by physicians to friend and foe alike. On this basis he suggested the formation of a "medical brotherhood for the purpose of upholding and accelerating the progress of international morality." This idea grew in his mind; he talked it over with all of his friends and finally organized definitely an association which he designated as a "Fraternitas Medicorum." He secured the cooperation of 150 of the leading medical men of the country and issued an appeal for membership in a statement printed in the Journal of the American Medical Association, July 31, 1915. The appeal was successful, some 16,000 names were secured in this country alone, and interest in the project was shown by leading physicians in other countries. Whatever progress might have been made if conditions had remained as they were when the brotherhood was organized, with this country as a neutral power, its actual operations were brought to a close temporarily by the fact that we entered the war. Under such conditions it was probable that any activity that might be initiated would be interpreted as unpatriotic, and in a letter to the editor of the Journal of the American Medical Association Meltzer announced that the sending out of literature would be suspended. After the war was over he made an attempt to revive the organization, hoping especially to induce some other leader in medicine whose German affiliations were less evident than his to undertake the direction of its work. As it turned out interest in the project lapsed somewhat after the cessation of the war, and it would seem as though the whole movement had come to naught. But it was a noble plan that did credit to Meltzer's heart and mind. It is in line with that evolution of the moral sense among mankind which to Huxley's mind constitutes the essential basis and hope of a progressive civilization, and we may assume that sooner or later the seed planted by Meltzer will bear fruit. There will be a world-wide organization such as he dreamed of which will aim to raise international ethics to that standard which now prevails within the confines of every civilized nation.

After Doctor Meltzer's death memorial exercises were held by several of the societies with whose activities he had been especially identified. At the Christmas meeting of the Federation of American Societies for Experimental Biology, held at Chicago, December 28, 1920, Prof. W. H. Howell read a paper commemorative of his life and work in relation to American physiology which was published in Science, February 4, 1921. At a meeting of the Society of Clinical Investigation a similar paper was read by Dr. W. T. Longcope, and at a special memorial meeting of the Society for Experimental Biology and Medicine, held in New York on January 6, 1921, addresses were made as follows: Memorial remarks by the president of the society, Prof. Gary N. Calkins; "A tribute to Doctor Meltzer's life and service," by Prof. George B. Wallace; "Doctor Meltzer's message to the present generation," by Dr. Phoebus A. Levene; "Personal Reminiscences of Doctor Meltzer," by Prof. Graham Lusk; "Doctor Meltzer's influence on American physiology," by Prof. W. H. Howell; "Doctor Meltzer's place in American medicine," by Prof. W. H. Welch. These addresses were subsequently published in full by the society in a memorial number of the proceedings of the society, 1921.

BIBLIOGRAPHY OF DR. S. J. MELTZER

1881–1921

1881

1. Ueber die Schluckmechanismus und dessen nervöse Hemmungen. Von H. Kronecker und S. J. Meltzer. Monatsbericht der Königl. Akademie der Wissenschaften zu Berlin, Januar 24, 1881.
2. On the propagation of inhibitory excitation in the medulla oblongata. H. Kronecker and S. J. Meltzer. Proceedings of the Royal Society, No. 216, 1831.

1882

3. Das Schluckcentrum, seine Irradiationen und die allgemeine Bedeutung derselben. S. J. Meltzer. Inaugural dissertation. Medicin und Chirurgie, August 12, 1882.
4. Geschlecht und Lungenvagus. S. J. Meltzer. Centralblatt f. d. med. Wissenschaften, No. 28, 1882.

1883

5. Die Irradiationen des Schluckcentrum und ihre allgemeine Bedeutung. S. J. Meltzer. Archiv. f. Anat. und Physiol., Physiol. Abth. 1883.
6. Schluckgerausche im Scrobisulus cordis und ihre physiologische Bedeutung. S. J. Meltzer. Centralblatt f. d. med. Wissenschaften, No. 1, 1883.

1884

7. Zu den Schluckgerauschen. S. J. Meltzer. Berl. klin. Wochenschr., No. 30, 1884.
8. Zur Histiophysik der roten Blutkörperchen. S. J. Meltzer und W. H. Welch. Centralb. f. d. med. Wissenschaften, No. 41, 1884.
9. The behavior of the red blood-corpuscles when shaken with different substances. S. J. Meltzer and W. H. Welch. Jour. of Physiol., Vol. V, No. 4, 1884.

1888

10. Ein Fall von Dysphagie nebst Bemerkungen. S. J. Meltzer. Berl. klin. Wochenschr., No. 8, 1888.

1889

11. Ueber die mechanischen Verhältnisse bei der Entstehung der Pneumonie. S. J. Meltzer. Medicinischen Monatsschrift, Februar, 1889.
12. Intubation in cases of foreign bodies in the air passages. S. J. Meltzer. Medical Record, Sept. 21, 1889.
13. Carbolsäurevergiftung bei einem Kinde. S. J. Meltzer. Medicinischen Monatsschrift, April, 1889.

1890

14. On the self-regulation of respiration. S. J. Meltzer. New York Medical Journal. Jan. 18, 1890.
15. Some remarks on my hypothesis of the self-regulation of respiration, and Dr. Cowl's discussion of it. S. J. Meltzer. New York Medical Journal, Nov. 22, 1890.
16. Ueber die Localisation der Tuberkulose in der Lungenspitze und über den Mechanismus des Hineingelangens von corpuscularen Elementen in die Lungenalveolen. S. J. Meltzer. Aus den Verhandlungen des X. internationalen medizinischen Congress. 1890.
17. Ueber den Rhythmus der Athmung und des Herzschlages. S. J. Meltzer. Aus den Verhandlungen des X. internationalen medizinischen Congress. 1890.

1891

18. Theories of respiration. S. J. Meltzer. New York Medical Journal, Aug. 8, 1891.

1892

19. On the importance of vibration to cell life. An experimental study. S. J. Meltzer. New York Medical Journal, Dec. 24, 1892.
20. On the respiratory changes of the intrathoracic pressure, measured in the mediastinum posterior. S. J. Meltzer. Jour. of Physiol., Vol. XII, Nos. 2, 3, and 4, 1892.

21. Die athemhemmenden und anregenden Nervenfasern innerhalb des Vagus in ihren Beziehungen zu Einander und zum Athemmechanismus. S. J. Meltzer. Archiv. f. Anat. u. Physiol., 1892. Physiol. Abthlg.
22. Eine Atropin-Intoxication nach epidermatischen Gebrauch einer Belladonna-Salbe. S. J. Meltzer. New York. med. Monatsschrift, Juni, 1892.
23. Eine Haarnadel in einer männlichen Harnrohre. S. J. Meltzer. New York. med. Monatsschrift, Juli, 1892.

1893

24. On the self-regulation of the beat of the heart. S. J. Meltzer. New York Medical Journal, May 13, 1893.
25. On subphrenic abscess. S. J. Meltzer. New York Medical Journal, June 24, 1893.

1894

26. Recent experimental contributions to the physiology of deglutition. S. J. Meltzer. New York Medical Journal, Mar. 31, 1894.
27. Ueber Myxoedem. S. J. Meltzer. New York. med. Monatsschr., April, 1894.
28. Ueber die fundamentale Bedeutung der Erschütterung fur die lebende Materie. S. J. Meltzer. Zeitschr. f. Biol., Bd. XXX, 1894.
29. Eine luftdiche Pleura-Kanüle. S. J. Meltzer. Zeitschr. f. Instrumentenkunde, Dezember, 1894.

1895

30. The effect of serum injections on the blood. S. J. Meltzer. New York Medical Journal, Apr. 27, 1895.
31. The alleged globulicidal effect of alien serum. S. J. Meltzer. New York Medical Journal, May 11, 1895.
32. On the thyreoid therapy; its history and its use in internal medicine. S. J. Meltzer. New York Medical Journal, May 25, 1895.
33. Ueber die Geschichte der Schilddrüsen-Therapie und über deren Anwendung in der innern Medizin. S. J. Meltzer. New York. medizinisch. Monatsschr., Mai, 1895.
34. An experimental study of direct and indirect faradization of the digestive canal in dogs, cats, and rabbits. S. J. Meltzer. New York Medical Journal, June 15, 1895.
35. Das motorische Verhalten des Verdauungscanales Inductionsströmen gegenüber. S. J. Meltzer. Cent. f. Physiol., Juli 13, 1895.

1896

36. Experimental contribution to the study of the path by which fluids are carried from the peritoneal cavity into the circulation. I. Adler and S. J. Meltzer. Jour. of Exper. Med., Vol. I, No. 3, 1896.
37. On absorption of strychnine and hydrocyanic acid from the mucous membrane of the stomach. An experimental study on rabbits. S. J. Meltzer. Jour. of Exper. Med., Vol. I, No. 3, 1896.
38. Ueber Reizversuche mit Inductionsströmen am Tiermagen. S. J. Meltzer. Arch. f. Verdauungskrankheiten, Bd. II, Heft 2, 1896.
39. Ueber die Unfähigkeit der Schleimhaut der Kaninchenmagens, Strychnin zu resorbiren. S. J. Meltzer. Centralblatt f. Physiol., Aug. 8, 1896, Heft 10.
40. Ueber die Bedeutung der Lymphwege für die Resorption kleiner Flüssigkeitsmengen aus der Bauchhöhle. D. Adler und S. J. Meltzer. Centralb. f. Physiol., Juli 11, 1896.

1897

41. The experiments on the faradization of the stomach of animals. S. J. Meltzer. (Reply to Dr. M. Einhorn's criticism.) New York Medical Journal, Apr. 24, 1897.
42. Diseases of the thyroid and thymus glands, myxoedema, cretinism, Graves' disease, and obesity. S. J. Meltzer. Hare's Therapeutics. 1897.
43. On the paths of absorption from the peritoneal cavity. S. J. Meltzer. Jour. of Physiol., Vol. XXII, Nov. 20, 1897.
44. The bactericidal action of lymph taken from the thoracic duct of the dog. S. J. Meltzer and Charles Norris. Jour. of Exper. Med., Vol. II, No. 6, 1897.
45. A further experimental contribution to the knowledge of the mechanism of deglutition. S. J. Meltzer. Jour. of Exper. Med., Vol. II, No. 5, 1897.
46. Ueber den Verschluss der Glottis während des Schluckactes. S. J. Meltzer. Contralb. f. Physiol., Oct. 2, 1897. Heft. 14.

1898

47. An experimental study of the direct inoculation of bacteria into the spleen of living animals; and a contribution to the knowledge of the importance of a lesion in animal tissue for the lodgement and multiplication of bacteria within it. T. M. Cheesman and S. J. Meltzer. Jour. of Exper. Med., Vol. III, Nos. 4 and 5, 1898.

48. On congenital hypertrophic stenosis of the pylorus in infants. S. J. Meltzer. Medical Record, Aug. 20, 1898.
49. On the nature of the cardiopneumatic movements. S. J. Meltzer. Amer. Jour. of Physiol., Vol. I, Jan. 3, 1898.

1899

50. On the causes of the orderly progress of the peristaltic movements in the œsophagus. S. J. Meltzer. Amer. Jour. of Physiol., Vol. II, Mar. 1, 1899.
51. Inhibition. S. J. Meltzer. New York Medical Journal, May 13, 20 and 27, 1899.
52. Otitis media and earache in lobar pneumonia of children. S. J. Meltzer. The Philadelphia Medical Journal, Aug. 5, 1899.
53. An experimental study of the absorption of strychnine in the different sections of the alimentary canal of dogs. S. J. Meltzer. Amer. Jour. of the Medical Sciences, November, 1899.
54. The effects of shaking upon the red blood cells. S. J. Meltzer. Contributions to the Science of Medicine (Welch Memorial Volume) and Vol. IX of the Johns Hopkins Hospital Reports, 1899.
55. An experimental contribution to the knowledge of the toxicology of potassium chlorate. S. J. Meltzer. International Contributions to Medical Literature, 1899.
56. Poisoning by chlorates. S. J. Meltzer. A reference handbook of the medical sciences. 1899.
57. On the influence of fasting upon the bactericidal action of the blood. S. J. Meltzer and Charles Norris. Jour. of Exper. Med., Vol. IV, 1899.

1900

58. Some of the physiological methods and means employed by the animal organism in its continual struggle against bacteria for maintenance of life and health. S. J. Meltzer. The Phila. Med. Jour., May 5, 1900.
59. Is living animal tissue capable of neutralizing the effects of strychnine and venom? An experimental study. S. J. Meltzer and G. Langmann. The Medical News, Nov. 3, 1900.
60. Wird Strychnin durch lebendes thierisches Gewebe entgiftet? S. J. Meltzer und G. Langmann. S. J. Meltzer. Cent. f. Innere Medizin, No. 37, 1900.
61. Bacteriology in health and disease. S. J. Meltzer. Vol. V., Transactions of the Congress of Amer. Phys. and Surgeons, 1900.

1901

62. Ueber den Einfluss der Peritonealhöhle auf das hämolytische Vermögen des fremden Serums. S. J. Meltzer. Cent. f. Bakt., Bd. XXX, 1901.
63. Paratyphus. S. J. Meltzer. N. Y. Med. Monatsschr., December, 1901.
64. Some experimental data on the significance of concentration and of multiplicity of area in hypodermic injections. S. J. Meltzer. Jour. of Exper. Med., Vol. V, October, 1901.
65. Hæmolysis. S. J. Meltzer. Medical Record, August, 1901.

1902

66. Paratyphoid. S. J. Meltzer. New York Medical Journal, Jan. 25, 1902.
67. On the contraction of the iliopsoas muscle as an aid in the diagnosis of the contents of the iliac fossa. S. J. Meltzer. N. Y. Medical Journal, July 19, 1902.
68. The effects of subminimum doses of strychnine in nephrectomized rabbits. S. J. Meltzer and W. Salant. Jour. of Exper. Med., Vol. VI, p. 107, 1902.
69. The rôle of inhibition in the normal and in some of the pathological phenomena of life. S. J. Meltzer. Medical Record, June 7, 1902.
70. The influence of alcohol upon infection and its use in the treatment of acute infectious diseases. S. J. Meltzer. American Medicine, Vol. IV, p. 60, 1902.

1903

71. Zur Frage der Entgiftung von Strychnin. S. J. Meltzer und G. Langmann. Zent. f. innere Medizin Nr. 3, 1903.
72. Ueber das Verhalten der Resorption nach Nephrectomie. S. J. Meltzer und W. Salant. Cent. f. allg. Pathol., Bd. XIV, 1903.
73. The influence of nephrectomy upon absorption. S. J. Meltzer and William Salant. Jour. of Med. Research, Vol. IX, p. 33, February, 1903.
74. A study of the vasomotor nerves of the rabbit's ear contained in the third cervical and in the cervical sympathetic nerves. S. J. Meltzer and Clara Meltzer. Amer. Jour. of Physiol., Vol. IX, April, 1903.
75. Experiments relating to the question of fixation of strychnine in animal tissue. S. J. Meltzer and G. Langmann. Jour. of Med. Research, Vol. IX, p. 19, 1903.
76. The share of the central vasomotor innervation in the vasoconstriction caused by intravenous injection of suprarenal extract. S. J. Meltzer and Clara Meltzer. Amer. Jour. of Physiol., Vol. IX, May, 1903.
77. Some observations on the effects of agitation upon Arbacia eggs. S. J. Meltzer. Amer. Jour. of Physiol., Vol. IX, July, 1903.

78. On the effects of subcutaneous injection of the extract of the suprarenal capsule upon the blood vessels of the rabbit's ear. S. J. Meltzer and Clara Meltzer. Amer. Jour. of Physiol., Vol. IX, July, 1903.
79. Relation of the innervation of an organ to the influence of suprarenal extract upon it. S. J. Meltzer and Clara Meltzer. American Medicine, Vol. V, p. 216, 1903.
80 Bemerkung uber Gastralgie, Magenkolik and Kolik im Allgemeinen. S. J. Meltzer. Archiv. f. Verdauungskrankheiten, 1903.
81. On a difference in the influence upon inflammation between the section of the sympathetic nerve and the removal of the sympathetic ganglion. S. J. Meltzer and Clara Meltzer. Jour. of Med. Research, Vol. X, p. 135, 1903.
82. Studies on the influence of artificial respiration upon strychnine spasms and respiratory movements. William J. Gies and S. J. Meltzer. Amer. Jour. of Physiol., Vol. IX, No. 1, Mar. 2, 1903.

1904

83. Studies on the "paradoxical" pupil-dilatation caused by adrenalin, Parts I–III. S. J. Meltzer und Clara Meltzer Auer. Amer. Jour. of Physiol., Vol. XI, p. 28, 1904.
84. Vitalism and mechanism in biology and medicine. S. J. Meltzer. Science, Vol. XIX, p. 18, 1904.
85. Our aims. S. J. Meltzer. The Medical News, Oct. 1, 1904.
86. Edema. S. J. Meltzer. American Medicine, Vol. VIII, 1904.
87. Ueber den Einfluss des Nebennierenextraktes auf Resorption und Transudation. S. J. Meltzer und John Auer. Cent. f. allg. Path. u. Pathol. Anat., Bd. XV, 1904.
88. On the clotting of the blood of nephrectomized rabbits. S. J. Meltzer and William Salant. Jour. of Med. Research, Vol. XII, July, 1904.
89. Myelopathic albumosuria. (Kahler's disease; multiple myeloma). S. J. Meltzer. Medical Record, June, 1904.
90. The domain of physiology and its relation to medicine. S. J. Meltzer. American Medicine, Vol. VIII, p. 755, 1904.
91. The influence of suprarenal extract upon absorption and transudation. S. J. Meltzer and John Auer. Trans. Assoc. of Amer. Phys., 1904.

1905

92. The effect of suprarenal extract upon the pupils of frogs. S. J. Meltzer and Clara Meltzer Auer. Amer. Jour. of Physiol., Vol. XI, August, 1905.
93. Studies on the toxicity of bile. I. The effects of intravenous injections of bile upon blood pressure. S. J. Meltzer and William Salant. Jour. of Exper. Med., Vol. VII, June, 1905.
94. Physiological and pharmacological studies of magnesium salts. 1. General anesthesia by subcutaneous injections. S. J. Meltzer and John Auer. Amer. Jour. of Physiol., Vol. XIV, p. 366, 1905.
95. The influence of suprarenal extract upon absorption and transudation. S. J. Meltzer and John Auer. Amer. Jour. of the Med. Sciences, January, 1905.
96. On the rate of absorption from intramuscular tissue. S. J. Meltzer and John Auer. Jour. of Exper. Med., Vol. VII, February, 1905.
97. Inhibitory and anesthetic properties of magnesium salts. S. J. Meltzer. Medical Record, December, 1905.
98. The men we lost. S. J. Meltzer. The Medical News, July 22, 1905.
99. The effect of pilocarpin hydrochlorate in strychnine poisoning. S. J. Meltzer and Wm. Salant. Jour. of the Amer. Med. Assoc., Dec. 31, 1905.

1906

100. Remarks on albuminuria, cylindruria, and Bright's disease. S. J. Meltzer. N. Y. Medical Journal, July, 1906.
101. Spinal anesthesia by magnesium sulphate. H. A. Haubold and S. J. Meltzer. Jour. of the Amer. Med. Assoc., Mar. 3, 1906.
102. Studies on the toxicity of bile. II. The toxic effect of bile upon the central nervous system and the elimination of strychnine through the bile in nephrectomized animals. S. J. Meltzer and William Salant. Jour. of Exper. Med., Vol. VIII, No. 1, 1906.
103. On the sensibility of abdominal organs and the influence of injections of cocaine upon it. L. Kast and S. J. Meltzer. Medical Record, Dec. 29, 1906.
104. Die hemmenden und anesthesierenden Eigenschaften der Magnesiumsalze. S. J. Meltzer. Berl. klin. Wochenschr., Nr. 3, 1906.
105. Vagus reflexes upon œsophagus and cardia. S. J. Meltzer and J. Auer. Brit. Med. Jour., Dec. 22, 1906.
106. Physiological and pharmacological studies of magnesium salts. II. The toxicity of intravenous injections; in particular the effects upon the centers of the medulla oblongata. S. J. Meltzer and John Auer. Amer. Jour. of Physiol., Vol. XV, Mar. 1, 1906.
107. Physiological and pharmacological studies of magnesium salts. III. The narcotizing effects of magnesium salts upon nerve fibers. S. J. Meltzer and John Auer. Amer. Jour. of Physiol., June 1, 1906.

108. Physiological and pharmacological studies of magnesium salts. IV. The relations of the salts to the peristalsis of the gastrointestinal canal. S. J. Meltzer and John Auer. Amer. Jour. of Physiol., Vol. XVII, Dec. 1, 1906.
109. The effects of intraspinal injection of magnesium salts upon tetanus. S. J. Meltzer and John Auer. Jour. of Exper. Med., Vol. VIII, No. 6, Dec. 17, 1906.
110. The action of ergot upon the stomach and intestines. S. J. Meltzer and John Auer. Amer. Jour. of Physiol., Vol. XVII, Oct. 1, 1906.

1907

111. The factors of safety in animal structure and animal economy. S. J. Meltzer. Science, Vol. XXV, No. 639, pp. 481–498, Mar. 29, 1907.
112. Die Sensibilität der Abdominalorgans und die Beeinflussung derselben durch Injektionen von Cocain. L. Kast und S. J. Meltzer. Berl. klin. Wochenschr., No. 19, 1907.
113. Observations on a rabbit for thirty months after the removal of the superior cervical ganglion. S. J. Meltzer. Proc. Soc. for Exper. Biol. and Med., Vol. IV, p. 98, 1907.
114. Secondary peristalsis of the esophagus. A demonstration on a dog with a permanent esophageal fistula. S. J. Meltzer. Proc. Soc. for Exper. Biol. and Med., Vol. IV, p. 35, 1907.
115. Peristaltic rush. S. J. Meltzer and J. Auer. Amer. Jour. of Physiol., Vol. XX, Oct. 1, 1907.
116. Schilddrüse, Epithelkörper und die Basedow'sche Krankheit. S. J. Meltzer. Vortrag, gehalten in der Deutschen Medizinischen Gesellschaft der Stadt New York, Nov. 14, 1907.
117. Physiological and parmacological studies of magnesium salts. S. J. Meltzer and D. R. Lucas. Jour. of Exper. Med., Vol. IX, May, 1907.
118. The influences of diuresis upon the toxic dose of magnesium salts. S. J. Meltzer. Proc. Soc. for Exper. Biol. and Med., Vol. IV, p. 140, 1907.

1908

119. A protest. S. J. Meltzer. Science, Vol. XXVIII, No. 710, Aug. 7, 1908.
120. The nature of shock. S. J. Meltzer. Archives of Inter. Med., July, 1908.
121. Rigor mortis and the influence of calcium and magnesium salts upon its development. S. J. Meltzer and J. Auer. Jour. of Exper. Med., Vol. X, January, 1908.
122. The action of strontium compared with that of calcium and magnesium. S. J. Meltzer and J. Auer. Amer. Jour. of Physiol., Vol. XXI, May 1, 1908.
123. The antagonistic action of calcium upon the irritability effect of magnesium. S. J. Meltzer and J. Auer. Amer. Jour. of Physiol., Vol. XXI, May 1, 1908.
124. The therapeutics of self-repair. S. J. Meltzer. Amer. Jour. of Med. Sciences, July, 1908.
125. Ideas and ideals in medicine. S. J. Meltzer. Jour. of the Amer. Med. Assoc., May 16, 1908.
126. The action of calcium upon the pupil and its relation to the effects of mydriatics. John Auer and S. J. Meltzer. Proc. Soc. for Exper. Biol. and Med., Vol. V, p. 86, 1908.
127. Is the anesthesia and motor paralysis caused by magnesium salts due to asphyxia? S. J. Meltzer and John Auer. Amer. Jour. of Physiol., Vol. XXIII, Dec. 1, 1908.

1909

128. The destructive effect of shaking upon the proteolytic ferments. A. O. Shaklee and S. J. Meltzer. Amer. Jour. of Physiol., Vol. XXV, Nov. 1, 1909.
129. The comparative toxicity of the chlorides of magnesium, calcium, potassium, and sodium. Don R. Joseph and S. J. Meltzer. Jour. of Pharm. and Exper. Therap., Vol. I, No. 1, June, 1909.
130. Die Ehrmannsche Methode. Eine Antwort auf die Entgegnung des Herrn Dr. R. Ehrmann. S. J. Meltzer, Deutsch. med. Wochenschr., No. 22, 1909.
131. Bemerkungen zur Wirkung von Adrenin auf die Froschpupille. S. J. Meltzer. Deutsch. med. Wochenschr., No. 13, 1909.
132. The effect of subminimal stimulation of the pneumogastric nerves upon the onset of cardiac rigor. Don R. Joseph and S. J. Meltzer. Amer. Jour. of Physiol., Vol. XXV, Nov. 1, 1909.
133. The auricular-ventricular bundle and Stanley Kent. S. J. Meltzer. Medical Record, Nov. 27, 1909.
134. The science of clinical medicine. What it ought to be and the men to uphold it. S. J. Meltzer. Jour. of the Amer. Med. Assoc., Aug. 14, 1909.
135. The influence of calcium upon the pupil and the pupilomotor fibers of the sympathetic nerve. John Auer and S. J. Meltzer. Amer. Jour. of Physiol., Vol. XXV, Oct. 1, 1909.
136. Die Sensibilität der Bauchorgane. L. Kast und S. J. Meltzer. Mitteilungen aus den Grenzgebieten der Medizin und Chirurgie. Bd. 19, Heft 4, 1909.
137. The life-saving action of physostigmin in poisoning by magnesium salts. Don R. Joseph and S. J. Meltzer. Jour. of Pharm. and Exper. Ther., Vol. I, No. 3, p. 369, October, 1909.
138. Einiges zur Physiologie und Pharmakologie des Magnesiums und Kalziums. S. J. Meltzer. Deutsch. med. Wochenschr., No. 45, 1909.
139. Die Einflüsse von NaCl und $CaCl_2$ auf die indirekte und direkte Errgbarkeit von Froschmuskeln. Don R. Joseph und S. J. Meltzer. Zent. f. Physiol., Bd. XXIII, 1909.

140. Continuous respiration without respiratory movements. S. J. Meltzer and John Auer. Jour. of Exper. Med., Vol. XI, No. 4, p. 622, 1909.
141. Wandert Adrenalin im Nerven? S. J. Meltzer.
142. The neurogenic and myogenic theories and the modern classification and interpretation of cardiac arrythmias. S. J. Meltzer. Medical Record, May 22, 1909.
143. Postmortem rigor of the mammalian heart and the influence of an antemortem stimulation of the pneumogastric nerves upon its development. Don R. Joseph and S. J. Meltzer. Jour. of Exper. Med., Vol. XI, Nos. 1 and 2, 1909.

1910

144. Dr. Meyer's criticism of the method of continuous iutratracheal insufflation. S. J. Meltzer. Medical Record, Apr. 2, 1910.
145. The practical and scientific significauce of "606," Ehrlich's new remedy. S. J. Meltzer. N. Y. State Medical Journal. October, 1910.
146. The influence of calcium and of sodium in N/10 solution upon the conductivity in nerve trunks. Don R. Joseph and S. J. Meltzer. Proc. of Soc. for Exper. Biol. and Med., Vol. VIII, p. 37, 1910.
147. Experimentelle Erfahrungen über die Behandlung von Strychninvergiftung mit Hilfe der intratrachealen Insufflation. A. O. Shaklee und S. J. Meltzer. Berl. klin. Wochenschr., No. 39, 1910.
148. The respiratory changes of pressure at the various levels of the posterior mediastinum. S. J. Meltzer and John Auer. Jour. of Exper. Med., Vol. XII, No. 1, 1910.
149. Bronchial asthma as a phenomenon of anaphylaxis. S. J. Meltzer. Jour. of the Amer. Med. Assoc., Sept. 17, 1910, p. 1021.
150. Animal experimentation in relation to our knowledge of secretions, especially internal secretions. S. J. Meltzer. Proc. Pathol. Soc. of Phil., September, 1910.
151. Der hemmende Einfluss des Magnesiums auf die indirekte und direkte Erregbarkeit von Froschmuskeln und die antagonistischen Effekte von Natrium und Calcium auf diesen Einfluss. Don R. Joseph und S. J. Meltzer. Zent. f. Physiol., Bd. XXIV, Nr. 1, 1910.
152. Die Methode der intratrachealen Insufflation, ihr wissenschaftliches Prinzip und ihre praktische Anwendbarkeit. S. J. Meltzer. Berl. klin. Wochenschr., Nr. 13, 1910.
153. The method of respiration by intratracheal insufflation, its scientific principle and its practical availability in medicine and surgery. S. J. Meltzer. Medical Record, Mar. 19, 1910.
154. Erwiderung zu den Bemerkungen des Herrn Kuhn über meine Insufflation und Kuhn's perorale Intubation. S. J. Meltzer. Berl. klin. Wochenschr., No. 38, 1910.

1911

155. Some observations on the physiological actions of sodium chloride. Don R. Joseph and S. J. Meltzer. Jour. Pharm., Vol. II, March, 1911.
156. Ueber die Behandlung von experimentellen Strychninvergiftungen mit intratrachealer Insufflation und Aethernarkose. T. S. Githens und S. J. Meltzer. Berl. klin. Wochenschr., Nr. 15, 1911.
157. Das Pylorusgeräusch. S. J. Meltzer. Deutsch. med. Wochenschr., No. 21, 1911.
158. Ueber Verteilung und Wirkungen vou gelösten Substanzen bei entherzten Fröschen. S. J. Meltzer. Zent. f. Physiol., Bd. XXV, No. 2, 1911.
159. On the distribution and action of soluble substances in frogs deprived of their circulatory apparatus. S. J. Meltzer. Proc. of the Royal Society, b., vol. 84, 1911.
160. The inhibitory action of sodium chloride upon the phenomena following the removal of the parathyroids in dogs. A preliminary communication. Don R. Joseph and S. J. Meltzer. Jour. of Pharm., Vol. II, No. 4, March, 1911.
161. Contributions to our knowledge of the action of sodium and calcium upon the direct and indirect irritability of the muscles of the frogs. Don R. Joseph and S. J. Meltzer. Amer. Jour. of Physiol., Vol. XXIX, Nov. 1, 1911.
162. On the convulsant action of acid fuchsin upon frogs deprived of their cardiac circulation. Don R. Joseph and S. J. Meltzer. Jour. of Pharm., Vol. III, No. 2, November, 1911.
163. The control of strychnine poisoning by means of intratracheal insufflation and ether. T. S. Githens and S. J. Meltzer. Jour. of Pharm., Vol. II, No. 4, March, 1911.
164. On absorption from intramuscular tissue. J. Auer and S. J. Meltzer. Jour. of Exper. Med., Vol. XIII, No. 3, 1911.
165. Intratracheal insufflation. S. J. Meltzer. Jour. of the Amer. Med. Assoc., Aug., 12, p. 521, 1911.
166. On the injection of drugs, especially of salvarsan (Ehrlich) into the lumbar muscles. S. J. Meltzer. Medical Record, Mar. 25, 1911.
167. Ueber die beschleunigte Resorption aus den Lendenmuskeln und die Verwendbarkeit dieser Muskeln für die Injektion von Salvarsan (Ehrlich). S. J. Meltzer. Berl. klin. Wochenschr., Nr. 10, 1911.
168. The status of respiratiou iu the methods of differential pressure compared with that under the method of intratracheal insufflation. John Auer and S. J. Meltzer. Jour. of Exper. Med., Vol. XIV, No. 6, 1911.
169. The present status of therapeutics and the significance of salvarsan. S. J. Meltzer. Jour. of the Amer. Med. Assoc., p. 1709, June 10, 1911.

170. The distribution of solutions in cardiectomized frogs. S. J. Meltzer. Jour. of Exper. Med., Vol. XIII, No. 4, 1911.
171. An experimental study of the question of aspiration of foreign material into the air passages during intratracheal insufflation. T. S. Githens and S. J. Meltzer. Jour. of Exper. Med., Vol. XIII, No. 4, 1911.

1912

172. Experimental pneumonia by intrabronchial insufflation. R. V. Lamar and S. J. Meltzer. Jour. of Exper. Med., Vol. XV, No. 2, 1912.
173. Intratracheale Insufflation in physiologischen Laboratorium. S. J. Meltzer. Zent. f. Physiol., Bd. XXVI, Nr. 4, 1912.
174. Experimentelle fibrinöse Pneumonie, hervorgebracht durch intrabronchiale Insufflation. R. V. Lamar und S. J. Meltzer. Cent. f. allg. Pathol. u. patholog. Anatomie, Bd. XXIII, Nr. 7, 1912.
175. Morphin und Kardiektomie. T. S. Githens und S. J. Meltzer. Zent. f. Physiol., Bd. XXVI, Nr. 3, 1912.
176. Pharyngeal insufflation, a simple method of artificial respiration. A preliminary note. S. J. Meltzer. Jour. of Amer. Med. Assoc., Vol. LVIII, p. 1413, May 11, 1912.
177. The training of the desirable practitioner and his mission. S. J. Meltzer. Jour. of the Amer. Med. Assoc., Vol. LIX, p. 585, Aug. 24, 1912.
178. Experimental bronchopneumonia by intrabronchial insufflation. Martha Wollstein and S. J. Meltzer. Jour. of Exper. Med., Vol. XV, p. 126, 1912.

1913

179. Pneumonic lesions made by intrabronchial insufflation of nonvirulent pneumococci. Martha Wollstein and S. J. Meltzer. Jour. of Exper. Med., Vol. XVII, No. 3, 1913.
180. Ueber die anästhetische und lähmende Wirkung von Magnesium, unterstützt durch Äther, 1. Die Wirkung von Magnesiumsulphat und Äther bei Tieren. S. J. Meltzer und John Auer. 2. Magnesium und Äther bei Operationen am Menschen. S. J. Meltzer. Zent. f. Physiol., Bd. XXVII, No. 12, 1913.
181. Der afferente Splanchnikus als Depressor. John Auer und S. J. Meltzer. Zent. f. Physiol., Bd. XXVI, No. 26, 1913.
182. Anesthesia by intratracheal insufflation. S. J. Meltzer. Keen's Surgery. Vol. No. 6, p. 968, 1913.
183. Simple devices for effective artificial respiration in emergencies. S. J. Meltzer. Jour. of the Amer. Med. Assoc., p. 1407, May 10, 1913.
184. Pneumonic lesions caused by bacillus megatherium. Martha Wollstein and S. J. Meltzer. Jour. of Exper. Med., Vol. XVIII, No. 5, 1913.
185. The character of the pneumonic lesions produced by intrabronchial insufflation of virulent streptococci. Martha Wollstein and S. J. Meltzer. Jour. of Exper. Med., Vol. XVIII, No. 5, 1913.
186. The relation of the rate of absorption of adrenalin to its glycosuric and diuretic effects. I. S. Kleiner and S. J. Meltzer. Jour. of Exper. Med., Vol. XVIII, No. 2, 1913.
187. The reaction of the lungs to the intrabronchial insufflation of killed virulent pneumocococci and of plain sterile bouillon. Martha Wollstein and S. J. Meltzer. Jour. of Exper. Med., Vol. XVII, No. 4, 1913.

1914

188. Ueber die kombinierte Wirkung von Oxalaten und Magnesium Salzen und die Gegenwirkung von Kalziumsalzen. F. L. Gates und S. J. Meltzer. Zent. f. Physiol., Bd., XXVII Nr. 22, 1914.
189. Ueber eine Methode zur experimentellen Erzeugung von Pneumonie und ueber einige mit dieser Methode erzielte Ergebnisse. S. J. Meltzer. Berl. klin. Wochenschr., Nr. 29, 1914.
190. Apnœa as an after-effect of pulmonary distension and its dependence upon the vagus nerves. Demonstration. T. S. Githens and S. J. Meltzer. Proc. of the Soc. for Exper. Biol. and Med., XII, p. 64, 1914.
191. The influence of depancreatization upon the state of glycemia following the intravenous injection of dextrose in dogs. I. S. Kleiner and S. J. Meltzer. Proc. of the Soc. for Exper. Biol. and Med., Vol. XII, p. 58, 1914.
192. Distribution of solutions in cardiectomized frogs with destroyed or inactive lymph hearts. T. S. Githens and S. J. Meltzer. Proc. of the Soc. for Exper. Biol. and Med., Vol. XII, p. 60, 1914.
193. Professor Hugo Kronecker. S. J. Meltzer. Biochem. Bulletin, Vol. III, July, 1914; also Science, July, 1914.
194. Der gegenwärtige Stand der intratrachealen Insufflation. S. J. Meltzer. Berl. klin. Wochenscher., Nr. 15 u. 16, 1914.
195. How deep should the tube be introduced in intracheal insufflation? S. J. Meltzer. Jour of the Amer. Med. Assoc., p. 1547, May 16, 1914.
196. Headship and organization of clinical departments of first-class medical schools. S. J. Meltzer. Science, Vol. XI, p. 620, Oct. 30, 1914.

1915

197. Magnesiumsulphat bei Tetanus. S. J. Meltzer. Berl. klin. Wochenschr., Nr. 11, 1915.
198. The effect of previous intravenous injection of pneumococci upon experimental lobar pneumonia produced by the method of intrabronchial insufflation. B. S. Kline and S. J. Meltzer. Proc. of the Soc. for Exper. Biol. and Med., Vol. XII, p. 185, 1915.

20154°—26——31

199. The relation of the purgative action of magnesium sulphate to peristalsis, and the general law of crossed innervation. S. J. Meltzer. Arch. of Inter. Med., Vol. XV, p. 955, June, 1915.
200. Pharyngeale Insufflation, ein einfacher Apparat für künstliche Atmung am Menschen; nebst Bemerkungen ueber andere Methoden der künstlichen Atmung. S. J. Meltzer. Berl. klin. Wochenschr., Nr. 17, 1915.
201. The aims of the Association of American Physicians. S. J. Meltzer. Jour. of the Amer. Med. Assoc., p. 2043, June 19, 1915.
202. Retention in the circulation of dextrose in normal and depancreatized animals, and the effect of an intravenous injection of an emulsion of pancreas upon this retention. I. S. Kleiner and S. J. Meltzer. Proc. of the Natl. Acad. of Sci., Vol. 1, p. 338, June, 1915.
203. Fraternitas Medicorum. S. J. Meltzer. Jour. of the Amer. Med. Assoc., Vol. 65, p. 971, Sept. 11, 1915.
204. Production of pneumonic lesions by intrabronchial insufflation of unorganized substances. S. J. Meltzer and B. S. Kline, Proc. of the Soc. for Exper. Biol. and Med., Vol. XIII, p. 29, 1915.
205. The deplorable contrast between intranational and international ethics, and the mission of medical science and medical men. S. J. Meltzer. Biochem. Bulletin. Vol. IV, p. 279, 1915; also Science, Vol. IV, p. 515, 1915.

1916

206. Inhibitory properties of magnesium sulphate and their therapeutic application in tetanus. S. J. Meltzer. Jour. of the Amer. Med. Assoc., Vol. LXVI, p. 931, Mar. 25, 1916.
207. An experimental study of the additive and antagonistic actions of sodium oxalate and salts of magnesium and calcium in the rabbit. F. L. Gates and S. J. Meltzer. Jour. of Exper. Med., Vol. XXIII, pp. 655, May 1, 1916.
208. The intravenous injection of magnesium sulphate for anesthesia in animals. J. Auer and S. J. Meltzer. Jour. of Exper. Med., Vol. XXIII, p. 641, May 1, 1916.
209. On the augmenting action of ergotoxin (Dale and Barger) on the gastro-intestinal movements. S. J. Meltzer and T. S. Githens. Proc. of the Soc. for Exper. Biol. and Med., Vol. XIII, p. 87, 1916.
210. The influence of intravenous injections of magnesium sulphate upon the activities of the center of deglutition. J. Auer and S. J. Meltzer. Proc. of the Soc. for Exper. Biol. and Med., Vol. XIII, No. 6, Mar. 15, 1916.
211. An active expiratory muscle in the chicken which is inhibited by stimulation of the central end of the vagus. A demonstration. A. L. Meyer and S. J. Meltzer. Proc. of the Soc. for Exper. Biol. and Med., Vol. XIII, Apr. 18, 1916.
212. The influence of morphin upon the elimination of intravenously injected dextrose in dogs. I. S. Kleiner and S. J. Meltzer. Proc. of the Natl. Acad. of Sci., vol. 2, p. 369, July 1916.
213. The treatment of acute poliomyelitis. S. J. Meltzer. New York Medical Journal, Aug. 29, 1916.
214. The treatment of infantile paralysis. S. J. Meltzer. Medical Record, July 22, 1916.
215. The serum treatment of poliomyelitis. S. J. Meltzer. New York Medical Journal, Sept. 2, 1916.
216. Autoinoculation in poliomyelitis; a renewed criticism of the method. S. J. Meltzer. New York Medical Journal, Sept. 23, 1916.
217. Anesthesia in human beings by intravenous injection of magnesium sulphate. Charles H. Peck and S. J. Meltzer. Journal of the Amer. Med. Assoc., Vol. LXVII, p. 1131, Oct. 14, 1916.

1917

218. Temporary suspension of activities of the Medical Brotherhood. S. J. Meltzer, Pres. Medical Brotherhood. Jour. Amer. Med. Assoc., p. 930, Mar. 24, 1917.
219. The disturbance of the law of contrary innervation as a pathogenetic factor in the diseases of the bile and the gall-bladder. S. J. Meltzer. Amer. Jour. of the Med. Sciences, Vol. CLIII, No. 4, p. 469, 1917.
220. History and analysis of the methods of resuscitation; with a description of the author's pharnygeal insufflation apparatus for artificial respiration in man. S. J. Meltzer. Medical Record, July 7, 1917.
221. The therapeutic value of oral rhythmic insufflation of oxygen; with description of a simple apparatus for its execution. S. J. Meltzer. Jour. of the Amer. Med. Assoc., Vol. LXIX, p. 1150, Oct. 6, 1917.
222. An air-tight pleura cannula. S. J. Meltzer. Amer. Jour. of Physiol., Vol. XLV, No. 1, December, 1917.

1918

223. Physiology of ærophagia. S. J. Meltzer. Medical Record, Jan. 19, 1918.
224. The administration of epinephrin by intraspinal injections, in acute or subchronic cases accompanied by a low blood pressure. John Auer and S. J. Meltzer. Jour. of the Amer. Med. Assoc., Vol. 70, p. 70, Jan. 12, 1918.
225. Prevention of blood clotting by Dakin's sodium hypochlorite solution. T. S. Githens and S. J. Meltzer. Jour. of the Amer. Med. Assoc., Vol. 70, p. 1115, Apr. 13, 1918.
226. Thoracic surgery. S. J. Meltzer. Medical Record. July 6, 1918.
227. The effect of painting the pancreas with adrenalin upon hyperglycemia and glycosuria. I. S. Kleiner and S. J. Meltzer. Jour. of Exper. Med., Vol. XXVII, No. 6, p. 647, June 1, 1918.

228. The minimum number of respirations sufficient to maintain life. A. L. Meyer and S. J. Meltzer. Proc. of the Soc. for Exper. Biol. and Med., Vol. XV, p. 128, 1918.
229. Experimental chemical pneumonia. Martha Wollstein and S. J. Meltzer. Jour. of Exper. Med., Vol. XXVIII, p. 547, Nov. 1, 1918.
230. Effect of intrabronchial insufflation of solutions of some inorganic salts. Martha Wollstein and S. J. Meltzer. Jour. of Exper. Med. Vol., XXVIII, p. 551, Nov. 1, 1918.
231. A method of producing experimental shock. T. S. Githens, I. S. Kleiner, A. L. Meyer, and S. J. Meltzer. Proc. Soc. Exper. Biol. and Med., Vol. XVI, p. 3. 1918.
232. Influence of mere opening of the abdomen, of state of shock, and of subsequent section of the sciatic nerve upon the blood flow from the femoral vein in cats. T. S. Githens, I. S. Kleiner, A. L. Meyer, and S. J. Meltzer. Proc. Soc. Exper. Biol. and Med., Vol. XVI, p. 6, 1918.
233. Some theories of shock. S. J. Meltzer. Penn. Med. Jour., Vol. XXII, p. 129, December, 1918.
234. The application of a concentrated solution of magnesium sulphate to scalds and burns. S. J. Meltzer. Jour. Pharm., Vol. XII, No. 4, November, 1918.
235. The blood-pressure curve following an intraspinal injection of adrenalin. J. Auer and S. J. Meltzer. Amer. Jour. Physiol., Vol. XLVII, No. 3, December, 1918.
236. Experimental pneumonia produced by *streptococcus hemolyticus*. Martha Wollstein and S. J. Meltzer. Proc. Soc. Exper. Biol. and Med., Vol. XVI, p. 40, 1918.

1919

237. Insufflation of oxygen in pneumonia. S. J. Meltzer. Boston Med. and Surg., Jour., Vol. CLXXX, No. 13, p. 249, Mar. 27, 1919.
238. The production of shock. S. J. Meltzer. Jour. Amer. Med. Assoc., Vol. 73, p. 439, Aug. 9, 1919.
239. The nose-licking reflex and its inhibition. S. J. Meltzer and T. S. Githens. Amer. Jour. of Physiol., Vol. L, No. 3, December, 1919.
240. The influence of degeneration of one vagus nerve upon the development of pneumonia. S. J. Meltzer and Martha Wollstein. Proc. Natl. Acad. Sci., Vol. V, p. 493, November, 1919.

1920

241. The needs of men of science in Rumania. S. J. Meltzer. Science, Vol. LII, p. 292, Sept. 24, 1920.
242. The dualistic conception of the processes of animal life. S. J. Meltzer. Transactions of the Association of American Physicians, 1920.
243. Are the superior cervical ganglia indispensable to the maintenance of life? S. J. Meltzer. Proceedings of the National Academy of Sciences, Vol. VI, p. 532, 1920.

1921

244. On the duration of constriction of blood vessels by epinephrin. S. J. Meltzer and John Auer. The Journa of Pharmacology and experimental Therapeutics, Vol. XVII, p. 177, 1921.

Edward W. Morley.

NATIONAL ACADEMY OF SCIENCES

Volume XXI
TENTH MEMOIR

BIOGRAPHICAL MEMOIR EDWARD WILLIAMS MORLEY

1838-1923

BY

FRANK WIGGLESWORTH CLARKE

Presented to the Academy at the Annual Meeting, 1924

EDWARD WILLIAMS MORLEY

By Frank Wigglesworth Clarke

Edward Williams Morley was born at Newark, N. J., January 29, 1838, and died at Hartford, Conn., February 24, 1923. His father, Sardis Brewster Morley, was a Congregational minister. His mother, whose maiden name was Anna Clarissa Treat, had been a teacher in a girl's school; and Catherine Beecher, the head of the school, said of her, "Clarissa Treat can make girls learn who can't learn." She was evidently a good teacher. Both parents came of early colonial stock, and of purely British origin. The Morley ancestry has been traced back as far as 1681, all in the Connecticut River Valley; but beyond that date the records are obscure. There were men of the same name in eastern Massachusetts as early as about 1630, but no connection between them and Edward's family has been traced.

Soon after Edward's birth the family moved to Hartford, where they remained until 1851. Then his father accepted the pastorate of a Congregational church in Attleboro, Mass. In 1857 they moved to Williamstown, Mass., in order to put Edward and his two brothers through Williams College, their father's alma mater.

In his childhood Morley suffered from ill health, and until he reached the age of 19 his education was carried on at home. His father was his teacher. He learned to read before he was 3 years old, began Latin at 6 and Greek at 11. I gather this information, and much that follows, from some autobiographical memoranda, in Morley's own handwriting, which now lie before me. These memoranda give clear information as to the influences which led him to a scientific career, and how he happened to become a chemist.

When Edward was quite a young lad he found among his father's many books one entitled, "Conversations on Chemistry," which fascinated him even more than the Arabian Nights, that stood near it on the same shelf. When 12 years old he spent all his pocket money on chemical experiments, which he carried on until he entered college. At about the age of 14 he obtained a copy of a textbook on chemistry by Benjamin Silliman, and, says Morley, "this was so much studied that when the subject was taken up in the junior year of my college course there was not much left to be learned." That means, of course, from such books as were within his reach.

At the age of 19 Morley entered Williams College. His preparation for college had been so thorough that he was able to skip the freshman year and to enter with advanced standing as a sophomore. He graduated in 1860 as A. B., and was valedictorian of his class. In 1863 he received his master's degree. A classmate said of him, "Morley recites everything as if it were the most interesting subject he knew," a saying which well describes one of his dominant mental characteristics. He was thorough in everything that he undertook.

Under Prof. Albert Hopkins, astronomy became a fascinating study. It was perhaps not so interesting as chemistry, but it provided an opportunity for the study of methods of precision which was not possible in any other subject at that time and place. Morley, while remaining at Williams for further study, mounted a transit instrument in the prime vertical, constructed a chronograph with Bond's spring governor, and determined the latitude of the college observatory, which so far was known only by sextant observations to within about five seconds of arc. Morley's determination was the subject of his first published paper, which was read before the American Association for the Advancement of Science in 1866. He also read much in the Méchanique Céleste of LaPlace. Furthermore, in a work on astronomy which was read in college, Morley found the statement "that if all the members of the solar system were suddenly brought to rest, they would all fall in straight lines to the center of gravity of the system." Morley called the attention of the editor of the book to the fact that the statement was erroneous, and at his request wrote a paper establishing the configuration of the system that would be required in order to make the statement true. This paper was not

intended for publication. These details I take almost verbatim from Morley's personal memoranda. This early work is a forecast of his later career, and illustrates his intense devotion to accuracy. He might well have become an astronomer, but chemistry was his first love.

It has already been said that Morley's father was a clergyman; and so, too, was his mother's only brother. Quite naturally it was decided that he should follow their example, and so in 1861 he entered Andover Theological Seminary, where he completed the course of study in 1864. It was here, doubtless, that he added to his knowledge of Latin and Greek a good working knowledge of Hebrew.

Morley's health was still poor, and he felt unable to undertake the duties of a minister. The Civil War was nearing its end, and during the year 1864–65 he was in the service of the sanitary commission, and was put in charge of their station at Fortress Monroe, Va. The next year he resumed his studies at Andover, and in the years 1866–1868 he taught in the South Berkshire Academy, at Marlboro, Mass. What subjects he taught is not stated.

His health was now restored and he sought for an opportunity to enter the ministry. Presently he was called to a small church at Twinsburg, Ohio. At about the same time he was offered and accepted the chair of natural history and chemistry in Western Reserve College, at Hudson, Ohio. In this very mild contest between science and theology, science was victorious. This was the turning point of Morley's career. He might have succeeded as a clergyman, for he was conscientious in all his varied kinds of work. He was an admirable lecturer, and doubtless would have been a good preacher; but what a loss to science had he chosen the other path. In scientific research Morley found his true vocation.

When Morley assumed his professorship at Hudson his opportunities for anything like original scientific work were small. The college, like nearly all American colleges at that time, had the old-fashioned fixed curriculum, in which the so-called classical languages were given the first place, and the natural and physical sciences were subordinate to them. The languages were supposed to be the essentials of scholarship; and a man who was deficient in Latin and Greek was not regarded as a scholar. Mathematics was taught as a matter of course, but was not carried very far. The observational and experimental sciences were mainly if not entirely taught by lectures and recitations, which might give some useful information but hardly any mental discipline. Few students were brought to realize that great new branches of knowledge had been developed, which were not only alive but also rapidly growing. Shortly before moving to Hudson, Morley married Miss Isabella Ashley Birdsall, of West Winsted, Conn. They had no children.

It is easy to see that Morley entered upon his duties as a college professor under a heavy handicap. He was called upon to teach, in addition to chemistry, two other sciences, namely, geology and botany, which left him little time for research. For the students the conditions were equally unfortunate. The college curriculum allowed so little time for instruction in any branch of science that even the brightest student could hardly get any real insight into the true significance of his studies.

Morley, however, was not easily discouraged. In spite of difficulties, he fitted up a small room as a chemical laboratory, and his pupils were given their first experience in laboratory practice; that is, they were taught to experiment, to observe facts accurately, and to draw correct conclusions from what they saw. They gained a new kind of mental discipline, of which the classicists had never dreamed. In this mode of teaching Morley originated nothing. He was merely a pioneer in the work of the smaller colleges; work in which the older American universities had already gone far. In the universities of continental Europe the laboratory methods of instruction had long been established, and American students who were able to do so went to France and Germany for advanced training in the sciences, and especially in methods of research. Morley had not had the advantage of foreign study, but he saw the light and followed it. At quite an early date he was even able to offer his students a course in qualitative analysis. Few of the smaller colleges went so far.

Professor Tower, in his biographical notice of Morley, of which I venture to make free use, says that his teaching—

was always inspiring. He insisted not only on a mastery of the principles of chemistry, but he also inculcated the proper use of the English language, good manners, and clear methods of expression. In a word, he made a course in general chemistry a liberal education in itself. This he could do very effectively while the classes were small, but after 1900, as the classes grew larger, he confessed he had not accomplished all he had hoped for. This made teaching less a pleasure to him, and he was not sorry to give it up in 1906. His former students always speak of him with great loyalty and acknowledge the helpful influence of his teaching.

In the college at Hudson, as in all other small colleges, the salaries paid the professors were small. Morley, therefore, found it desirable, and perhaps necessary, to eke out his income by expert work, chiefly analytical, outside of his regular duties. I have no means of knowing whether he was often called upon to testify as an expert in court, but I do know of one amusing case in which he testified to good purpose. I have the story from his own lips, although I had already heard it from others.

The case to which I refer involved certain questions relative to sugar, and the day before the hearing the lawyer on the opposite side notified his friends that if they wanted to see some fun they had better be present and hear him cross-examine the professor. The cross-examination began, and after some preliminary questions the lawyer said: "Now, Professor Morley, what is the chemical formula of sugar?" "What sugar do you mean?" Morley replied. "I asked you, Professor Morley, to tell me the chemical formula of sugar." Morley repeated his former answer. "Professor Morley," said the lawyer, "you need not try to evade my question, but if you don't know the formula of sugar you are not qualified to appear as an expert in this case." "Well," said Morley, "if you don't know that there are at least 20 different sugars, you are not qualified to cross-examine me." The hearers were amused, but not quite as their legal friend had expected. This anecdote illustrates Morley's quickness of mind, and suggests that in his first reply he had laid a trap for the lawyer. He had doubtless heard many foolish questions from students, and knew how to deal with them.

In 1873 Morley added the professorship of chemistry and toxicology in the Cleveland Medical School to his other duties, which forced him to spend much time in traveling between Hudson and Cleveland. This position he resigned in 1888 when his other work made greater demands upon his time. In 1882 the college was moved from Hudson to Cleveland, where, as Adelbert College, it became a part of Western Reserve University. This meant an increase in Morley's opportunities for research, and at Cleveland, where he remained until his retirement, he carried out most of the larger investigations which made him known to the world as an experimentalist of the first rank.

During his residence in Cleveland Morley brought together one of the best private collections of chemical periodicals to be found in America. He even included in it a Russian journal, and learned enough of the Russian language to make use of it. After his retirement from teaching, the university bought his library, and it is now in the new chemical laboratory, for which he drew the plans, and which is now known as the Morley Chemical Laboratory. In 1906 he went to West Hartford, Conn., near the home of his boyhood, where he built a small house, with a garage, and also a laboratory, in which he made, with his usual thoroughness, many analyses of rocks and minerals. Of this work I shall have more to say later. Morley could not be idle. Indeed, for many years he was in the habit of working about 14 hours a day.

Except during Morley's early years his health was fairly good, which made it possible for him to perform his many and varied labors. Professor Tower says of him that—

his laboratory and classroom were on the third floor of the main building of Adelbert College, while his research work was carried on in the basement. He would make the trip up and down stairs scores of times a day, watching a class in quantitative analysis at the same time that he was engaged upon one of his most delicate operations in the basement. Out of curiosity he one day attached to himself a pedometer, and found that he walked about 20 miles when putting in a busy day. However, no constitution could withstand indefinitely this strain of long hours and hard work. In 1895 his strength gave out and he was granted a year's leave of absence, which he spent in Europe, resting and recuperating. This was the only full year's rest that he took during his teaching. . . . After his return in 1896 the college authorities granted him more assistance, and in 1898 the trustees voted to relieve him of as much teaching as he was willing to relinquish, so that he could give more time to research. He chose, however, to retain the course in general chemistry and the one in quantitative analysis.

These courses he continued to teach until he retired as professor emeritus.

In 1878 Morley began the series of investigations that made him famous. His attention had already been drawn to the fact that the proportions of oxygen in air were not absolutely constant, but subject to slight variations. What do these variations mean? That is the question that Morley sought to answer, at least in part, and his task was one of extreme delicacy. The solution of the problem involved the construction of elaborate eudiometric apparatus, with which the probable error of a determination of oxygen in air was not more than one four-hundredth of 1 per cent. The results of his investigation appeared in 10 separate papers, published between the years 1879 and 1882, three years of labor.

On one side Morley's problem was meteorological. Professor Loomis, of Yale College, had put forth the hypothesis that so-called "cold waves," those severe and sudden falls in the temperature of the air, were not due to horizontal currents moving from the north southward but to the descent of air from high elevations at times of high barometric pressure. The upper layers of the atmosphere were poorer in the relatively heavier oxygen than the lower layers near the surface of the earth. Hence, if the Loomis hypothesis is correct, the air collected during a cold wave should show a deficiency of oxygen.

Morley had already made analyses of air from different localities, and in 1880, during 110 consecutive days, he made analyses of the air at Hudson. Each determination of oxygen was made on the day that the sample of air was taken. To quote Morley's own words:

> The theory that the deficiencies of oxygen in the atmosphere are caused by the descent of air from an elevation fairly well agrees with the facts.

This cautious statement shows the scrupulous honesty of the man. A more positive assertion would have been justifiable.

An attempt to trace the workings of another man's mind would of course be rather presumptuous. To do that is the privilege of the novelist, who can create imaginary characters. It seems highly probable, however, that Morley's research upon the composition of air had much in it to suggest his next and most famous investigation on the composition of water; that is, on the relative atomic weights of oxygen and hydrogen. The transition from one research to the other was quite natural. An intermediate step was the determination of the amount of moisture retained by gases after drying by means of sulphuric acid, or over phosphorus pentoxide, for that was an essential preliminary to his work on the composition of water by volume; a study of the proportions by volume in which the two gases, oxygen and hydrogen, combine. The volume relations and the weight relations are not the same. In the determination of gaseous densities the first relation is needed as a small correction to the other.

Morley's work on the atomic weight of oxygen, that of hydrogen being taken as unity, covered a period of 11 years. Much time was spent in the detection of constant errors, and in making "assurance doubly sure" as to the purity of his materials. No precaution was overlooked, for the highest possible accuracy was his aim.

The determinations of atomic weight were made by two distinct methods. First, he effected the direct synthesis of water from weighed quantities of its component elements. The hydrogen was weighed as occluded in palladium, 600 grams of that metal being used. Secondly, he determined the density of the two gases, using the correction mentioned in the preceding paragraph. The results obtained checked each other within the admissible range of experimental uncertainty, and later determinations by other chemists have differed but slightly from Morley's. His final value for the atomic weight of oxygen was 15.879. The outstanding uncertainty is probably not much greater than 1 part in 10,000.

It is evident that this research of Morley's upon atomic weights was quite as much physical as chemical. Even while he was engaged upon it he found time to cooperate with others in some purely physical investigations. He worked with A. A. Michelson in the famous attempt to determine the relative motion of the earth and the luminiferous ether, and also upon the possibility of establishing a light wave as an absolute standard of length. With H. T. Eddy, and afterwards with D. C. Miller, he studied the velocity of light in a magnetic field. With Miller he also investigated the thermal expansion of air, nitrogen, and carbon dioxide. In connection with the latter research Morley constructed a new form of manometer by which differences of pressure as small as one ten-thousandth of a millimeter could be measured. With W. A. Rogers he studied the expansion of metals by the interferential method; and with C. F. Brush the conduction of heat

through water vapor. These researches were published, some of them only in abstract, between the years 1886 and 1905. The references to them can be found in the bibliography at the end of this memoir.

Of all these physical researches the most noteworthy is that of the relative motion of the earth and the ether. Fizeau had already shown that the ether is entirely unaffected by the motion of the matter which it permeates. To repeat Fizeau's experiment was the first task in which Michelson and Morley cooperated, and this was followed by another experiment to detect any difference in the velocity of light owing to the motion of the apparatus toward or away from waves of light in the ether. The details of these investigations can not be considered here, but Morley in his personal memoranda gives a clear statement as to his share in them. I now quote Morley, almost but not quite literally. He speaks in the first person, which I venture to change to the third. Beyond this alteration only a few words have been changed from the wording in the original statements, and then only for the sake of clearness.

When Michelson was ready to repeat Fizeau's experiment, certain conveniences were available in Morley's laboratory, and he was cordially invited to make use of them. Morley had no assistant, and so, naturally, it fell to him to see that Michelson had what was needful, and so became pretty familiar with Michelson's plans. His work was interrupted by illness and absence, and an erroneous diagnosis led him to turn over to Morley an appropriation for the experiment with a request to him to conclude it. Morley had got the apparatus ready for the final observations when Michelson wrote that he should return in a short time. Morley, therefore, ceased work, and wrote, surrendering the conduct of the experiment to Michelson, and the latter accordingly proposed that it should be a joint experiment.

When Michelson was ready to make a decisive experiment on the velocity of light parallel to and across the line of drift through the luminiferous ether, the best available place for the apparatus was again found to be Morley's laboratory, and this experiment also was made a joint affair. The result was negative. No difference was found.

In 1900, as Morley was going to a meeting of the Congress of Physics in Paris, Lord Kelvin saw him and asked him with much earnestness if there was any possibility of escape from the unexpected result of the experiment mentioned above. The conversation showed that Lord Kelvin was anxious to know whether the result would be in any degree altered by change of the sandstone slab of that experiment for other materials. After subsequent interviews with Kelvin, Morley resolved to repeat the experiment, and secured the cooperation of Prof. Dayton C. Miller. With the aid of a grant from the Bache fund, an apparatus of pine was set up at the Case School of Applied Science. After its completion there was not time enough to finish the observations during the summer vacation. When another summer vacation came it was found that the apparatus had been subjected to such heat and dryness during the winter that its instability prevented observations. Accordingly a third apparatus was built of structural steel, in which the optical path could be limited either by the steel of the framework, or by pine distance rods determining the distance of the mirror holders.

With this apparatus it was found that if there were any portion of the results expected in the previous experiment it was not more than one one-hundredth of 1 per cent. Here again the result was practically negative.

In the later years of his life, in his private laboratory at West Hartford, Morley made about 70 analyses of igneous rocks that were collected by J. P. Iddings in the Malay Archipelago. It is hardly necessary to say that these difficult and complicated analyses were made with the greatest thoroughness, and that none better can be found in the whole literature of petrology. Nineteen of these analyses, of rocks from Java and Celebes, were published in a joint paper by Iddings and Morley. This was Morley's last contribution to chemistry. Morley was not a voluminous writer; his published bibliography contains only 55 titles. But a single great research may outweigh many small ones.

Morley was the recipient of many honors, among them three medals, namely, the Davy medal of the Royal Society, the Elliot Cresson medal of the Franklin Institute, and the Willard Gibbs medal of the Chicago section of the American Chemical Society. He had the degree of doctor of laws from Williams College, Western Reserve, Lafayette, and Pittsburgh; of doctor of

philosophy from Wooster, and of doctor of science from Yale. He was an honorary member of the Chemical Society of London, of the Royal Institution of Great Britain, and the only American honorary member of the American Chemical Society. Of the British Association for the Advancement of Science he was a corresponding member.

He was a member of the National Academy of Sciences, and he had served as president of the American Association for the Advancement of Science and also of the American Chemical Society. He also held membership in the American Academy of Arts and Sciences, the American Philosophical Society, the Washington Academy of Sciences, the Astronomical and Astrophysical Society of America, the German Chemical Society, and the French Physical Society. He was honorary president of the Eighth International Congress of Applied Chemistry.

Morley was an extremely versatile man and had many interests apart from his devotion to science. His early training was unusually broad, although as an investigator he was self-trained. In his college days he was much interested in philosophy as taught by the famous president of the college, Mark Hopkins. He was well read in general literature and a good amateur musician. In his manuscript memoranda he says that in boyhood he was so fond of music that he used to practice four hours a day, except on holidays, when eight hours could be used in the same manner. "It was," he says, "a severe trial when professional studies brought devotion to music to an end." For a few years after stopping daily practice, some enjoyment of his own performance on the piano was possible. For a good many more years he could enjoy performing on a cabinet organ. Then he had to await the improvement of the player piano up to the point where the performance was satisfactory. He neglects, however, to add that for a while he played the organ in the college chapel at Hudson. One of his last acts was to give a fund of $5,000 to the Congregational Church in Hartford in memory of his wife, whose death preceded his by only a few months. This money was for the purchase of an organ and in aid of the musical part of the church service. Morley was a religious man, but by no means bigoted or fanatical. In my more than 40 years' personal acquaintance with him I never heard him refer to his theological beliefs.

Morley's life at West Hartford was quiet and uneventful. In a letter to his college classmates, reproduced by Professor Tower, Morley says:

> In 1906, after teaching just forty years, I retired, hoping by timely rest from hard work to retain a fair degree of health and good spirits and power of enjoyment. I built a house and a small laboratory in West Hartford, and we are living there, seeming to find as much enjoyment as at any time in our lives. I grow a good many gladioli and use the camera. Walking and bicycling, which are a great delight, are now somewhat too strenuous; it is six years since I walked from North Adams over Greylock to Adams and took several similar walks in Berkshire County. Now the valleys of Berkshire must content me. My eyesight is still good; I wrote the Lord's prayer within the space covered by a three-cent piece a few months ago, without any magnifying glass. My hearing is not so good.

In this letter Morley fails to mention the pleasure he found in trips in his automobile, which he drove himself. Much lovely scenery was easily within his reach. Only the summer before he died he and Mrs. Morley took a long ride in his machine through northern Massachusetts into southern Vermont.

Professor Morley was a very modest man and by no means given to self-advertising. Consequently he was little known outside of scientific circles, but among chemists and physicists his reputation was world-wide. In his social relations he was rather diffident and made acquaintances slowly. But among his friends he was most companionable. Professor Tower says of him that he had—

> a remarkably retentive mind, so that practically everything that he read was stored in his memory, whence it could be drawn whenever needed. He not only possessed great clarity of expression in writing and speaking, but, what is rarer, he had the ability to present scientific and abstruse matters in a manner which made them interesting to laymen. His public lectures on such subjects as the ether-drift experiments were always well attended, and he held the attention of his audiences to the end.

Morley died, following a surgical operation, in the Hartford Hospital, at the ripe age of 85 years. He and his wife were both buried in the family plot at Pittsfield, the city in which his parents had passed their declining years. If his epitaph could be written in one word, that word might well be "Thorough."

The following bibliography was compiled by Professor Tower, the successor of Morley in the chair of chemistry at Adelbert College.

BIBLIOGRAPHY

On the latitude of Williams College Observatory. Proc. Amer. Assoc. Adv. Sci., 1866, p. 384.

An apparatus for rapid filtration. Amer. Journ. Science, vol. 6, 1873, p. 214.

On the chemical composition of a saline efflorescence occurring at Goat Island. Proc. Amer. Assoc. Adv. Sci., 1876, p. 132.

Measurements of Moller's diatomaceen-probe-platten. Read before American Association for the Advancement of Science. Monthly Microsc. Journ., vol. 15, 1876, p. 223.

Measurements of rulings on glass. Read before American Association for the Advancement of Science, 1876. Monthly Microsc. Journ., vol. 17, 1877, p. 137.

On the limits of accuracy in measurements with the microscope. Amer. Quarterly Microsc. Journ., 1878, p. 93. Read before Amer. Acad. of Arts and Sciences, 1879.

On a possible cause of variation in the proportion of oxygen in the air. Amer. Journ. Science, vol. 18, 1879, p. 168; vol. 22, 1881, p. 417.

Reduction to zero of measured volume of gases. Proc. Amer. Assoc. Adv. Sci., 1880, p. 345. Chem. News, vol. 45, 1882, p. 246.

The most convenient scale for a thermometer used in gas analysis. Proc. Amer. Assoc. Adv. Sci., 1880, p. 346.

Jolly's apparatus for determining the amount of oxygen in the air. Proc. Amer. Assoc. Adv. Sci., 1880, p. 348.

Some points in the construction of an apparatus for the accurate analysis of gases. Proc. Amer. Assoc. Adv. Sci., 1880, p. 350.

Numerical results for the mean ratio of oxygen to the sum of oxygen and nitrogen in atmospheric air. Proc. Amer. Assoc. Adv. Sci., 1880, p. 351.

Some conclusions as to the cause of the frequent fluctuations in the ratio of oxygen to nitrogen in the air at different times. Proc. Amer. Assoc. Adv. Sci., 1880, p. 353.

On Jolly's hypothesis as to the cause of the variations in the proportion of oxygen in the atmosphere. Amer. Journ. Science, vol. 22, 1881, p. 429.

Method for accurate and rapid analyses of air. Amer. Chem. Journ., vol. 3, 1881, p. 275; Chem. News, 1881 and 1882.

Special instructions relative to collecting specimens of air. Ann. Rept. U. S. Signal Office, 1881, p. 1285.

The amount of moisture which sulphuric acid leaves in a gas. Amer. Journ. Science, vol. 30, 1885, p. 140; Zeitsch. anal. Chem., vol. 24, 1885, p. 533.

Amount of moisture left in a gas after drying by phosphorus pentoxide. Amer. Journ. Science, vol. 34, 1887, p. 199; Proc. Amer. Assoc. Adv. Sci.; Zeitsch. anal. Chem., vol. 27, 1888.

On the determination of the atomic weight of oxygen. Amer. Chem. Journ., vol. 10, 1888, p. 21.

Determination of the tension of the vapor of mercury at ordinary temperatures. Proc. Amer. Assoc. Adv. Sci., 1890, p. 91.

Determination of the volumetric composition of water. Journ. Amer. Chem. Soc., vol. 12, 1890, p. 275; Proc. Amer. Assoc. Adv. Sci., 1890, p. 161; Amer. Journ. Science, vol. 41, 1891, pp. 220 and 279.

Ratio of the densities of oxygen and hydrogen. Proc. Amer. Assoc. Adv. Sci., 1890, p. 163.

Carbon as an impurity in hydrogen affecting determinations of its atomic weight. Amer. Chem. Journ., vol. 12, 1890, p. 460; Journ. Amer. Chem. Soc., vol. 12, 1890, p. 276.

A new method for measuring the expansion of solids. Proc. Amer. Assoc. Adv. Sci., 1891, p. 137.

Synthesis of weighed quantities of water from weighed quantities of oxygen and hydrogen gases. Proc. Amer. Assoc. Adv. Sci., 1891, p. 185.

Jean Servais Stas, Memoir. Journ. Amer. Chem. Soc., vol. 14, 1892, p. 173.

A self-acting mercuric pump. Amer. Journ. Science, vol. 47, 1894, p. 439.

On the volumetric composition of water. Proc. Amer. Assoc. Adv. Sci., 1895, p. 87.

On the atomic weight of oxygen. Syntheses of weighed quantities of water from weighed quantities of hydrogen, and oxygen. Amer. Chem. Journ., vol. 17, 1895, pp. 267 and 396; Zeitsch. physikal. Chem., vol. 17, 1895, p. 87.

On the densities of oxygen and hydrogen and the ratio of their atomic weights. Smithsonian Contributions to Knowledge, No. 980, 1895; Zeitsch. physikal. Chem., vol. 20, 1896.

Value of the atomic weight of oxygen computed from all available data. Western Reserve University Bull., 1895, p. 14.

A completed chapter in the history of the atomic theory. President's address, American Assoc. Adv. Sci., Proc., 1896, p. 1.

William Augustus Rogers, memoir. Biograph. Memoirs, Natl. Acad. Sci., vol. 4, 1902, p. 185.

Are further experiments needed for determining the atomic weight of oxygen? Journ. Amer. Chem. Soc., vol. 22, 1900, p. 51.

Note on the amount of moisture remaining in a gas after drying with phosphorus pentoxide. Journ. Amer. Chem. Soc., vol. 26, 1904, p. 1171; Journ. de Chim. phys., vol. 3, 1905, p. 241.

Alcoholometric tables. Journ. Amer. Chem. Soc., vol. 26, 1904, p. 1185.

Vapor pressure of mercury at ordinary temperatures. Amer. Journ. Science (4), vol. 18, 1904, p. 83; Phil. Mag. (6), vol. 7, p. 662; Zeitsch. physikal. Chem., vol. 49, p. 95.

Oliver Wolcott Gibbs, memoir. Proc. Amer. Phil. Soc., vol. 49, 1910, p. xix.

Fundamental chemical constants. Journ. Franklin Inst., Aug., 1912, p. 203.

PAPERS IN COLLABORATION WITH OTHERS

WITH PROF. ALBERT A. MICHELSON

Influence of the motion of the medium on the velocity of light. Amer. Journ. Science, vol. 31, 1886, p. 377. Also publ. in Nature, Beiblätter, and Repertorium.

On the relative motion of the earth and the luminiferous ether. Amer. Journ. Science, vol. 34, 1887, p. 333. Also publ. in Phil. Mag., Beiblätter, Journ. de Physique, and the Sidereal Messenger.

On the method of making the wave-length of sodium light the actual and practical standard of length. Amer. Journ. Science, vol. 34, 1887, p. 427.

On the feasibility of establishing a light-wave as the ultimate standard of length. Amer. Journ. Science, vol. 38, 1889, p. 181.

WITH PROF. HENRY T. EDDY

On the velocity of light in a magnetic field. Proc. Amer. Assoc. Adv. Sci., 1890, p. 81.

WITH PROF. WILLIAM A. ROGERS

Measurement of the expansion of Jessop's steel by a new method. Proc. Amer. Assoc. Adv. Sci., 1891, p. 138; Science, vol. 2, 1895, p. 35.

On the measurement of the expansion of metals by the interferential method. Physical Rev., vol. 4, 1896, pp. 1 and 106.

WITH PROF. DAYTON C. MILLER

On the coefficient of expansion of certain gases. Proc. Amer. Assoc. Adv. Sci., 1897, p. 123. This paper had to do with air, nitrogen, and carbon dioxide.

Another paper on "the coefficient of thermal expansion of oxygen" was read before another scientific society a year or two later.

Experiments on the velocity of light in a magnetic field. Phys. Rev., vol. 7, 1898, p. 283; Proc. Amer. Assoc. Adv. Sci., 1898, p. 123.

On the speed and accuracy of photographic shutters. Sci. Amer. Sup., vol. 55, 1903, p. 22979.

On the theory of experiments to detect aberrations of the second degree. Phil. Mag. (6), vol. 9, 1905, p. 669.

An experiment to detect the Fitzgerald-Lorentz effect. Phil. Mag. (6), vol. 9, 1905, p. 680; Proc. Amer. Acad. Arts and Sciences, vol. 41, 1905, p. 321.

WITH MR. CHARLES F. BRUSH

The conduction of heat through water vapor. Repts. British Assoc. Adv. Sciences, 1901, p. 525.

A new gauge for the measurement of small pressures. Amer. Journ. Science (4), vol. 13, 1902, p. 455.

WITH PROF. JOSEPH P. IDDINGS

Contributions to the petrography of Java and Celebes. Journ. of Geology, vol. 23, 1915, p. 231.

Harmon N. Morse

NATIONAL ACADEMY OF SCIENCES

Volume XXI
ELEVENTH MEMOIR

BIOGRAPHICAL MEMOIR HARMON NORTHROP MORSE

BY

IRA REMSEN

Presented to the Academy at the Annual Meeting, 1923

HARMON NORTHROP MORSE

Compiled by Ira Remsen

Harmon Northrop Morse was born and brought up among the Green Mountains in a most picturesque region of rugged Vermont. The Lamoille River flows about his father's farm and is full of wild beauty. Ancient forests clothe the mountains, and the clearest of brooks sparkle as they rush down the hillsides.

His earliest paternal American ancestor was John Morse, who came from England in 1639 and settled at New Haven.

His father, Harmon Morse, was a believer in hard work, few holidays, and little schooling. He looked upon all forms of recreation as objectionable. The death of the boy's mother when he was too young to remember her removed a much-needed gracious and loving influence. His brother Anson and his young sister Delia were comrades and comforters in his life, which for the most part lacked the elements of love and geniality.

The courage and ambition of the boy overcame all obstacles and difficulties. His maternal grandfather left a legacy whereby each of the three children was enabled to prepare for higher education, and thus Harmon was led to Amherst College, entering in 1869.

His passion for work and his keen and investigating mental processes dated back to his boyhood days, and were a heritage from his honored forefathers.

He was born at Cambridge, Vt., October 15, 1848, and died September 8, 1920. He was married December 13, 1876, to Caroline Augusta Brooks, of Montpelier, Vt. She died in 1887. He was married again, December 24, 1890, to Elizabeth Dennis Clarke, of Portland, Me. She survives him.

Having thus in brief outline stated a few of the main facts in the life of Professor Morse, it now seems best to let some of those who knew him best tell of him and his work. Shortly after his death, the compiler of this notice was asked to prepare a sketch for Science. This was published November 26, 1920, and is here reproduced. On Sunday, April 24, 1921, a meeting in memory of Professor Morse was held at the Johns Hopkins University. President Goodnow presided at the meeting. Addresses were made by former President Remsen, for many years a colleague and friend of Professor Morse; President Woodward, of the Carnegie Institution of Washington; Prof. J. C. W. Frazer, of the Department of Chemistry; and Dr. W. H. Howell, of the School of Hygiene and Public Health. As Doctor Remsen spoke without manuscript, the brief article from Science is substituted for his address. The addresses of the others named are given in full.

I. DOCTOR REMSEN'S ARTICLE FROM SCIENCE

After a long life spent in service in Johns Hopkins University, Professor Morse died September 8, in the seventy-second year of his age. He was born October 15, 1848, at Cambridge, Vt., the son of a farmer, and died at Chebeague, Me., where he had spent his summers for many years. He was graduated from Amherst College in 1873, then went to Göttingen, and received the degree of Ph. D. from that university in 1875. The year 1875–76 was spent at Amherst as assistant in chemistry. In 1875 it was announced that the Johns Hopkins University would begin its work in 1876. Shortly after it became known that the writer of this notice was to be the professor of chemistry in the new university he received a call from Morse, who brought a letter of introduction from Emerson. This letter led me to take more than ordinary interest in the bearer. Whatever we were to do in Baltimore, it seemed clear that I should need an assistant, and I told him I would in due time arrange for his appointment. Hearing a little later of the fellowships that were to be awarded, I secured one of these for Morse, and so his connection with the Johns Hopkins University began. Before the doors were opened, however, he was designated associate, and we began our work together for better or for worse. We had

no laboratory. We had less than a handful of students. What was to come of it? I need not go into the story thus suggested except to say that we were absolutely untrammeled and left to work out our own salvation. Morse and I were of one mind as to the object to be attained, and there were no discussions in regard to the methods to be adopted. They were not original, but they had never been tried in this country. There had never been an opportunity. The opportunity that many of us had hoped for, had dreamed of, was furnished by the bounty of Johns Hopkins and the wisdom of his trustees and of President Gilman.

Morse remained an associate until 1883, when he became an associate professor. In 1892 he was promoted to be professor of inorganic and analytical chemistry, and in 1908 he became director of the chemical laboratory. In 1916 he withdrew from active service and became professor emeritus.

From the beginning of our work in the new university the importance of research was emphasized. That was indeed its most characteristic feature. Morse was as anxious as any of us to take part in this work. For one reason and another it was some time before he got going. To be sure he did show his hand in some small and rather unpromising pieces of work, and I think he became discouraged, but he was faithful to his teaching. Gradually, however, his researches opened up new fields and he began their exploration. This is not the place for a full review of his contributions, and those of his last years so overshadowed all that preceded that a reference to those alone will do substantial justice to his memory.

In the early nineties he turned his attention seriously to the question of the stability of solutions of potassium permanganate, and in 1896 he published an article on "The reduction of permanganic acid by manganese superoxide," A. J. Hopkins and M. S. Walker appearing as joint authors. Pursuing this subject Morse and H. G. Byers in 1900 published an article "On the cause of the evolution of oxygen when oxidizable gases are absorbed by permanganic acid." The results were such that it became desirable to obtain an aqueous solution of pure permanganic acid. It was decided to prepare this by dissolving the heptoxide in water. In an article by Morse and J. C. Olsen that appeared in 1900 occurs the following passage:

> [We] accordingly prepared a quantity of the anhydride by mixing potassium permanganate and concentrated sulphuric acid in vessels cooled by ice and salt. We soon learned, however, that something more than a low temperature is essential to safety in handling the product; for a minute quantity of the anhydride—certainly less than half a drop—which had been separated from the sulphuric acid, exploded with great violence and with disastrous results to one of us.[1] Some idea of the force of the explosion may be gained from the fact that one of the flying fragments of glass passed entirely through a burette which was mounted in the vicinity, leaving holes over half the diameter of the burette, edges of which were entirely free from cracks. After this experience, we decided to abandon the anhydride as a source of the acid, and to work out, if practicable, an electrolytic method of separating it from its salts.

The electrolytic method worked very satisfactorily, and led to the further use of this method in the preparation of osmotic membranes. The first results of this investigation are given in an article by Morse and D. W. Horn that appeared in 1901. They say:

> In this connection, it occurred to the authors that if a solution of a copper salt and one of potassium ferrocyanide are separated by a porous wall which is filled with water, and a current is passed from an electrode in the former to another in the latter solution the copper and the ferrocyanogen ions must meet in the interior of the wall and separate as copper ferrocyanide at all points of meeting, so that in the end there should be built up a continuous membrane well supported on either side by the material of the wall. The results of our experiments in this direction appear to have justified the expectation and to be worthy of a brief preliminary notice.

This marks the real beginning of the work on osmotic pressure with which the name of Morse will always be associated. But before the cells were available and therefore before any reliable measurements could be made, years of patient, skillful work were still necessary. Difficulties that seemed insurmountable frequently arose and necessitated new efforts. It must be said that some of us in the laboratory, including myself, at times lost faith in the ultimate success of the work and were perhaps inclined to advise the use of cells that were not perfect. But Morse went steadily on. He had in mind a practically perfect cell that could be used for

[1] To make this story complete it should be added that Morse was the "one of us" here referred to. A piece of glass passed through the tissues of his neck in close proximity to the jugular vein. His escape from death was almost miraculous.

high pressures as well as low. He tried all sorts and conditions of clay and after many, many discouragements he succeeded in finding one and in making a satisfactory glaze quite different from any available, and he achieved success.

In 1902 he and J. C. W. Frazer described "The preparation of cells for the measurement of high osmotic pressures." A careful reading of this article will give some idea of the tremendous difficulties that were met and overcome. The closing paragraph may be advantageously quoted in this connection:

The difficulties of construction are by no means completely overcome, and we have in view a number of changes which we hope will prove of advantage. That these difficulties are of great magnitude will be realized if one considers that in our last experiment the pressure which was measured and which was still below what we were called upon to control would suffice to raise a column of water at 20° to a point 15 meters higher than the top of the Eiffel Tower, or which would raise from its base a marble shaft whose height is 120 meters. These comparisons will perhaps make it clear that the most painstaking attention to every detail of construction is absolutely essential to success when an apparatus like ours is to be made up of several parts, consisting of different materials, and which must be united without the usual mechanical means of securing strong joints.

Soon after this the Carnegie Institution of Washington lent its powerful aid to the large investigation thus begun. In 1914 the institution published a memoir entitled "The Osmotic Pressure of Aqueous Solutions: Report on Investigations made in the Chemical Laboratory of the Johns Hopkins University during the years 1899–1913. By H. N. Morse." In it is given a detailed account of this remarkable piece of experimental work. Anyone who reads it understandingly will recognize that no one but a master of experiment could have done this. The work required the highest degree of resourcefulness and skill, of patience and persistence. Anyone of ordinary caliber would have stopped short of the accomplishment. Morse was not satisfied with anything but perfection as nearly as this could be reached, and as it never can be reached, he worried about the residual, no matter how small it might be. In the concluding chapter of the Carnegie memoir occur these words:

The work reported upon in the preceding chapters is only a fraction of the task which the author hopes to accomplish, or to see accomplished by others. The investigation—already 15 years old—was undertaken, in the first instance, with a veiw to developing a practicable and fairly precise method for the direct measurement of the osmotic pressure of aqueous solutions. The need of such a method for the investigation of solutions seemed to the author very great and very urgent.

Honors came to him rather late, but they came; the chief among these was the award of the Avogadro medal of the Turin Academy of Sciences, in 1916.

In 1911 an international congress of scientists assembled at Turin, Italy, to celebrate the centennial of the announcement of the hypothesis of Avogadro. Those in attendance decided to award a medal to be known as the Avogadro medal. This medal was to be awarded to the investigator who should, in the judgment of the awarding committee, make the most valuable contribution to the subject of molecular physics during the years 1912, 1913, and 1914.

A few words in regard to Morse, the man. He was quiet and uneffusive. He did not care for the ordinary intercourse with his fellowmen. He lived, when not in the laboratory, for his family and a few kindred spirits. He married twice and had four children—a daughter and three sons. His second wife, who was Miss Elizabeth Dennis Clarke, of Portland, Me., his daughter, and two sons survive him. In his later years his wife was of great assistance to him in preparing his articles for publication and was a true helpmate in every way.

For many years he spent his summers at Chebeague, in the beautiful Casco Bay. Here he had a simple comfortable cottage and garden. He delighted to work, both in and out of the house, and this gave him his exercise. He was rather stout and he knew that he needed exercise to keep his weight down. He therefore indulged in walking, bicycling, and finally in motoring, and he managed to keep fairly well. But after his retirement in 1916 his health failed. His strength gave out and his courage also. He did not dare to take his car out of the garage, and his walks were very short. I saw him in May, just before he went to Maine, and thought he seemed more like his old self. He even talked of taking up his work again. It was not to be. I heard nothing from him after that. And then came the dispatch announcing his rather sudden and entirely unexpected death. He was buried at Amherst, a place that meant so much to him—where he had spent his college years and for some time had had a summer home.

II. PRESIDENT WOODWARD'S ADDRESS

When a man of distinction in science closes his terrestrial career and passes over to one of the older planets, or possibly to one of the hotter stars, of the universe, his demise gives rise, in general, to few reflections and to few regrets. The best that the public can say of him is that he left none of the memories of mischief which constitute what Doctor Johnson called "no desirable fame." But this is because the world at large, learned and unlearned, does not understand him rather than by reason of any disposition to underrate his motives or his achievements. In proportion as his work has been advanced or recondite, it will be difficult to understand, and in about the same proportion the rewards he receives will be mostly posthumous. The unlearned of his contemporaries will call him a "high brow," while the majority of the learned, in our day, at any rate, will call him a "narrow specialist," and let him go at that.

It thus happens that the discoveries and the advances of any age, are, as a rule, adequately understood and utilized only by succeeding ages, and that the originators of such discoveries and advances are oftenest unknown and hence unappreciated except by a very limited number of fellow specialists working in the same or in adjacent fields of research. Not infrequently the pioneer work of these originators is either overlooked, forgotten, or attributed to others. Hence we have patent laws and patent offices to determine priorities and rights in cases of inventions, and medals and prizes and a "Hall of Fame" to bestow belated honors on our eminent contemporaries and predecessors.

But while these ex post facto devices have the merit of providing means for inductions based on all evidence available, they generally fail to afford the public any adequate recognition either of the nature of the work commended or of the methods by which it was accomplished. Such pioneer achievements are still, even in this enlightened age, commonly attributed not to foresight, industry, persistence, and the utilization of a long line of mistakes and successes of our predecessors, but to the vague discontinuities of supermen and of miraculous conditions. In respect to the real place in civilization to be assigned to constructive thinkers and in respect to the need of such for progress, we have still almost everything to learn. The truth of this apparently dogmatic assertion is well illustrated by the ease with which the populace is now led to entertain the notion that Euclidean geometry and Newtonian dynamics may be displaced summarily by a highly praiseworthy "theory of relativity" whose author makes no pretensions to such revolutionary sentiments.

It is specially fitting at this time, therefore, that your university should hold a conference in commemoration of the life, the character, and the accomplishments of one of her most devoted and most productive investigators. His career exemplifies well the singleness of purpose and the arduous labors essential to progress in the realm of learning in general and in the domain of physical science in particular. He was a typical man of science. His interests, like those of the German chemist, Becher, of the seventeenth century, lay among the "flames and the fumes," and if need be, among the "poisons and the poverty" of the laboratory. Becher lived in an age when chemistry was slowly emerging out of alchemy, but what he said of himself was doubtless often thought, if not said, by Morse and by many of our contemporaries. In his physica subterranea Becher says, "My kingdom is not of this world," referring, of course, to those who would in his day, as in ours, measure everything by the gold standard. And of the alchemists, whose prototypes are still to be reckoned with, he says, "Pseudo-chemists seek gold, but the true philosophers, science, which is more precious than any gold."

Such undoubtedly were the ideals that animated Morse in his career as a chemist, as a teacher, and as an investigator; but he was not a man who would render his ideals or his activities obtrusive in comparison with, or in competition with, the interests of men in other fields of learning. He possessed in high degree that sort of modesty and that sort of reserve which are born of a knowledge of men and things, including especially among the latter the obstinate but constant and determinate properties of matter, with which the chemist and the physicist have more particularly to deal.

Of his vocations as a chemist, as a teacher, and as an investigator, others, who knew him more intimately, are better qualified to speak. My impressions of him were formed, unfortunately for me, during the last 15 years of his life and then only in the capacity of a distant administrator. What impressed me most from the inception of acquaintance with him was his tenacity of purpose. He had a problem to solve and he was willing to go to any extent of time and effort to reach an effective solution. This attitude, it may be remarked, affords one of the surest tests of the productive investigator. He who wanders, or vacillates, or lacks capacity to concentrate attention on a limited range of phenomena, is almost certain to become lost in a maze of futilities. The impression gained of him as a teacher was that he would probably "neglect" his students. But if this was the case, it must have been, as with Rowland and with Mall, a great privilege for the students. To be permitted to "stand around" in the presence of evolving knowledge is the highest opportunity a university can offer and the greatest favor a worthy student may seek. The best teachers are not those who think most for their students, but those who make the students do their own thinking. In the higher work of a university, at any rate, it is essential that the novitiates learn early to use their own heads.

The special work of Morse with which it was my good fortune to become somewhat acquainted was his research on the osmotic pressure, carried on by aid of grants made to him by the Carnegie Institution of Washington. What is called osmose, or osmosis, is a subject beset by technicalities, but its elementary essentials are easily apprehended. When two liquids or gases are separated by a common membrane, there is manifested a tendency to transference from one side to the other through the membrane; and if the liquid or gas to which the transfer takes place is confined, an increase of pressure will result, and this under some circumstances may be not merely appreciable but very great. A homely illustration of osmose and osmotic pressure is afforded in the preparation of cranberry sauce. If osmose is permitted to act, the result will be a good sauce; if osmose is prevented the result will be a bad sauce. Osmose follows slowly if the berries are immersed in a hot solution of sugar; meantime the skins will partake of the general dissolution and become edible with the rest of the gelatinous mass. On the other hand, if the berries are boiled and stirred violently, the well-known inedible product follows.

Briefly stated, the research to which Morse made a capital contribution in this field was that of determining what, for a given membrane and for a determinate range of solutions and of attending temperatures, are the pressures generated. To this research he brought a degree of patience, persistence, and continuity worthy of the highest praise, and by its prosecution to definite conclusions he won for himself a place among the masters in experimental physics. Just as we look with admiration, for example, on the early work in optics of the Arabian physicist and mathematician, Alhazen, so the world will regard with admiration the man who first measured with precision the far more difficult data leading to definite knowledge of osmotic pressures.

All researches best worth while in physical science are beset by obstacles which try the souls of investigators. Most of their time and effort are required, usually, in surmounting these obstacles. This was the case with Morse. He needed a uniform, stable membrane, capable of withstanding repeated pressures of many atmospheres. He was led to use a porous, earthenware cup as a matrix for the electrolytic deposition of metallic salts which furnished the required uniformity of porosity. But it turned out that the potters could not make a cup of sufficiently uniform texture and of sufficient strength to stand the pressures developed. Hence Morse had to apply his science to the art of pottery and learn how to select, to sift, to compress, and to burn clay. This was a task that consumed the greater part of his time for about a decade. But while this was the greatest of his difficulties, it was only one of them. This may suffice here, however, to indicate that the tenacity of purpose already referred to was an indispensable requirement to the success of his project. Baffling and discouraging as was his early experience in this work, Morse never rested until he completed a well-rounded and definite chapter which must be considered the first great classic on the experimental side in the field of osmotics.

It is interesting and instructive to reflect that this field is only one of the numerous fields in the domain of the doctrine of atomism. This doctrine was foreshadowed about 2,000 years ago by the philosophers Leucippus and Democritus and by the poet Lucretius. It has grown astonishingly along with the developments of modern physical science, especially since the advent of the atomic theory of Dalton and the advent of the electrochemical theories of Davy and Faraday. It has now reached the very advanced stage of a complete overthrow of the doctrine of continuous media, a doctrine much alive also 20 centuries ago, and finding its modern Anaxagoras, in this university, in no less a personage than Lord Kelvin, who, as some of you will remember, maintained the continuity of that something we call the ether in his famous "Baltimore lectures" of the year 1884. The structure of matter now seems to have been proved to be plinthoid, and attention is at present concentrated on the individual bricks, the numbers of them per unit volume, and the arrangement of the corpuscles, or subbricks, in them. The contribution of Morse was immediately recognized as a part and parcel of the grand aggregate of evidence in favor of the doctrine of atomism; and it was not a matter of surprise to those acquainted with the subject that the Turin Academy of Sciences awarded him the Avogadro prize on the occasion of the celebration of the one hundredth anniversary of the promulgation of what has since been known as Avogadro's hypothesis, namely, that equal volumes of different gases, subject to the same pressures and temperatures, contain the same numbers of molecules.

The dignified directness, simplicity, and sincerity of Professor Morse were agreeably manifested in the correspondence had with him in reference to his work and its support. He had always a just sense of realities. His enthusiasm and his optimism were always tempered by a recognition of existing conditions and limitations. Although not a professional mathematician, he understood well the meaning and the rigor of the much-neglected rules of arithmetic. His characteristics as a man among men are clearly indicated by himself in the following self-explanatory letter, written in his own plain hand, as were most of his communications—it is dated February 29, 1916:

> I have just received from the Accademia delle Scienze di Torino the announcement that the medal provided for at the centennial celebration of the promulgation of Avogadro's Hypothesis, for the best work in molecular physics which should appear in the three following years, i. e., during 1912, 1913, and 1914, has been awarded to my report to the Carnegie Institution on investigations in osmotic pressure.
>
> I hasten to inform you, because I am glad to have justified the confidence you have shown in the work and the liberal support you have given it, without which it would have been impossible for me to have succeeded.

But it should be understood that Morse was not working for medals, or for prizes, or for the approval of learned societies. That the first chapter of his enterprise was completed in time for consideration by the Turin Academy was only a happy coincidence. His zeal and industry were founded in the more enduring sentiments derived from contemplative studies of the properties of matter. He sought to add, and did add, to that sort of knowledge which is verifiable and hence permanently useful to our race. His position in science is therefore secure, for it is written in with the history of the demonstrated constancy of the material phenomena he helped to penetrate, and these phenomena are more enduring than the works of men.

III. PROFESSOR FRAZER'S ADDRESS

My remarks this afternoon are dictated by the intimate association I enjoyed with Professor Morse, extending over a long period of years and covering the time of his greatest scientific productivity. I came to know him as teacher, patient and painstaking, as friend, staunch and self-forgetful, and, lastly, as investigator, resolute and resourceful.

In these circumstances I may be permitted to speak more particularly of the work which has placed the name of Morse so high among the scientists of his time, the work with which his name will always be associated.

Although Professor Morse was primarily an investigator, he devoted long years of his life to routine instruction. His extensive knowledge of the facts of chemistry and his habit of careful individual instruction made the work in his laboratory extremely valuable. The

personal contact which he always established with his students, together with his genial disposition and sympathetic attitude, at once established a spirit of friendly cooperation on the part of the student. Most of his students undoubtedly look back on these daily visits in the laboratory as among their most pleasant and profitable experiences while at the university.

When the work on osmotic pressure began Professor Morse was engaged in an extensive investigation on permanganic acid. In the electrolytic method, which he devised for the preparation of this substance, a solution of potassium permanganate was electrolyzed between electrodes separated by a porous clay vessel. At times the pores of this vessel became filled with finely divided manganese dioxide which was formed by the decomposition of the permanganic acid. When in this condition the cells frequently showed slight osmotic activity. This accidental formation of an osmotic cell furnished the idea that the artificial, semipermeable membrane of Traube as used by Pfeffer could be deposited electrically and advantage taken of the great driving force of the electric current to bring up a strong, continuous, semipermeable membrane. Little difficulty was encountered in putting this idea into practice, and in 1901 a brief account of this ingenious method was published.

Subsequent work showed that even such a perfect method for the formation of the membrane could not give a perfect osmotic cell unless the porous clay vessel used for its support was of the required texture. At first it was not believed that the matter of the porous wall would be a difficult part of the problem of making an osmotic cell; it was thought that the production of a suitable porous vessel could be left to the potter, with such instruction as seemed necessary as to what was desired. After an experience of more than a year spent in testing the products of several potteries, it was realized that if solved at all the problem must be taken into the laboratory and a careful scientific study made of the conditions influencing the texture of the product. The efforts of the potter were not, however, complete failures; a few of the first lot of cells submitted, while not perfect, were quite good and served a most valuable purpose, as I shall point out. Up to this time the direct measurement of osmotic pressure of any magnitude was considered an experimental impossibility; but with the best of these cells of the potter a number of measurements were made of the osmotic pressure of sucrose solutions, thus demonstrating the possibilities of direct measurement. Unfortunately all of these cells were broken in attempting to extend the work beyond the strength of the cells to withstand pressure. These first cells of the potter served a second purpose also. By a microscopic study of sections of these cells the desired texture was ascertained, and this information was of considerable aid in directing the course of the experiments when the production of a suitable osmotic cell was made a laboratory problem.

About two years were spent in continuous efforts to produce suitable osmotic cells. At the end of that time it seemed that every possible precaution had been taken to secure success, but failure was the result. But, with the knowledge that the potter had on one occasion succeeded quite well, efforts were continued until encouraging results were obtained; the first stone was loosened and the wall was then easily breached. I shall not attempt to give any of the details of these difficulties. What was constantly in mind during these disappointing years was a perfect osmotic cell; this vision of perfection guided and sustained the efforts in the laboratory until, as in the case of the potter of old, all that was foreseen was either found or created.

After having perfected a method for the deposition of the membrane and worked out in detail the method for the production of a suitable cell, the remaining difficulties, which were largely of a mechanical nature connected with assembling the various parts of the cell, were rather easily overcome. And so, step by step, the obstacles were removed, until after a period of about two years two good cells were finally produced in the laboratory and served to make the first quantitative measurements, and the accumulation of data began. This brief statement will give some idea of the difficult nature of the work. At the present time it requires about three months to get an osmotic cell into measuring condition, and even then it must be given a considerable period of rest between experiments, as it is disastrous to attempt to use a cell too frequently.

Before the work of Professor Morse began, the importance of osmotic pressure had been very generally recognized. Not only in chemistry, where it is of fundamental significance, but also in allied sciences, such as botany and physiology, the importance of osmosis in connection with the motion of the fluids in living tissues was clearly recognized. In fact, its importance in these connections was recognized before its importance to chemistry, and we owe a great deal of the early experimental work on osmotic pressure to investigators working in these fields; but their results were either indirect or covered a very limited range and had little claim to accuracy. So, for years, science had stood before this closed door of knowledge waiting for some one skillful and resourceful enough to gain entrance. This situation left no room to doubt that the one who could succeed in overcoming the experimental difficulties in the way of further progress would perform a valuable service to many branches of science. For this reason one may, at the present time, appraise the scientific contribution of Professor Morse at its true value without any of the uncertainty that so frequently attends the consideration of work so soon after its completion. I shall limit further reference to the importance of this work to chemistry alone, since this was the field of Professor Morse's labors.

The importance of osmotic pressure to chemistry and physics was first pointed out by Van't Hoff in 1885. Van't Hoff showed the quantitative relation that exists between any of the colligative properties of solutions, and these relations and the closely connected theory of electrolytic dissociation of Arrhenius form our present theory of solutions and to a large extent modern theoretical chemistry as well. The introduction of these theories has so completely changed the character of chemical instruction and has been so fruitful in the field of research that it may be truly said that for a large part of the intervening time chemistry has lived upon these ideas. Although the theory of Van't Hoff is based on thermodynamic reasoning, the almost complete lack of experimental evidence on the subject of osmotic pressure so weakened his chain connecting these fundamental properties that many chemists of the older school hesitated to trust it. Briefly, the work of Professor Morse on osmotic pressure was to forge the last link of this chain of experimental evidence and by so doing perform a valuable contribution to theoretical chemistry.

The scientific career of Professor Morse is unusual in that his most important contributions came so late in his life. At the time when others seek to lay aside their burdens and rest, he was striking his most telling blows, and not until his day was far spent and its shadows lengthening did he succeed in completing his chapter of science to his own satisfaction.

This work on osmotic pressure is a model of experimentation which in American research has in some respects a counterpart in the work of Morley on the densities of oxygen and hydrogen. Both are large pieces of experimental research of a fundamental nature extending over many years, both involved overcoming numerous experimental difficulties, and both resulted in giving to the world data of an unexpected excellence.

The work of Professor Morse on osmotic pressure must, therefore, remain one of the brilliant contributions to American chemistry, a precious heritage of our university, an inspiration to those who follow, and a perpetual monument to his memory.

III. DOCTOR HOWELL'S ADDRESS

I had the good fortune to know Doctor Morse with increasing intimacy through many years; from the time that I was a student in his classes in 1880 until his death last summer. My acquaintance with him during this time passed through several stages; the relation of teacher and student, of a friend and colleague, and finally that of a near neighbor for some 20 years during the long pleasant summer vacations—each of these periods gave me a new point of view in regard to his personal qualities; and while I am not qualified to speak as a specialist in reference to his scientific work, it is a pleasure and a privilege to express in a few words my great respect and admiration for him as a man, and my appreciation of the important part that he took in establishing the reputation of this university as a center of scientific research.

My student impressions of him were quickly formed and gauged accurately, I believe; some of them characteristics which made him so eminently successful in his scientific work. The

impressions of that period are still very vivid in my memory. I entered one of his classes in quantitative chemical analysis. An enthusiastic young student, I was eager to push ahead as rapidly as possible. When I had repeated a certain method of analysis two or three times I felt that I had got about all there was of value in the procedure and proposed to go on to something new, but my eyes were soon opened to the error of my ways. The big kindly soft-spoken man to whom I reported my work gave me to understand very gently but very firmly that approximate results did not suffice—I was to do the work over and over again until exact and consistent findings were obtained. I recall that upon one occasion, after spending my entire Christmas vacation in a futile attempt to ascertain the composition of a given mineral, all of my tests turning out completely negative, Professor Remsen, in one of his daily walks through the laboratory, was kind enough and indiscreet enough to drop a hint about certain of the rarer metals which finally put me upon the right track. After several experiences of this kind I felt that my position was much the same as that of Peter when he asked how often he should forgive his brother, mentioning seven times as a sort of outside limit. He got the reply, you will remember, that if necessary he must forgive seventy times seven. That was about the kind of admonition that I received from Professor Morse. Then and later I found that accuracy and thoroughness were the underlying principles of his nature and the principles which he most sought to inculcate in his students. Neither trouble nor time weighed much in the balance against these virtues, and certainly he did not spare himself or his pupils in his effort to obtain the greatest possible perfection in methods and in results. Difficulties did not discourage him, in fact it seemed to me that they rather attracted than repelled him; for gentle as he was in mood and in manner, there was nothing soft or yielding in his character. Determination and inflexibility of purpose were among his conspicuous qualities, and his colleagues know well how greatly these characteristics served him in the difficult problems that he undertook to solve. It is probable that as a teacher he was not especially well suited to the average student. He did not possess or had not cultivated that specious art of beguiling the careless or indifferent student into a love for his subject. His methods were sober and serious, and for those who were in earnest and had a definite end in view he was a great teacher. They got from him the kind of training that leaves a permanent impression throughout life. This is the testimony that I have heard from not a few of his students who have since achieved distinction as scientific workers. He was a teacher for the few, not for the many, and those pupils of his who afterwards became investigators themselves will always cherish a grateful remembrance of the benefits they derived from his example and his instruction. When I came to know Doctor Morse as a colleague I had frequent opportunities to discuss with him general scientific questions and educational policies, especially as they affected this university. The dominant impression that I recall from these conversations is his high ideals in regard to scientific research. He had much of the common sense and practical ingenuity which we are accustomed to consider as inherent in the New England stock. But long and close association with university life had developed a sincere appreciation of the value of fundamental work in science. He had absorbed, and to an important extent had helped to create, that fine spirit of research which was the chief glory and distinction of the university. It is not easy to describe this spirit in words. All research that is sincere and well planned is good and useful, whether its purpose is to discover new truths or to devise methods of applying knowledge to the benefit of mankind. But there is one glory of the sun, and another glory of the moon, and every one must admit, I believe, that in this matter of research the greater glory belongs to him who pursues knowledge for knowledge's sake. Unless this spirit is in a man he is not fitted for the higher and more difficult tasks of discovery. And just because research of this kind is not valued by the majority, it is important that the few who realize its worth shall be steadfast in its support. This was Doctor Morse's attitude. He did not of course undervalue utilitarian investigations; on the contrary, he placed a great value upon them as any sensible person must do, but his point of view was that the more fundamental research which serves to advance our theoretical knowledge is the kind that should be especially fostered in a university.

Toward the end of his life he showed some signs of discouragement with existing conditions in the universities. So much so, in fact, that he was inclined to advise his young men to seek

positions in the industrial laboratories rather than in the universities. I am not sure that I understood fully his reasons for taking this somewhat paradoxical attitude. Possibly it was of the nature of a protest against what he considered the inadequate opportunities and compensation offered to young men in the universities; possibly he felt, with some others, that the industrial and special laboratories, in some subjects at least, are offering the best opportunities in theoretical as well as practical research, and that young men of ambition and promise may look to them with more confidence for that substantial support and encouragement which work of this kind must have to insure its proper development.

The good chance that made me his neighbor for many years on an island on the coast of Maine during the summer vacations gave me abundant opportunities to discover and to appreciate his many sterling and lovable personal characteristics. I found that beneath his quiet and somewhat stern exterior there was a warm heart, an active emotional nature, and a great love of humor. Those whose acquaintance with Doctor Morse was only incidental or official must have gained the impression that he was an extremely reserved man. In a general gathering he had little to say as a rule. The rapid-fire exchange of question and comment did not suit his deliberate temperament, and he was likely under such conditions to remain in the background as a quiet listener. But in a small company of intimates he could be a most delightful companion, both entertaining and instructive. On suitable occasions he had many good stories to tell, dealing mostly with the human frailties of the older natives of the island. The point of the story was always brought out with a reminiscent chuckle or a good hearty laugh which showed his own enjoyment in the recollection, and expressed also perhaps his sympathetic realization of those touches of nature that make us humans all akin. When the conversation turned upon more serious topics he displayed a remarkable fund of accurate information gathered from his wide experience and extensive reading. When others guessed or spoke vaguely and uncertainly, he was sure to have some precise and authentic knowledge. His interest in matters pertaining to the progress and welfare of the country, especially in political and social affairs, was real and warm. They were not for him simply matters of reason and judgment, they penetrated deep into his emotional nature. While his manner and mode of expression were judicial and conveyed the impression of a coldly rational temperament, experience led me to realize that beneath the surface there was that kind of emotional heat that makes a loyal partisan. He was a man who took sides on important questions, and once he had made up his mind he could maintain his position with a granitelike firmness against which arguments had little effect. In our estimates of men and affairs we differed sometimes toto caelo, and in the discussions that ensued I rarely had the satisfaction of seeing any of my chance arguments penetrate the joints of his armor. But it is a pleasure to remember that our discussions never became heated or bitter, for he knew how to differ in his opinion in a courteous and considerate manner. It was in fact a great pleasure and inspiration to talk matters over with him, whether we agreed or disagreed, because of the fine and sturdy patriotism he exhibited under all circumstances. His scientific interests did not prevent him from following minutely all the movements and tendencies of the times, and I was often surprised to find in place of the ultraconservatism that one might have expected to encounter in a man of his type, a marked degree of modernism. So far as his country and his science were concerned, he was always on the side of expansion and progress.

Outside his reading his main occupation and recreation in summer was the care of his garden. Into this work and play, for it was both to him, he carried the same spirit of unusual thoroughness that was so characteristic of his scientific experiments. His materials and tools must be of the best quality and all the processes of leveling, weeding, planting, and transplanting were carried out with a degree of perfection that excited general comment in our small neighborhood. It was well understood in that locality that anyone who did work for him was expected to measure up to a very high standard of performance. No matter how small the undertaking, it was planned with a singular degree of completeness, for he cordially disliked anything of the nature of a makeshift or a temporary expedient. Nature was not always kind to his agricultural experiments. Between the rigors of the climate and unexpected acts of

Providence they encountered many serious setbacks, but in this case, as in his scientific work, opposition and misfortune served simply to stimulate him to renewed effort. If one scheme failed he devised another more complete, and usually, so it seemed to me, more difficult of performance. Temporary failures seemed to act as challenges to his resourcefulness and determination, and I am confident that he experienced a real joy in those contests with nature.

Science needs for its continued progress talents of many kinds, insight and inventiveness, enthusiasm, wide knowledge, a high degree of experimental skill, and many other of the best qualities, but perhaps no gifts are more essential than exactness and thoroughness. Through them advancement is made certain; few backward steps must be taken. It was in these qualities that Doctor Morse was preeminent. The work that he did was exceedingly well done, so that other men might build upon it with confidence. By the exercise of these talents he was able to contribute to the science of chemistry knowledge of lasting value, and it is pleasant to remember that for this work he received the highest reward that a scientist can hope to obtain—I mean the sincere gratitude and applause of his fellow specialists

APPENDIX

INVESTIGATIONS CARRIED OUT UNDER THE DIRECTION OF H. N. MORSE

Morse, H. N. Ueber eiue neue Darstellungsmethode der Acetylamidophenole. Ber. d. chem. Gesell., **11**, 232 (1878).

——— On the determination of barium as chromate. Amer. Chem. Journ., **2**, 176 (1880).

Morse, H. N., and Day, W. C. Determination of chromium in chrome iron ore. Amer. Chem. Journ., **3**, 163, (1881).

Morse, H. N., and Bayley, W. S. Haydenite. Amer. Chem. Journ., **6**, 24 (1884).

Morse, H. N., and Keiser, E. H. A simple apparatus which enables students to determine the equivalents of certain elements. Amer. Chem. Journ., **6**, 347 (1884).

Morse, H. N. A method of determining the value of zinc dust. Amer. Chem. Journ., **7**, 52 (1885).

——— Two forms of apparatus which facilitate the correct reading of gas volumes over water. Amer. Chem. Journ., **7**, 58 (1885).

——— An apparatus for the purification of mercury by distillation in a vacuum. Amer. Chem. Journ., **7**, 60 (1885).

Morse, H. N., and Burton, W. M. A method for the determination of butter in milk. Amer. Chem. Journ., **8**, 222 (1886).

Morse, H. N., and Linn, A. F. The determination of nitric acid by the absorption of nitric oxide in a standard solution of permanganate of potassium Amer. Chem. Journ., 8, 274 (1886).

Morse, H. N., and Piggot, C. A method for the determination of butter in milk. Amer. Chem. Journ., **9**, 108 (1887).

Morse, H. N., and Burton, W. M. On the supposed dissociation of zinc oxide and the condition of the atmosphere within a platinum vessel heated by a gas flame. Amer. Chem. Journ., **10**, 148 (1888).

——— A method for the separation and determination of boric acid. Amer. Chem. Journ., **10**, 154 (1888).

——— The atomic weight of zine as determined by the composition of the oxide. Amer. Chem. Journ., **10**, 311 (1888).

——— The removal of iodate from the iodide of potassium by means of zine amalgam. Amer. Chem. Journ., **10**, 321 (1888).

——— A method for the analysis of butter, oleomargarine, etc. Amer. Chem. Journ., **10**, 322 (1888).

Morse, H. N., and White, J., Jr. The dissociation of the oxides of zinc and cadmium in the vapors of their respective metals. Amer. Chem. Journ., **11**, 258 (1889).

——— The dissociation of the sulphides of cadmium and zinc by means of metallic cadmium and zinc. Amer. Chem. Journ., **11**, 348 (1889).

Morse, H. N., and Jones, H. C. On the action of metallic cadmium on the halogen salts of cadmium, and on the sub-hydroxide and sub-oxide of cadmium. Amer. Chem. Journ., **12**, 488 (1890).

Morse, H. N., and White, J., Jr. The dissociation of magnesium oxide by means of metallic magnesium. Amer. Chem. Journ., **13**, 128 (1891).

Morse, H. N., and Jones, H. C. A redetermination of the atomic weight of cadmium. Amer. Chem. Journ., **14**, 261 (1892).

Morse, H. N., and White, J., Jr. The transportation of solids in a vacuum by the vapors of metals. Amer. Chem. Journ., **14**, 314 (1892).

Morse, H. N., and Blalock, T. L. Instruments for the graduation and calibration of volumetric apparatus. Amer. Chem. Journ., **16**, 479 (1894).

Morse, H. N., and Chambers, A. D. A method for the standardization of potassium permanganate and sulphuric acid. Amer. Chem. Journ., 18, 236 (1896).

Morse, H. N., Hopkins, A. J., and Walker, M. S. The reduction of permanganic acid by manganese dioxide Amer. Chem. Journ., 18, 401 (1896).

Morse, H. N., and Arbuckle, H. B. A redetermination of the atomic weight of zinc. Amer. Chem. Journ., **20**, 195 (1898).

Morse, H. N., and Reese, C. L. A reduction of permanganic acid by manganese peroxide. Amer. Chem. Journ., **20**, 521 (1898).

Morse, H. N., and Arbuckle, H. B. The atomic weight of cadmium. Amer. Chem. Journ., **20**, 536 (1898).

Morse, H. N., and Byers, H. G. On the cause of the evolution of oxygen when oxidizable gases are absorbed by permanganic acid. Amer. Chem. Journ., **23**, 313 (1900).

Morse, H. N., and Olsen, J. C. Permanganic acid by electrolysis. Amer. Chem. Journ., **23**, 431 (1900).

Morse, H. N., and Horn, D. W. The action of carbon dioxide on the borates of barium. Amer. Chem. Journ., **24**, 105 (1900).

Morse, H. N., and Horn, D. W. Preparation of osmotic membranes by electrolysis. Amer. Chem. Journ., **26**, 80 (1901).

Morse, H. N., and Frazer, J. C. W. The preparation of cells for the measurement of high osmotic pressures. Amer. Chem. Journ., **28**, 1 (1902).

Morse, H. N. New osmotic membranes prepared by the electrolytic process. Amer. Chem. Journ., **29**, 173 (1903).

Morse, H. N., and Frazer, J. C. W. A new electric furnace and various other electric heating appliances for laboratory use. Amer. Chem. Journ., **32**, 93 (1904).

Morse, H. N., and Taylor, L. S. An electrical method for the combustion of organic compounds. Amer. Chem. Journ., **33**, 591 (1905).

Morse, H. N., and Frazer, J. C. W. The osmotic pressure and freezing-points of solutions of cane sugar. Amer. Chem. Journ., **34**, 1 (1905).

Morse, H. N., and Gray, C. W. An electrical method for the simultaneous determination of hydrogen, carbon and sulphur in organic compounds. Amer. Chem. Journ., **35**, 451 (1906).

Morse, H. N., Frazer, J. C. W., and Hopkins, B. S. The osmotic pressure and depression of the freezing-points of solutions of glucose. Amer. Chem. Journ., **36**, 1 (1906).

Morse, H. N., Frazer, J. C. W., Hoffman, E. J., and Kennon, W. L. A redetermination of the osmotic pressure and depression of the freezing-points of cane sugar solutions. Amer. Chem. Journ., **36**, 39 (1906).

Morse, H. N., Frazer, J. C. W., and Lovelace, B. F. The osmotic pressure and the depression of the freezing-points of solutions of glucose. Amer. Chem. Journ., **37**, 324 (1907).

Morse, H. N., Frazer, J. C. W., and Holland, W. W. The osmotic pressure of cane sugar solutions in the vicinity of the freezing-point of water. Amer. Chem. Journ., **37**, 425 (1907).

Morse, H. N., Frazer, J. C. W., and Rogers, F. M. The osmotic pressure of glucose solutions in the vicinity of the freezing-point of water. Amer. Chem. Journ., **37**, 558 (1907).

Morse, H. N., Frazer, J. C. W., and Dunbar, P. B. The osmotic pressure of cane sugar solutions in the vicinity of 5°. Amer. Chem. Journ., **38**, 175 (1907).

Morse, H. N., and Morse, H. V. The osmotic pressure of cane sugar solutions at 10°. Amer. Chem. Journ., **39**, 667 (1908).

Morse, H. N., and Holland, W. W. The osmotic pressure of glucose solutions at 10°. Amer. Chem. Journ., **40**, 1 (1908).

Morse, H. N., and Mears, B. The osmotic pressure of cane sugar solutions at 15°. Amer. Chem. Journ., **40**, 194 (1908).

——— Improvement in cells for the measurement of osmotic pressure. Amer. Chem. Journ., **40**, 266 (1908).

Morse, H. N., and Lovelace, B. F. Improvements in manometers for the measurement of osmotic pressure. Amer. Chem. Journ., **40**, 325 (1908).

Morse, H. N., and Holland, W. W. The osmotic pressure of cane sugar solutions at 25°. Amer. Chem. Journ., **41**, 1 (1909).

——— The regulation of temperature in the measurement of osmotic pressure. Amer. Chem. Journ., **41**, 92 (1909).

——— The osmotic pressure of cane sugar solutions at 20°. Amer. Chem. Journ., **41**, 257 (1909).

Morse, H. N., Holland, W. W., Frazer, J. C. W., and Mears, B. The relation of osmotic pressure to temperature, Part I. Amer. Chem. Journ., **45**, 91 (1911).

Morse, H. N., Holland, W. W., and Carpenter, J. L. The relation of osmotic pressure to temperature, Part II. Amer. Chem. Journ., **45**, 237 (1911).

Morse, H. N., Holland, W. W., and Zies, E. G. The relation of osmotic pressure to temperature, Part III. Amer. Chem. Journ., **45**, 383 (1911).

Morse, H. N., Holland, W. W., and Myers, C. N. The relation of osmotic pressure to temperature, Part IV. Amer. Chem. Journ., **45**, 517 (1911).

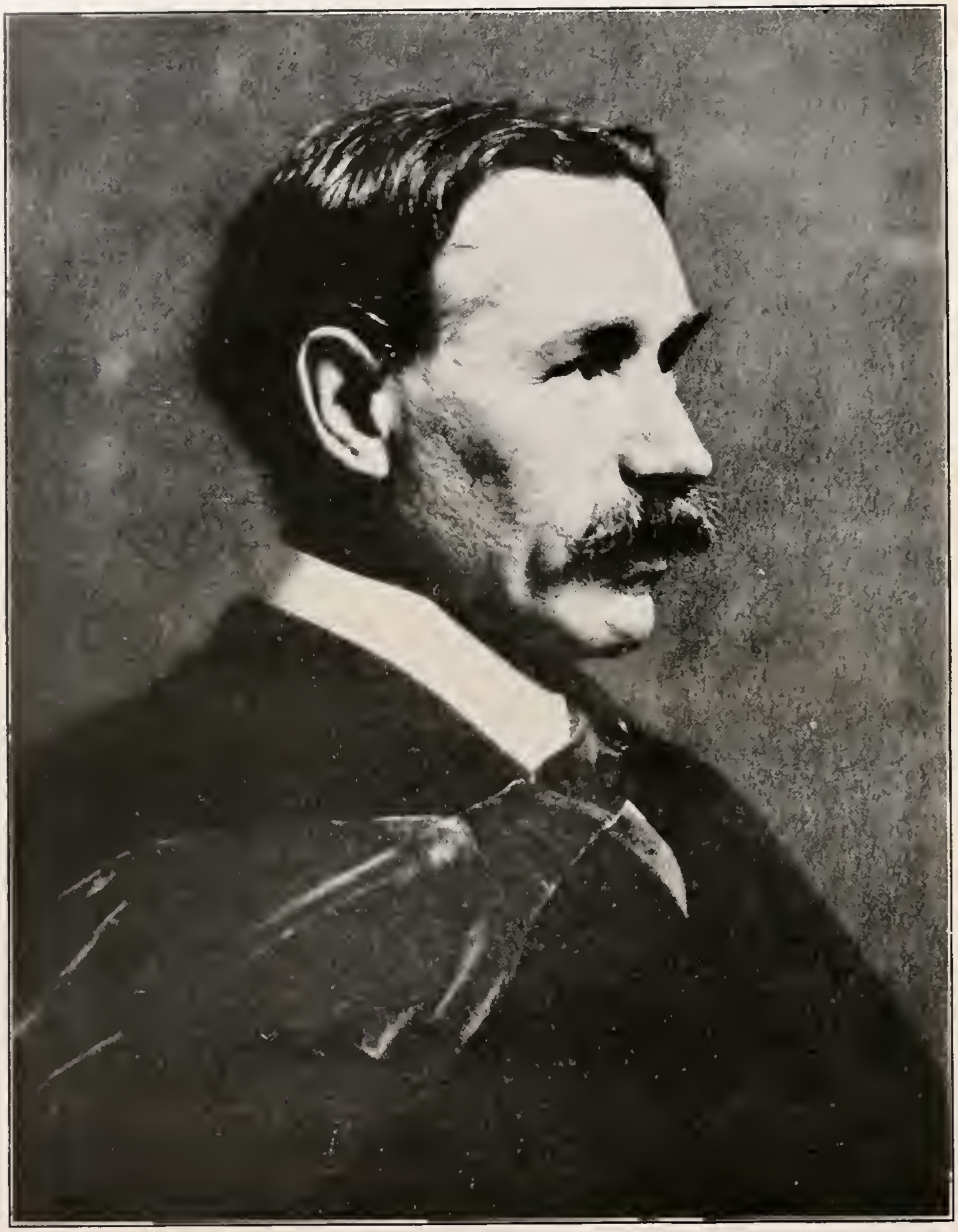
Morse, H. N., Holland, W. W., Zies, E. G., Myers, C. N., Clark, W. M., and Gill, E. E. The relation of osmotic pressure to temperature, Part V. Amer. Chem. Journ., **45**, 554 (1911).

Morse, H. N., Holland, W. W., Myers, C. N., Cash, G., and Zinn, J. B. The osmotic pressure of cane sugar solutions at high temperatures. Amer. Chem. Journ., **48**, 29 (1912).

BOOKS

Morse, H. N. Exercise in quantitative chemistry. Boston: Ginn & Co. (1905).

——— The osmotic pressure of aqueous solutions. Report on investigations made in the chemical laboratory of the Johns Hopkins University during the years 1899–1913. Monograph published by the Carnegie Institution of Washington, No. 198 (1914).



Alexander Smith

NATIONAL ACADEMY OF SCIENCES

Volume XXI
TWELFTH MEMOIR

BIOGRAPHICAL MEMOIR ALEXANDER SMITH

1865-1922

BY

WILLIAM A. NOYES

Presented to the Academy at the Annual Meeting, 1923

ALEXANDER SMITH

By William. A. Noyes

Alexander Smith was born at No. 4 Nelson Street, Edinburgh, Scotland, on September 11, 1865. His grandfather, Alexander Smith, was a sculptor. His father, also Alexander Smith, studied modeling in clay in the school of art in Edinburgh and was awarded a first prize for his work. He also studied music and became a musician and a teacher of singing. At least one very noted American chemist is the son of an artist, and in his ancestry and in that of Alexander Smith we may find strong evidence that the spirit of science is very closely akin to the spirit of art. His uncle, John Smith, was interested in paintings, and his aunt, Mary Smith, in church work. His paternal grandmother's maiden name was Jane Stewart.

His mother, Isabella Carter Smith, was the daughter of Andrew Carter. Her sister married John Bryce. They had nine children, of whom five were sons, all of whom engaged in business. One is a farmer in Dakota, another is in Australia.

He has one sister, Isabella Carter Smith, now Mrs. James Rae, of Edinburgh.

Alexander Smith married Sara Bowles, of Memphis, Tenn., February 16, 1905. Her grandfather was born in Kentucky and was one of the earliest settlers of Memphis. Her father, William Bowles, succeeded his father as head of the oldest firm of cotton buyers in Memphis. Her father entered the Confederate Army at the age of 16 and was a member of Forest's Cavalry. He was a celebrated shot and was a sharpshooter in the army. His mother was a Montague of Virginia. Mrs. Smith's maternal grandfather was Henry Potter, brother of Martin Potter, who founded the Cincinnati Enquirer. She has one brother, Potter Bowles, of Santa Ana, Calif.

There are two children, Isabella Carter Smith, born February 8, 1909, and William Bowles Smith, born October 27, 1910. Isabella is interested in literature, William in art. They are now attending schools in England.

Prof. George E. Fellows, now professor of history in the University of Utah, was one of the English and American group of students in Munich in 1889, living there at the time with his wife and small daughter. After Doctor Smith and Doctor Fellows had taken their degrees the latter spent several months with the family of Doctor Smith in Edinburgh and at Dunblane. In the summer of 1890 Doctor Smith visited Professor and Mrs. Fellows at Aurora, Ill. After he went to Chicago he lived for five years in their family, before his marriage. I am indebted to Professor Fellows for the following anecdote:

Alexander Smith showed very early an unusual interest in scientific research. It is well known and related by the members of his family that when he was between 3 and 4 years of age he became much interested in a bird that he saw. He turned the pages of an encyclopedia until he found the picture of this particular bird, then carried the book to members of the family and demanded that the story of the bird should be read. Not getting all that he desired, he continued to drag the heavy book about with him for several days until compelled to desist.

At the age of 10 he entered the Edinburgh Collegiate School. The principal was Archibald Hamilton Bryce, a brother of Lord Bryce, the British ambassador to the United States. The school report for 1879–80 shows that his work in Latin and Greek was poor, but that he was proficient in mathematics, French, bookkeeping, and shorthand.

He entered the University of Edinburgh as a candidate for the degree of B. Sc. in chemistry in 1882 and graduated in 1886. For some time before entering the university and while attending there he studied astronomy more than chemistry. He found, however, that there was no opportunity in Great Britain to earn a salary which would sustain life if he continued in that field. Accordingly, after graduation at Edinburgh he went to study chemistry in the laboratory of Baeyer at Munich. At that time a very large majority of the chemists of the

world were working in the field of organic chemistry, and while Alexander Smith secured at Edinburgh and at Munich very thorough training in inorganic and analyticical chemistry—physical chemistry was only just beginning to be noticed as a separate field—he gave especial attention to organic chemistry and carried out a piece of work on 1,3-diketones under the guidance of Ludwig Claisen, one of the brilliant group working in Baeyer's laboratory at that time. The work contributed toward the solution of some of the problems concerning tautomerism and condensations of the acetoacetic ester type, which were then engaging the attention of Claisen and other chemists.

After securing his degree Doctor Smith returned to Edinburgh and spent another year at the university as an assistant in charge of qualitative analysis. He also gave a course on organic syntheses. As promotion seemed likely to be very slow in Scotland or England, he visited America during the summer of 1890, hoping for an appointment. I had made his acquaintance in Munich in 1889. By one of those happy coincidences which sometimes occur, I received within a very few days a letter from Doctor Smith saying he would be glad to secure a position in America, and a letter from Prof. John M. Coulter, then at Wabash College, Crawfordsville, Ind., saying that their chair of chemistry was vacant and asking me to suggest a candidate. I named Doctor Smith and he visited Crawfordsville and interviewed a number of the trustees. Wabash College has Presbyterian affiliations and Doctor Smith's Scotch relationships and his other brilliant personal qualities made a very favorable impression, and the appointment was made as professor of chemistry and mineralogy.

During the four years that followed, 1890–1894, Professor Smith rapidly gained experience as a very careful and forceful lecturer and successful teacher. He also continued his researches in the field of organic chemistry, studying especially condensation by means of potassium cyanide, and derivatives of benzoin.

In 1894 he was asked to go to the University of Chicago as assistant professor of chemistry in charge of the instruction in elementary inorganic chemistry. In 1898 he was promoted to the rank of associate professor, and in 1904 he was given the title of professor of chemistry and director of physical and inorganic chemistry. From 1901 to 1911 he was dean in junior colleges, in charge of science students.

When he went to Chicago Professor Smith saw very clearly that it would be to his advantage, personally, to change his field of research from organic to inorganic and physical chemistry. Such a change was also very useful in promoting the development of a more varied department of chemistry in the university. His early training in mathematics and physics gave him a splendid preparation for his new line of work, and he soon became one of the best-known physical chemists in America.

In physical and inorganic chemistry, his first research of considerable importance was an exhaustive, classical investigation of the forms of sulphur. Many others had preceded him in this field, but he brought to the problem such an insight into the varied and complex factors involved and such an ingenuity in applying the methods of modern physical chemistry that at the close of his work the subject might be considered as an almost completed chapter of our chemical knowledge.

In recognition of his work with the forms of sulphur and his studies of vapor pressure in collaboration with A. W. C. Menzies, Professor Smith was awarded the Keith prize and medal in 1912 by the Royal Society of Edinburgh. In announcing the award, Sir William Turner summarized his work as follows:

> The work on sulfur was published in seven papers. At the time these investigations were begun, the published observations upon the behavior of melted sulfur were full of apparent inconsistencies, and could not be formulated in harmony with physico-chemical theory.
>
> The first step was to settle the disputed question as to the relations of amorphous and soluble sulfur in the melt. Measurements of freezing points and of the corresponding proportions of amorphous sulfur in the freezing liquid showed that Raoult's law held rigorously. This established the existence of liquid amorphous sulfur dissolved, but distinct from the melted soluble sulfur.
>
> The fact that melted sulfur, when kept at a given temperature, gives, on chilling, very inconstant proportions of amorphous sulfur was next investigated. It was discovered that the introduction of sulfur dioxide and

other foreign substances greatly influenced the proportions. These foreign bodies were proved to act catalytically, and retard or hasten the change from amorphous to soluble sulfur. The establishment of this conclusion at once afforded a basis for explaining a large proportion of the apparent inconsistencies in the older as well as the more recent observations. In connection with this work, the proportions of amorphous sulfur present in equilibrium at various temperatures were measured.

In the fifth paper, studies of some other peculiarities in the behavior of melted sulfur were described, and all the results were shown to harmonize with a theory of the relation of the two liquid forms as dynamic isomers.

Precipitated sulfur was the subject of the sixth paper, and it was shown that, when first liberated, the sulfur consists of droplets of liquid amorphous sulfur. In presence of weak acids, or in neutral or alkaline solutions, this changes wholly to crystalline, soluble sulfur. In presence of active acids, the amount of amorphous sulfur surviving in the final product is proportional to the concentration of the acid.

In the seventh paper, the generally accepted melting points (or freezing points) of the various forms of sulfur, determined before the complex nature of the problem which such measurements involved was in the least suspected, were subjected to revision, and the correct values, in harmony with the theory, were given.

The work on Vapor Pressures (carried out in collaboration with Prof. A. W. C. Menzies) is described in seven papers. The first two deal with a simple device, named the "submerged bulblet," by which boiling points and vapor pressures of liquids and of non-fusing solids may be determined with the use of only minute amounts of material.

In the third and fifth papers, forms of apparatus for the exact study of vapor pressures, and named respectively the static and dynamic "isoteniscope," are described. To ascertain the possibilities of the methods, values for water, which agree with the best previous determination, were obtained by the static method, and values for benzene and for ammonium chloride by the dynamic method.

The fourth paper describes a determination of the vapor pressures of mercury. These were made because exact values were required for the subject of the sixth paper, and the existing results (e. g., those of Regnault, Ramsay and Young, and others) were highly inconsistent with one another, and the methods used were open to serious criticism.

The sixth paper deals with the constitution of calomel vapor, a matter long but inconclusively discussed by chemists. By making measurements of the vapor pressures of mercury, of calomel, and of a mixture of the two, and applying the laws of chemical equilibrium to the resulting data, it was shown conclusively that the vapor is wholly composed of mercury and corrosive sublimate. The close quantitative correspondence showed that in these measurements the order of accuracy was much higher than in any previous measurements of vapor pressures at elevated temperatures.

The seventh paper shows that, as the laws of chemical equilibrium applied to the result of the preceding paper predict, calomel, when dried in the most rigorous manner, exercises, even at high temperature, no measurable pressure whatever. This is the only successful experimental confirmation of a familiar and important application of the theory.

In 1911 he was called to Columbia University, New York City, as professor of chemistry and administrative head of the department of chemistry, succeeding Prof. C. F. Chandler, who had retired in 1910. He continued in this position till his failing health compelled him to retire in the fall of 1919. He died at Edinburgh, on September 8, 1922.

At Columbia University he directed his attention chiefly to a study of the vapor pressures and densities of the ammonium halides. Some years before, Baker had shown that thoroughly dry ammonium chloride vaporizes without dissociation (J. Chem. Soc., **65**, 615 (1894) and **73**, 422 (1898). It had long been known that ordinary ammonium chloride, containing a trace of moisture, is almost completely dissociated when it is heated somewhat above its point of vaporization. Professor Smith and his collaborators devised methods for determining the density of *saturated* vapors of ammonium halides and demonstrated that the dissociation is very far from complete in such conditions. The dissociation of ammonium chloride does not exceed 67 per cent at 280° to 330°, while the dissociation of ammonium bromide and ammonium iodide is still less. They also demonstrated that the transition point of ammonium chloride is the same for the carefully dried as for the undried salt. The purpose of these later experiments was to find some explanation for the anomalous fact that the vapor pressure of carefully dried ammonium chloride, which vaporizes without dissociation, is the same as that of the partially dissociated chloride. A satisfactory explanation was not found, but the suggestion of Wegscheider (Z. anorg. Chem. **103**, 207 (1918)), that the constancy of the vapor pressure was due to the fact that the dried salt does not undergo a transition at 184.5°, was shown to be untenable.

From the time when he began his work at Wabash College Professor Smith gave very careful attention to the selection and proper presentation of those topics which should be used in the instruction of students during their first year in the study of chemistry. After going to Chicago he was especially responsible for the elementary courses in the university. His experience led him to a very firm belief in a system of instruction by recitations in comparatively small classes and the use of a textbook rather than instruction by means of lectures.

On September 8, 1899, Dean Russell, of Teachers' College, asked him to write the portion devoted to chemistry of a book on the Teaching of Chemistry and Physics. The portion on the teaching of physics was written by Prof. E. H. Hall, of Harvard. The book was published by Longman, Green & Co. in 1902, and still holds a unique place in our scientific literature.

The first edition of his Laboratory Outline of General Chemistry was published in 1899. The sixth edition (sixty-sixth thousand) appeared in January, 1917, under the title "Experimental Inorganic Chemistry." The book was translated into German by Prof. F. Haber and Doctor Stoecker, in 1904; into Russian by N. B. Schmoelling, in 1908; into Italian by Dr. F. C. Palazzo and Prof. A. Peratoner, in 1910.

The Introduction to Inorganic Chemistry was begun at the Yerkes Observatory, Williams Bay, September 18, 1903. It was completed to chapter 34 on January 3, 1904. June 13 to August 8, 1904, were spent at Williams Bay in completing the first writing of the book. June 10 to July 10, 1905, were devoted to correcting, adding to, and adjusting the manuscript. The book was issued by the Century Co. on February 15, 1906, and the demands surprised both Professor Smith and the publishers. More than 6,000 copies were sold before October 1, 1906. A second revised edition was published in 1908 and a third edition, largely rewritten, in 1917. The success of the book was partly due to the clear, lucid style in which it is written, but it was due fully as much to the adequate presentation of modern theories of solution and of equilibria for the first time in an introductory English textbook.

The book was translated into German, Russian, Italian, and Portuguese.

He also wrote a General Chemistry for Colleges, published in 1908, and a Textbook of Elementary Chemistry, which appeared in 1914.

Professor Smith was president of the American Chemical Society for the year 1911. His presidential address on "Lomonossoff, an early physical chemist," was a notable contribution to a little-known historical subject.

He was elected to membership in the Royal Society of Edinburgh in 1891; as an honorary foreign member of the Societdad Espagñola de Fisica y Quimica of Madrid in 1911; as a member of the American Academy of Arts and Sciences in 1914; as a member of the National Academy of Sciences in 1915.

In 1919 his alma mater, the University of Edinburgh, awarded him the degree of LL. D. He was introduced at the graduation ceremonies as follows:

> A most distinguished graduate of our own university, Professor Smith, has risen to the rank of a super-chemist in the United States, head of a department embracing many specialized professorships, and director of one of the most important laboratories in the New World. We congratulate Columbia University on the possession of a teacher and investigator of such rare ability, and we congratulate ourselves on the opportunity of laureating an alumnus whose success reflects no little luster on the institution where he received his early training.

To those who knew him best Professor Smith was a valued and loyal friend, a kind and considerate husband and father, with the highest ideals in his personal life and with a broad human interest in art and literature and in many fields of science other than chemistry. His knowledge was broad and profound, and his keen wit, quickness of repartee, and the epigrammatic quality of his remarks made conversation with him stimulating in a high degree. He has left a deep impression on many students who worked with him, and the science of chemistry in America and in the world has been enriched by his labors as a teacher and as an investigator.

BIBLIOGRAPHY OF SCIENTIFIC PAPERS

ORGANIC CHEMISTRY

1. Thesis, Ueber 1, 3-Diketone. Munich, 1889. Beiträge zur Kenntniss der 1, 3-Diketone. (With Prof. Claisen.) Liebig's Annalen, 277, 184–206 (1893).

2. On desylacetophenone. Journal of the Chemical Society, 57, 643–652 (1890).

3. Ueber Condensation mittelst Cyankalium. Berichte der deutschen chemischen Gesellschaft, **26**, 60–65 (1892).

4. Ueber die Condensation von Aceton mit Benzoin mittelst Cyankalium. Berichte der deutschen chemischen Gesellschaft, **26**, 65–71 (1892).

5. Two stereo-isomeric hydrazones of benzoin. (With J. H. Ransom.) Royal Society Edinburgh, Proceedings, pp. 201–202; 1894; Americau Chemical Jourual, **16**, 108–115 (1894).

6. Die Einwirkung von Hydrazin und von Phenylhydrazin auf **1**, 4-Diketone. Liebig's Annalen, **289**, 310–337 (1896).

7. Condeusation with benzoin by means of sodium ethylate. (Thesis of J. B. Garner.) University of Chicago Press, 1897.

8. On the phenylhydrazones of benzoin. American Chemical Journal, 22, 198–207 (1899).

9. On potassium cyanide as a condensing agent. American Chemical Journal, 22, 249–256 (1899).

10. Notizen über die Einwirkung von Pheuylhydrazin Auf Einige 1, 4-Diketone. (With H. N. McCoy.) Berichte der deutschen chemischen Gesellschaft, **35**, 2169–2171 (1902).

INORGANIC AND PHYSICAL CHEMISTRY

11. Amorphous sulphur and its relation to the freezing point of liquid sulphur. (With W. B. Holmes.) Royal Society Edinburgh, Proceedings, **1902**, pp. 290–301; decennial publications of the University of Chicago, IX; Zeitschrift für physikalische Chemie, **42**, 469–480 (1903).

12. On two liquid states of sulphur, S_μ and S_λ, and their transition point. (With W. B. Holmes and E. S. Hall.) Royal Society Edinburgh, Proceedings, 1905, 588–589; Zeitschrift für physikalische Chemie, **52**, 602–625; J. Amer. Chem. Soc., **27**, 801 (1905).

13. The nature of the amorphous sulphur, and contributions to the study of the influence of foreign bodies on the phenomena of supercooling observed when melted sulphur is suddenly chilled. (With W. B. Holmes.) Royal Society Edinburgh, Proceedings, 1905, 590–592; Berichte der deutschen chemischen Gesellschaft, **35**, 2992–2994 (1902); Zeitschrift für physikalische Chemie, **54**, 257–293.

14. Further study of the two forms of liquid sulphur as dynamic isomers. (With C. M. Carson.) Royal Society Edinburgh, Proceedings, 1906, 352–356; Zeitschrift für physikalische Chemie, **57**, 689–717.

15. Ueber den amorphen Schwefel: V. Das System Schwefel–Jod (with C. M. Carson). Zeitschrift für physikalische Chemie, **61**, 200–208 (1907).

16. Precipitated sulphur. (With R. H. Brownlee.) Royal Society Edinburgh, Proceedings, 1907, 308–311; Zeitschrift für physikalische Chemie, **61**, 209–226.

17. Ueber den amorphen Schwefel. VII. Die Gefrierpunktskurven des flüssigen Schwefels bei Abscheidung von perlmutterartigens, bzw. rhombischen Schwefel. (With C. M. Carson.) Zeitschrift für physikalische Chemie, **77**, 661–676 (1911).

18. The solubilities of orthophosphoric acid and its hydrates. A new hydrate. (With A. W. C. Menzies.) J. Amer. Chem. Soc., **31**, 1183–91 (1909); Proc. R. S. E., XXX, 63–64 (1909).

19. The electrical conductivity and viscosity of concentrated solutions of orthophosphoric acid. (With A. W. C. Menzies), J. Amer. Chem. Soc., **31**, 1191–1194 (1909).

20. Does calomel furnish another contradiction of the theory of heterogeneous dissociation equilibrium? J. Amer. Chem. Soc., **32**, 187–189 (1910).

21. Studies in vapor pressure: I. A method for determining under constant conditions the boiling points of even minute quantities of liquids and of nonfusing solids. (With A. W. C. Menzies.) J. Amer. Chem. Soc., **32**, 897–905 (1910); Zeit. physikal. Chem., **75**, 494–497; Proc. R. S. E., **30**, 432–435.

22. A common thermometric error in the determination of boiling points under reduced pressure. (With A. W. C. Menzies.) J. Amer. Chem. Soc., **32**, 905–907 (1910): Zeit. physikal. Chem., **75**, 498–499; Proc. R. S. E., **30**, 436.

23. Studies in vapor pressure: II. A simple dynamic method applicable to both solids and liquids, for determining vapor pressures, and also boiling points at standard pressures. (With A. W. C. Menzies.) J. Amer. Chem. Soc., **32**, 907–914 (1910); Zeit. physikal. Chem., **75**, 500–502; Proc. R. S. E., **30**, 437–439.

24. Studies in vapor pressures: III. A static method for determining the vapor pressures of solids and liquids. (With A. W. C. Menzies.) J. Amer. Chem. Soc., **32**, 1412–1434 (1910); Wied. Ann. (4), **33**, 971–978; Proc. R. S. E., **30**, 523–528.

25. Studies in vapor pressure: IV. A redetermination of the vapor pressures of mercury from 250° to 435°. (With A. W. C. Menzies.) J. Amer. Chem. Soc., **32**, 1434–1447 (1910); Wied. Ann. (4), **33**, 979–988; Proc. R. S. E., **30**, 521–522.
26. Studies in vapor pressure: V. A dynamic method for measuring vapor pressures, with its application to benzene and ammonium chloride. (With A. W. C. Menzies.) J. Amer. Chem. Soc., **32**, 1448–1459 (1910); Wied. Ann. (4), **33**, 989–994; Proc. R. S. E., **31**, 179–182.
27. Studies in vapor pressure: VI. A quantitative study of the constitution of calomel vapor. (With A. W. C. Menzies.) J. Amer. Chem. Soc., **32**, 1541–1555 (1910); Zeit. physikal. Chem., **76**, 251–254; Proc. R. S. E., **31**, 183–185.
28. Studies in vapor pressure: VII. The vapor pressure of dry calomel. (With A. W. C. Menzies.) Zeit. physikal. Chem., **76**, 713–720 (1911); Proc. R. S. E., **31**, 519–520.
29. On amorphous sulphur: The freezing point curves of liquid sulphur when rhombic sulphur and soufre nacré crystallize out. (With C. M. Carson.) Zeit. physikal. Chem., **77**, 661–676 (1911); Proc. R. S. E., **32**, 1–3.
30. The dissociation pressures of ammonium and tetramethyl ammonium halides and of phosphonium iodide and phosphorus pentachloride. (With R. P. Calvert.) J. Amer. Chem. Soc., **36**, 1363–1382 (1914).
31. The densities and degrees of dissociation of the saturated vapors of the ammonium halides, and the related thermal data. (With R. H. Lombard.) J. Amer. Chem. Soc., **37**, 38–70 (1915); Proc. R. S. E., **35**, 162–167.
32. The densities and degrees of dissociation of the saturated vapor of phosphorus pentachloride. (With R. H. Lombard.) J. Amer. Chem. Soc., **37**, 2055–2062 (1915).
33. The dissociation pressures of mercurous chloride. (With R. P. Calvert.) J. Amer. Chem. Soc., **38**, 801–807 (1916).
34. The allotropy and solubilities in water of ammonium bromide. (With H. E. Eastlack.) J. Amer. Chem. Soc., **38**, 1261–1266 (1916).
35. Ammonium iodide, its solubilities and the absence of a transition point. (With H. E. Eastlack.) J. Amer. Chem. Soc., **38**, 1500–1502 (1916).
36. The transition of dry ammonium chloride. (With H. E. Eastlack and George Scatchard.) J. Amer. Chem. Soc., **41**, 1961–1969 (1919).

ASTRONOMY

1. The asteroids and their origin. North British Advertiser, Edinburgh, December 5, 1885.
2. The recent meteor shower. The United Presbyterian Magazine, **3**, 20–21 (1886).
3. Recent discoveries in meteoric astronomy. Scotsman, Edinburgh, Oct. 11, 1886.
4. Photographic star-charting. Chambers' Journal, **3**, 732–733 (1886).

TEACHING CHEMISTRY, ETC.

1. Physiological fiction. The Wabash, Crawfordsville, Ind. **15**, 7–12 (1891).
2. The growth and progress of university extension in Great Britain. The Wabash, Crawfordsville, Ind. **16**, 1–5 (1891).
3. Heredity and education. Proc. Indiana College Association, 1892, 16.
4. The high school course in chemistry. School Review **5**, 497–503 (1897).
5. Articles on chemistry and electrolysis. Werner's New American Supplement to the Encyclopedia Britannica, pp. 767–776, 1174–1176.
6. The value of chemistry as part of a school or college course. National Educational Association, Proc. 1897, 945–951.
7. Articulation of school and college work in the sciences. School Review, **7**, 411–417, 453–461, 527–535 (1899).
8. Report of the subcommittee on chemistry of the National Educational Association on college entrance requirements. N. E. A. Report, 1899, pp. 26 and 165.
9. The pupil before and after taking chemistry. School Science and Mathematics, **3**, 189–207 (1903).
10. Nature study and high school chemistry. Nature Study Review, **2**, 193–198 (1906).
11. The point of view in chemistry. School Science and Mathematics, **7**, 128–135 (1907).
12. High school science in relation to college or university work. Educational Bi-monthly, **1**, 328–335 (1907).
13. Comment on "Another point of view." School Science and Mathematics, **7**, 508–510 (1907).
14. Review of five histories of chemistry. Science, N. S., **27**, 303–306 (1908).
15. The experimental view point in chemistry. School Science and Mathematics, 8, 582–590 (1908).
16. The rehabilitation of the American college and the place of chemistry in it. Science, N. S. **30**, 457–466 (1909).
17. An early physical chemist. M. W. Lomonossoff. J. Amer. Chem. Soc., **34**, 109–119 (1912). Science, N. S. **35**, 121–129.
18. Articles on chemistry. Encyclopedia of Education, Macmillan, pp. 585–596, 1911.
19. Some possible items, new and old, for the course in elementary chemistry. Proc. of the New England Assn. of Chemistry Teachers. February, 1915.
20. The content, methods, and results of the high school course in chemistry. School Science and Mathematics, **16**, 289–302 (1916).
21. The training of chemists. Science, N. S. **43**, 619–629 (1916); J. Indust. and Engin. Chem., 8, 527–533.

BOOKS

Lassar-Cohn. Laboratory manual of organic chemistry (translation). Pp. xx+403, 8vo. London: Macmillan & Co., 1895.

A laboratory outline of general chemistry. Pp. xii+88, 8vo. Chicago: University of Chicago Press, 1899. Reprinted 1900.

The same. Second edition, revised. Pp. xii+107. 1902. Reprinted 1903; reprinted 1905.

The same. Third edition, revised in collaboration with W. J. Hale. Pp. ix+136. New York: The Century Co., June, 1907.

The same. Fourth edition. December, 1907.

The same. German translation, by Prof. F. Haber and Dr. Stoecker. Karlsruhe, 1904. Second edition, 1910.

The same. Russian translation, by Dr. von Schmoelling. St. Petersburg, 1908.

The same. Italian translation, by Prof. Peratoner and Dr. Palazzo. Palermo, 1910.

Louis Pasteur. Researches on molecular asymmetry (translation). Pp. 46, 8vo. Alembic Club Reprints, No. 14.

The teaching of chemistry and physics in the secondary school. (The chapters dealing with physics, specifically, were written by Prof. Edwin H. Hall, of Harvard University.) Pp. xiii+377, 8vo. New York and London: Longmans, Green & Co., 1902. Reprinted, 1903.

J. H. Van't Hoff. Physical chemistry in the service of the sciences (translation). Nine lectures delivered at the University of Chicago. Pp. xviii+126, 8vo. Chicago: University of Chicago Press, 1903.

Introduction to general inorganic chemistry. Pp. XVIII+780. 8vo. New York: The Century Co. London: Geo. Bell & Sons, March, 1906. Reprinted July, 1906; September, 1906; August, 1907; October, 1907 (completing fifteenth thousand). Second edition, 1908. Third edition, rewritten, 1917.

———. German translation by Stern and Haber, Karlsruhe, 1909. Second edition, 1913; third edition, revised by J. D'ans, 1914.

———. Italian translation by Montemartini. S. T. E. N. Torino, 1912.

———. Russian translation by Melikov. Odessa, 1911. Russian translation of German edition by Pisarskovski, Odessa, 1911.

———. Portuguese translation, by Gomez, 1911.

General chemistry for colleges. Pp. XIII+529, 8vo. New York: The Century Co., May, 1908. Second edition, entirely rewritten, 1916.

A text book of elementary chemistry. Pp. VIII+439. New York: The Century Co., March, 1914. London: Geo. Bell & Sons, 1915.

A laboratory outline of elementary chemistry. Pp. X+137. New York: The Century Co., August, 1914. London: Geo. Bell & Sons, 1915.

A laboratory outline of college chemistry. Pp. IV+206. New York: The Century Co., September, 1916. London: Geo. Bell & Sons.

Experimental inorganic chemistry (5th edition of laboratory outline of general chemistry). New York: The Century Co., 1917. London: Geo. Bell & Sons.

Reviews: A large number of reviews of current books on chemistry have been written for Science, the Journal of the American Chemical Society, School Science, the Journal of Physical Chemistry, the School Review, and other magazines.

Intermediate chemistry. New York: The Century Co., 1918.

Wallace C. Sabin.

NATIONAL ACADEMY OF SCIENCES

Volume XXI
THIRTEENTH MEMOIR

BIOGRAPHICAL MEMOIR WALLACE CLEMENT WARE SABINE

1868-1919

BY

EDWIN H. HALL

Presented to the Academy at the Annual Meeting, 1924

WALLACE CLEMENT WARE SABINE

1868–1919

By Edwin H. Hall [1]

Our colleague, Wallace Clement Ware Sabine, was born in Richwood, Ohio, June 13, 1868. According to family tradition, four racial strains were joined in him, each of his four names representing some family of his ancestors, one Scotch, one Dutch, one English, and one French.

The most remote ancestor of whom we have knowledge was William Sabine, or Sabin, who was living in the town of Rehoboth, near Plymouth, Mass., in 1643. He is supposed to have been of Huguenot stock, though conclusive evidence of this derivation is not found. It seems clear that he was a man of substance and of good standing in his community. His will, written in 1685, probated in Boston and, I think, still to be seen there, mentions by name "sixteen of his twenty children."

From him the line of descent runs through Benjamin, four Nehemiahs in succession, John Fletcher, and Hylas, to Wallace Clement. Early in the nineteenth century the fourth Nehemiah Sabine went from Massachusetts to Ohio, "with a family of eleven children." "He was a pioneer preacher, with a circuit of fifty miles radius. Leather saddle-bags on his horse carried the Bible and his doctrine of eternal rewards and punishments." His son, John Fletcher, "became a Universalist, gentle and generous." "He [John Fletcher] died at the age of eighty-nine, with mind as vigorous and clear as in youth, and with a remarkably retentive memory. His wife was Euphemia Clement, a gentle, industrious, reliable woman. Hylas Sabine was their oldest son." Of Wallace Sabine's maternal grandfather, Jacob Reed Ware, who was of English Quaker stock, it is written, "He was one of the early, ardent abolitionists and lived on the most direct line from southern slavery to freedom in Canada." "Untiring of body, alert of mind, and exceedingly strong of purpose, he lived in perfect health, with such simple habits that at the age of ninety-eight, without disease, he fell asleep." "J. R. Ware married Almira Wallace, a woman of force and uprightness. Anna Ware was their first daughter."

To those who knew Sabine well this brief family history is deeply significant. Gentleness, courtesy, rectitude, untiring energy, fixity of purpose that was like the polarity of a magnet, all these traits we find in him. It is interesting and impressive to see how the individualism and stern conscience that made his ancestors on the one side Quakers in England and, probably, on the other side Protestants in France, found expression in him, under changed intellectual conditions. He was of the very stuff of which martyrs are made; in fact, he died a martyr to his sense of duty, but, with an austerity of morals and a capacity for devotion which none of his conspicuously religious forefathers could have surpassed, he held aloof, silently but absolutely, from all public profession of religious creed, and he took small part in religious observances.

Hylas Sabine, the father of Wallace, was Railway Commissioner of Ohio, and it is said that he was the first commissioner in the country to establish and adequately enforce State supervision of railways through State inspection and compulsory returns. One of his maxims, quoted to me by his son, was substantially this: *Treat everyone with courtesy, and especially yourself.* He had "esthetic sense and ability to design and draw," which qualities both his children inherited.

In mental and physical energy, intensity of feeling, and bent for exact science, Wallace resembled his mother, whose vigorous yet delicate personality charmed everyone who met her.[2]

The blended inheritance from the parents is perhaps shown in the fact that the daughter, now the wife of Prof. Wilbur Siebert, of Columbus, Ohio, once engaged in research on the

[1] In this memoir I shall draw freely, usually without quotation marks, from two papers written by me soon after Professor Sabine's death, one for the Harvard Graduates' Magazine, the other for the records of the Harvard Faculty of Arts and Sciences. In many cases what appears to be a continuous quotation from Sabine's papers is made up of selected passages. E. H. H.

[2] She died in March, 1923, aged 88 years. Most of the genealogical matter contained in this paper was furnished by her, and a considerable part of it is here given in her own telling words.

telephone, under Professor Cross, at the Massachusetts Institute of Technology, and later became highly skilled as a painter of miniature portraits, while the son found his chief distinction in a field of exact science closely related to esthetic art.

As a child Wallace was allowed to develop without forcing, but such was the natural vigor of his mind that he gained the degree of A. B. at Ohio State University at the age of 18. He is said not to have specialized in his college studies, but he had in Prof. T. C. Mendenhall an inspiring teacher of physics, and his early interest in scientific matters is shown by the fact that he attended a meeting of the American Association for the Advancement of Science held in Philadelphia in 1884, when he was 16 years old. On leaving Ohio State University in 1886 he went to Harvard as a graduate student in mathematics and physics, and he received the Harvard A. M. in 1888. From 1887 to 1889 he held a Morgan fellowship, but in the latter year he became an assistant in physics. Rather early in his Harvard residence he was taken by Professor Trowbridge as partner in a photographic study of the oscillating electric discharge, and he showed a remarkable aptitude for work of this kind, requiring high experimental skill, yet he never became a candidate for the Ph. D. Absorption in the work of teaching prevented him for several years from engaging deeply in further work of research. He spent his energy and his talents in building up courses of laboratory work, designing and making apparatus for instruction, and in every way practicing with devotion the profession of a teacher. It is not too much to say that, for the 15 years preceding his taking the duties of a deanship, he was the most effective member of the Harvard department of physics in giving inspiration and guidance to individual students of promise. This was due in part to his comparative youth, though no one of the department was repellently old; in part to his sympathetic willingness to give help and to spend much time in giving help, though others were not lacking in this quality. It was perhaps due mainly to the fact that, while he was no more deeply versed than others in the profundities of physics and mathematics, he had a peculiarly clear vision for the right kind of experimental problem and for the best way of attacking it, and his students instinctively, it may be, perceived this.

For a long time he seemed to be content to remain in comparative obscurity, while directing others into paths of conspicuous achievement. He was made assistant professor of physics in 1895, after six years of teaching, in which he published little or nothing descriptive of research. This was partly because he had a most severe standard for what a research paper should be; it should describe some piece of work so well done that no one would ever have to investigate this particular matter again. To this standard he held true, with the result that his published papers were, to the end, remarkably few and remarkably significant.

One might have expected him, when he found time for research, to take up some problem in light, for that had seemed to be his chief field of interest; but accident, and a sense of duty, turned him to a different quarter. The Fogg Art Museum, on its completion in 1895, proved to have an auditorium [3] that was monumental in its acoustic badness, and President Eliot, not fully realizing the importance of the step he was taking, but acting with his usual sure judgment of men, called upon Sabine to find a remedy, as a practical service to the university. The nature of Sabine's problem and the way he analyzed it can best be shown by the following passages taken from the first [4] of his "Collected papers on acoustics":

> No one can appreciate the condition of architectural acoustics—the science of sound as applied to buildings—who has not with a pressing case in hand sought through the scattered literature for some safe guidance. Responsibility in a large and irretrievable expenditure of money compels a careful consideration, and emphasizes the meagreness and inconsistency of the current suggestions. Thus the most definite and often repeated statements are such as the following, that the dimensions of a room should be in the ratio 2:3:5, or according to

[3] The architect of this building was Richard Morris Hunt, one of the most eminent in America of his generation. The following story is told of a conversation between him and the Rev. Henry Ward Beecher:

Mr. BEECHER. Mr. Hunt, how much do you know about acoustics?

Mr. HUNT. As much as anyone, Mr. Beecher.

Mr. BEECHER. How much is that?

Mr. HUNT. Not a damned thing.

Mr. BEECHER. I think you are right.

[4] Published first in the American Architect and the Engineering Record, 1900.

some writers, 1:1:2, and others, 2:3:4; it is probable that the basis of these suggestions is the ratios of the harmonic intervals in music, but the connection is untraced and remote. Moreover, such advice is rather difficult to apply; should one measure the length to the back or to the front of the galleries, to the back or the front of the stage recess? Few rooms have a flat roof, where should the height be measured? One writer, who had seen the Mormon Temple, recommended that all auditoriums be elliptical. Sanders Theatre is by far the best auditorium in Cambridge and is semicircular in general shape, but with a recess that makes it almost anything; and, on the other hand, the lecture room in the Fogg Art Museum is also semicircular, indeed, was modeled after Sanders Theatre, and it was the worst. But Sanders Theatre is in wood and the Fogg Lecture room is plaster on tile; one seizes on this only to be immediately reminded that Sayles Hall in Providence is largely lined with wood and is bad. . . .

In order that hearing may be good in any auditorium, it is necessary that the sound should be sufficiently loud; that the simultaneous components of a complex sound should maintain their proper relative intensities; and that the successive sounds in rapidly moving articulation, either of speech or music, should be clear and distinct, free from each other and from extraneous noises. These three are the necessary, as they are the entirely sufficient, conditions for good hearing. . . . Within the three fields thus defined is comprised without exception the whole of architectural acoustics.

Starting with the simplest conceivable auditorium—a level and open plain, with the ground bare and hard, a single person for an audience—it is clear that the sound spreads in a hemispherical wave diminishing in intensity as it increases in size, proportionally. If, instead of being bare, the ground is occupied by a large audience, the sound diminishes in intensity even more rapidly, being now absorbed. The upper part of the sound-wave escapes unaffected, but the lower edge—the only part that is of service to an audience on a plain—is rapidly lost. The first and most obvious improvement is to raise the speaker above the level of the audience; the second is to raise the seats at the rear; and the third is to place a wall behind the speaker. The result is most attractively illustrated in the Greek theatre. These changes being made, still all the sound rising at any considerable angle is lost through the opening above, and only part of the speaker's efforts serve the audience. When to this auditorium a roof is added the average intensity of sound throughout the room is greatly increased. . . .

In discussing the subject of loudness the direct and reflected sounds have been spoken of as if always reenforcing each other when they come together. A moment's consideration of the nature of sound will show that, as a matter of fact, it is entirely possible for them to oppose each other. The sounding body in its forward motion sends off a wave of condensation, which is immediately followed through the air by a wave of rarefaction produced by the vibrating body as it moves back. These two waves of opposite character taken together constitute a sound wave. The source continuing to vibrate, these waves follow each other in a train. Bearing in mind this alternating character of sound, it is evident that should the sound travelling by different paths—by reflection from different walls—come together again, the paths being equal in length, condensation will arrive at the same time as condensation, and reenforce it, and rarefaction will, similarly, reenforce rarefaction. But should one path be a little shorter than the other, rarefaction by one and condensation by the other may arrive at the same time, and at this point there will be comparative silence. . . . When the note changes in pitch the interference system is entirely altered in character. A single incident will serve to illustrate this point. There is a room in the Jefferson Physical Laboratory, known as the constant-temperature room. . . . While working in this room with a treble *c* . . . organ pipe blown by a steady wind-pressure, it was observed that the pitch of the pipe apparently changed an octave when the observer straightened up in his chair from a position in which he was leaning forward. The explanation is this: The organ pipe did not give a single pure note, but gave a fundamental treble *c* accompanied by several overtones, of which the strongest was in this case the octave above. Each note in the whole complex sound had its own interference system, which, as long as the sound remained constant, remained fixed in position. It so happened that at these two points the region of silence for one note coincided with the region of reenforcement in the other, and *vice versa.*

Sound, being energy, once produced in a confined space, will continue until it is either transmitted by the boundary walls, or is transformed into some other kind of energy, generally heat. This process of decay is called absorption. Thus, in the lecture room of Harvard University, in which, and in behalf of which, this investigation was begun, the rate of absorption was so small that a word spoken in an ordinary tone of voice was audible for five and a half seconds afterwards. During this time even a very deliberate speaker would have uttered the twelve or fifteen succeeding syllables. Thus the successive enunciations blended into a loud sound, through which and above which it was necessary to hear and distinguish the orderly progression of the speech. Across the room this could not be done. . . . This [disturbance] may be regarded, if one so chooses, as a process of multiple reflection from walls, from ceiling and from floor, first from one and then another, losing a little at each reflection until ultimately inaudible. This phenomenon will be called reverberation, including as a special case the echo.

So much from Sabine by way of analysis and illustration of the conditions to be dealt with in architectural acoustics. His final method of experimental attack on the particular difficulty presented by the lecture room in question, that of the Fogg Art Museum, now appears, and, since his work in this room gives us the first known instance of the rational and successful treatment of such a difficulty, the story should be told in his own words.

With an organ pipe as a constant source of sound, and a suitable chronograph for recording, the duration of audibility of a sound after the source had ceased in this room when empty was found to be 5.62 seconds.

All the cushions from the seats in Sanders Theatre were then brought over and stored in the lobby. On bringing into the lecture room a number of cushions having a total length of 8.2 meters, the duration of audibility fell to 5.33 seconds. On bringing in 17 meters the sound in the room after the organ pipe ceased was audible but 4.94 seconds. Evidently, the cushions were strong absorbents and rapidly improving the room, at least to the extent of diminishing the reverberation. The result was interesting and the process was continued. Little by little the cushions were brought into the room, and each time the duration of audibility was measured. When all the seats (436 in number) were covered, the sound was audible for 2.03 seconds. . . . In this lecture room felt was finally placed permanently on particular walls, and the room was rendered not excellent, but entirely serviceable. . . .

Let us note the extreme simplicity of the apparatus and method finally used by Sabine for studying the reverberation of a room. A standard horn or organ pipe was blown by means of a certain air pressure till the room was as full of sound as this source could make it. The action of the horn was then stopped by the push of a button which simultaneously recorded itself on the cylinder of a chronograph; a good observer[5] placed somewhere in the room listened intently but with unassisted ear, till the reverberation became inaudible and at this instant pushed another button and thus made another mark on the chronograph cylinder. The interval of time between the two records on the chronograph—that is, the duration of audible reverberation after the sound supply was cut off—could be measured to about 0.01 second.

Extremely simple in theory, but hard enough in practice; successfully carried out because the man born to do this thing and bound to do it, to break through the armor of difficulties hiding the secret of acoustics, had been found and had found his place. The work must be done "during the most quiet part of the night, between half-past twelve and five."

To secure accuracy, . . . it was necessary to suspend work on the approach of a street car within two blocks, or the passing of a train a mile distant. In Cambridge these interruptions were not serious; in Boston and in New York it was necessary to snatch observations in very brief intervals of quiet. In every case a single determination of the duration of the residual sound was based on the average of a large number of observations.

Three general conclusions which may be rather surprising to most people were derived from these observations, as applying to rooms of fairly regular shape:

1. *The duration of audibility of the residual sound is nearly the same in all parts of an auditorium.*

2. *The duration of audibility is nearly independent of the position of the source (the horn).*

3. *The efficiency of an absorbent in reducing the duration of a residual sound is, under ordinary circumstances, nearly independent of its position.*

All this comes from the fact that a sound wave emitted by the source is reflected back and forth across an ordinary room many times a second and in many directions, so that, when a horn has been sounding for a few seconds and has then stopped suddenly, all parts of the air of the room are about equally full of sound energy.

At first the unit of absorptive capacity employed by Sabine was that of a running meter of the Sanders Theater cushions mentioned in one of the preceding quotations. Thus, the absorbing power of the walls, floor, windows, etc., of a certain bare room was found to be equal to that of 146 running meters of these cushions. But presently it was found that the exposed vertical edge of a cushion counts as much per unit area as the exposed horizontal top.[6] So the square

[5] Mr. Gifford LeClear and Mr. E. D. Densmore assisted in this labor, each making his own observations of the duration of reverberation, for comparison with those of Sabine. This is an example of the latter's power, already mentioned, to enlist the interest of capable young men. On the other hand, he was able to give full recompense to LeClear and Densmore by getting professional work for them at the beginning of their career, now become distinguished, as building engineers. Another who should be mentioned here is Mr. John Connors, head janitor of the Jefferson Physical Laboratory, a man of remarkable resourcefulness and practical good sense. He came from Ireland about the time Sabine came from Ohio. They were nearly of the same age and though, naturally, very unlike in many respects, were close friends. When Sabine died, "John" knew more about his experiments and his plans than did any one of the teaching staff.

[6] This fact was brought out in striking fashion after experiments made with cushions having one edge pushed against the backs of settees had given anomalous results. "It was then recalled that about two years before, at the beginning of an evening's work, the first lot of cushions brought into the room were placed on the floor, side by side, with edges touching, but that after a few observations had been taken the cushions were scattered about the room, and the work was repeated. This was done not at all to uncover the edges, but in the primitive uncertainty as to whether near cushions would draw from each other's supply of sound, as it were, and thus diminish each other's efficiency. No further thought was then given to these discarded observations until recalled by the above-mentioned discrepancy. They were sought out from the notes of that period, and it was found that, as suspected, the absorbing power of the cushions when touching edges was less than when separated. Eight cushions had been used and, therefore, fourteen edges had been touching. A record was found of the length and the breadth of the cushions used and, assuming that the absorbing power was proportional to the area exposed, it was possible to calculate their thickness by comparing the audible duration of the residual sound in the two sets of observations; it was thus calculated to be 7.4 centimeters. On stacking up the same cushions and measuring their total thickness, the average thickness was found to be 7.2 centimeters, in very close agreement with the thickness estimated from their absorption of sound. Therefore, the measurements of the cushions should be, not in running meters of cushion, but in square meters of exposed surface."

meter of cushion replaced the running meter as a convenient unit of reference, though a square meter of open window was taken as the final standard. The general relation of these two units is brought out in the following passage:

For the purposes of the present investigation, it is wholly unnecessary to distinguish between the transformation of the energy of the sound into heat and its transmission into outside space. Both shall be called absorption. The former is the special accomplishment of cushions, the latter of open windows. It is obvious, however, that if both cushions and windows are to be classed as absorbents, the open window, because the more universally accessible and the more permanent, is the better unit. The cushions, on the other hand, are by far the more convenient in practice, for it is possible only on very rare occasions to work accurately with the windows open, not at all in summer on account of night noises—the noise of crickets and other insects—and in the winter only when there is but the slightest wind; and further, but few rooms have sufficient window surface to produce the desired absorption. It is necessary, therefore, to work with cushions, but to express the results in open-window units.

Having no money,[7] in the early years of his investigation at least, for building structures such as he would like to study, Sabine was obliged to depend on chance opportunities to measure the influence of different building materials. Sometimes these opportunities were of a kind not likely to recur, and we should sadly underestimate the merit of his achievements if we failed to note the swiftness and finality of his action in such cases.

This is his story of one; evidently the new rooms described might never be empty again:

Through the kindness of Professor Goodale, an excellent opportunity for securing some fundamentally interesting data was afforded by the new botanical laboratory and greenhouse recently given to the university. These rooms—the office, the laboratory, and the greenhouse—were exclusively finished in hard-pine sheathing, glass, and cement; the three rooms, fortunately, combined the three materials in very different proportions. They and the constant-temperature room in the physical laboratory—the latter being almost wholly of brick and cement—gave the following data:

	Area of hard-pine sheathing	Area of glass	Area of brick and cement	Combined absorbing power
Office	127.0	7	0	8.37
Laboratory	84.8	6	30	5.14
Greenhouse	12.7	80	85	4.64
Constant-temperature room	2.1	0	124	3.08

This table gives for the three components the following coefficients of absorption: Hard-pine sheathing, 0.058; glass, 0.024; brick set in cement, 0.023.

Another case of conditions difficult to obtain is described as follows:

Next in interest to the absorbing power of wall surfaces is that of an audience. During the summer of 1897, at the close of a lecture in the Fogg Art Museum, the duration of the residual sound was determined before and immediately after the audience left. The patience of the audience and the silence preserved left nothing to be desired in this direction, but a slight rain falling on the roof seriously interfered with the observations. Nevertheless, the result, 0.37 per person, is worthy of record. The experiment was tried again in the summer of 1899, on a much more elaborate scale and under the most favorable conditions, in the large lecture room of the Jefferson Physical Laboratory. In order to get as much data and from as independent sources as possible, three chronographs were electrically connected with each other and with the electro-pneumatic valve controlling the air supply of the organ pipe. One chronograph was on the lecture table, and the others were on opposite sides in the rear of the hall. The one on the table was in charge of the writer, who also controlled the key turning on and off

[7] The following letter gives some indication of that severe scrupulosity which was inveterate in Sabine:

HARVARD UNIVERSITY,
Cambridge, November 12th, 1897.

DEAR MR. SABINE: Your explanation of November 3rd about your expenditures in making the investigation which Mr Hooper and I asked you to make is very far from being satisfactory. You have made sufficient progress to be able to prescribe for the Fogg Lecture-room, and you are going to make that prescription. What the Corporation wants is to pay all the costs to this date of that investigation, not of those experiments only which certainly contributed to the result, but of all the experiments made with that object in view which Mr. Hooper and I set before you. Unless you enable the Corporation to do this by rendering an account of your expenditures, you leave the Corporation in the position of having engaged you can work in their interest which not only cost you much time and labor, but also cost you money. It seems to me that on reflection you will perceive that this is not a suitable relation for the Corporation to be left in with one of its assistant professors. You will of course be at liberty to continue the investigation at your discretion, and at your own charge; but up to this time all charges ought to be paid by the Corporation, including the travelling expenses, admission tickets, and the purchase of instruments. These expenses do not require any justification—they are matters of course in such an inquiry.

Very truly yours,

(signed) CHARLES W. ELIOT

Professor W. C. SABINE

the current at the four instruments. The two other chronographs were in charge of other observers, provision being thus made for three independent determinations. After a test had been made of the absorbing power of the whole audience—157 women and 135 men, sufficient to crowd the lecture room—one half, by request, passed out, 63 women and 79 men remaining, and observations were again made. On the following night the lecture was repeated and observations were again taken, there being present 95 women and 73 men. There were thus six independent determinations on three different audiences and by three observers. In the following table the first column of figures gives the total absorbing power of the audience present; the second gives the absorbing power per person; the initials indicate the observer.

	Observer	Total absorbing power	Absorbing power per person
First night, whole audience	W. C. S.	123.0	0.42
Do	G. LeC.	113.0	.39
First night, half audience	W. C. S.	58.3	.41
Do	G. LeC.	58.3	.41
Second night, whole audience	W. C. S.	66.2	.40
Do	E. D. D.	64.6	.39
			[1] .40(3)

[1] This is additional to the absorbing power of the seat and floor area covered by the person. Correction on this account makes the "absolute" absorbing power per person 0.44—that is, 0.44 of the absorbing power of a square meter of open window.

E. H. H.

In view of the difficulties of the experiment the consistency of the determination is gratifying. The average result of the six determinations is probably correct within 2 per cent.

The following passage illustrates the nicety of observation aimed at and attained in these tests:

Under certain circumstances the audience will not be compactly seated, but will be scattered about the room and more or less isolated, for example, in a council room, or in a private music room, and it is evident that under these conditions the individual will expose a greater surface to the room and his absorbing power will be greater. It is a matter of the greatest ease to distinguish between men and women coming into a small room, or even between different men. In fact, early in the investigation, two months' work—over three thousand observations—had to be discarded because of failure to record the kind of clothing worn by the observer. The coefficients given in the following table are averages for three women and for seven men, and were deduced from experiments in the constant-temperature room.

Absorbing power of an audience

Audience per square meter	0.96
Audience per person	.44
Isolated woman	.54
Isolated man	.48

Evidently, the absorbing power of an "isolated woman" as compared with that of an "isolated man" must vary with the fashions in clothing.

Along with this study of the absorbing power of different objects and materials went the determination of a mathematical formula[8]—

$$t = k \div (a + x),$$

in which t is the number of seconds the residual sound lasts—the "duration of audibility,"—k is a constant quantity depending on the size of the room, a is the absorbing power of the bare walls, floor, and ceiling, and x is the increase of absorbing power due to the furniture and audience.

Studying rooms of different shapes and sizes, Sabine found that k is approximately $0.171V$, where V is the volume of a room in cubic meters. Then, knowing the absorbing power of different surfaces and the area of these several surfaces, he could calculate, even in advance of construction, how long the reverberation would last in a given auditorium.

Of course this feature is not the whole of architectural acoustics, but it is one of the most important parts, and it is the only part discussed in the following passage relating to the design of Boston Symphony Hall, the first auditorium to be affected, in construction, by Sabine's advice. It is greatly to the credit of the distinguished architects of this building, Messrs.

[8] This is the simple, approximate, formula to indicate the nature of the relation discovered. The exact formula is much too complicated for useful reproduction here.

McKim, Mead & White, that they were the first of their profession to appreciate the acoustic studies made by a young and little-known physicist and to change their plans in accordance with his criticisms. The following is a part of his account of the matter:

In a theatre for dramatic performances, where the music is of entirely subordinate importance, it is desirable to reduce the reverberation to the lowest possible value in all ways not inimical to loudness; but in a music hall, concert room, or opera house, this is decidedly not the case. To reduce the reverberation in a hall to a minimum, or to make the conditions such that it is very great, may, in certain cases, present practical difficulties to the architect—theoretically it presents none. To adjust, in original design, the reverberation of a hall to a particular and approved value requires a study of conditions, of materials, and of arrangement, for which it has been the object of the preceding papers to prepare.

It is not at all difficult to show *a priori* that in a hall for orchestral music the reverberation should neither be very great, nor, on the other hand, extremely small. However, in this matter it was not necessary to rely on theoretical considerations. Mr. Gericke, the conductor of the Boston Symphony Orchestra, made the statement that an orchestra, meaning by this a symphony orchestra, is never heard to the best advantage in a theatre, that the sound seems oppressed, and that a certain amount of reverberation is necessary. An examination of all the available plans of the halls cited as more or less satisfactory models, in the preliminary discussion of the plans for the new hall, showed that they were such as to give greater reverberation than the ordinary theatre style of construction. While several plans were thus cursorily examined the real discussion was based on only two buildings—the present [old] Boston Music Hall and the Leipzig Gewandhaus; one was familiar to all and immediately accessible, the other familiar to a number of those in consultation, and its plans in great detail were to be found in *Das neue Gewandhaus in Leipzig, von Paul Gropius und H. Schmieden.* It should, perhaps, be immediately added that neither hall served as a model architecturally, but that both were used rather as definitions and starting points on the acoustical side of the discussion. The old Music Hall was not a desirable model in every respect, even acoustically, and the Leipzig Gewandhaus, having a seating capacity about that of Sanders Theatre, 1,500, was so small as to be debarred from serving directly, for this if for no other reason.

The history of the new hall is about as follows: A number of years ago, when the subject was first agitated, Mr. McKim prepared plans and a model along classical lines of a most attractive auditorium, and afterwards, at Mr. Higginson's instance, visited Europe for the purpose of consulting with musical and scientific authorities in France and Germany. But the Greek theatre as a music hall was an untried experiment, and because untried was regarded as of uncertain merits for the purpose by the conductors consulted by Mr. Higginson and Mr. McKim. It was, therefore, abandoned. Ten years later, when the project was again revived, the conventional rectangular form was adopted, and the intention of the building committee was to follow the general proportions and arrangement of the Leipzig Gewandhaus, so enlarged as to increase its seating capacity about seventy per cent; thus making it a little more than equal to the old hall [of Boston]. At this stage calculation was first applied.

The often-repeated statement that a copy of an auditorium does not necessarily possess the same acoustical qualities is not justified, and invests the subject with an unwarranted mysticism. The fact is that exact copies have rarely been made, and can hardly be expected. The constant changes and improvements in the materials used for interior construction in the line of better fire-proofing—wire lath or the application of the plaster directly to tile walls—have led to the taking of liberties in what were perhaps regarded as nonessentials; this has resulted, as shown by the tables, in a changed absorbing power of the walls. Our increasing demands in regard to heat and ventilation, the restriction on the dimensions enforced by location, the changes in size imposed by the demands for seating capacity, have prevented, in different degrees, copies from being copies, and models from successfully serving as models. So different have been the results under what was thought to be safe guidance—but a guidance imperfectly followed—that the belief has become current that the whole subject is beyond control. Had the new Music Hall been enlarged from the Leipzig Gewandhaus to increase the seating capacity seventy per cent, which, proportions being preserved, would have doubled the volume, and then built, as it is being built, according to the most modern methods of fireproof construction, the result, unfortunately, would have been to confirm the belief. No mistake is more easy to make than that of copying an auditorium—but in different materials or on a different scale—in the expectation that the result will be the same. Every departure must be compensated by some other—a change in material by a change in the size or distribution of the audience, or perhaps by a partly compensating change in the material used in some other part of the hall—a change in size by a change in the proportions or shape. For moderate departures from the model such compensation can be made, and the model will serve well as a guide to a first approximation. When the departure is great the approved auditorium, unless discriminatingly used, is liable to be a treacherous guide. In this case the departure was necessarily great.

The comparison of halls should be based on the duration of the residual sound after the cessation of a source that has produced over the hall some standard average intensity of sound—say one million times the minimum audible intensity, $1{,}000{,}000i'$.

From the known dimensions and materials of the Leipzig Gewandhaus, Sabine found 2.30 seconds as the duration of reverberation of tone C_4, 512 vibrations, therein, and his calculations foretold that the new Symphony Hall, the architects following his suggestions, would reverberate

2.31 seconds with the same tone. The practical results satisfied the director, Mr. Gericke, at once and have now been approved by the audiences of many years.

It is interesting to note, in passing, that Messrs. Wheelwright and Haven, architects in 1902 of a new building for the New England Conservatory of Music, called on Sabine for help in regard to the acoustics of moderate-sized rooms intended for piano practice. Seeing that he had to do here with a question of musical taste, not one of physics merely, he experimented with five different rooms, varying the furnishings in each till a jury of musicians declared themselves satisfied. Then, measuring the duration of reverberation of C_4 in each of the rooms, as approved, he found the shortest time to be 0.95 second and the longest 1.16 seconds, the mean being 1.08 seconds, less than one-half the approved time for a symphony concert hall.

Sabine tells us (p. 199 of his Collected Papers) that investigation of the absorption coefficients of different materials for the single note of violin C, the "C_4" mentioned above, "required every other night from twelve until five for a period of three years,"[9] yet this study, vastly important as it had proved to be, was not enough:

> It can be shown readily that the various materials of which the walls of a room are constructed and the materials with which it is filled do not have the same absorbing power for all sounds regardless of pitch. Under such circumstances the previously published work with C_4 512 must be regarded as an illustration, as a part of a much larger problem—the most interesting part, it is true, because near the middle of the scale, but, after all, only a part. Thus a room may have great reverberation for sounds of low pitch and very little for sounds of high pitch, or exactly the reverse; or a room may have comparatively great reverberation for sounds both of high and of low pitch and very little for sounds near the middle of the scale. In other words, it is not putting it too strongly to say that a room may have very different quality in different registers, as different as does a musical instrument; or, if the room is to be used for speaking purposes, it may have different degrees of excellence or defect for a whisper and for the full rounded tones of the voice, different for a woman's voice and for a man's—facts more or less well recognized. Not to leave this as a vague generalization, the following cases may be cited. Recently, in discussing the acoustics of the proposed cathedral of southern California in Los Angeles with Mr. Maginnis, its architect, and the writer, Bishop Conaty touched on this point very clearly. After discussing the general subject with more than the usual insight and experience, possibly in part because Catholic churches and cathedrals have very great reverberation, he added that he found it difficult to avoid pitching his voice to that note which the auditorium most prolongs notwithstanding the fact that he found this the worst pitch on which to speak. This brings out, perhaps more impressively because from practical experience instead of from theoretical considerations, the two truths that auditoriums have very different reverberation for different pitches, and that excessive reverberation is a great hindrance to clearness of enunciation. Another incident may also serve, that of a church near Boston, in regard to which the writer has just been consulted. The present pastor, in describing the nature of its acoustical defects, stated that different speakers had different degrees of difficulty in making themselves heard; that he had no difficulty, having a rather high pitched voice; but that the candidate before him, with a louder but much lower voice, failed of the appointment because unable to make himself heard.

Accordingly, about 1900, Sabine undertook an extension of his investigations to cover nearly "the whole range in pitch of the speaking voice and of the musical scale," from C_1, of 64 vibrations per second, to C_7, of 4,096 vibrations. Carrying meanwhile his full share of work as a conscientious teacher and seizing time and opportunity as best he could for research, he was engaged about five years with this new labor.

The difficulties and devices of the undertaking can best be shown by certain quotations:

> In the very nature of the problem, the most important datum is the absorption coefficient of an audience, and the determination of this was the first task undertaken. By means of a lecture on one of the recent developments of physics, wireless telegraphy, an audience was thus drawn together and at the end of the lecture requested to remain for the experiment. In this attempt the effort was made to determine the coefficients for the five octaves from C_2 128 to C_6 2048, including notes E and G in each octave. For several reasons the experiment was not a success. A threatening thunderstorm made the audience a small one, and the sultriness of the atmosphere made open windows necessary, while the attempt to cover so many notes, thirteen in all, prolonged the experiment beyond the endurance of the audience. While this experiment failed, another the following summer was more successful. In the year that had elapsed the necessity of carrying the investigation further than the limits intended became evident, and now the experiment was carried from C_1 64 to C_7 4096, but included only the C notes,

[9] It is not surprising that in 1899 Sabine had an attack of appendicitis that very nearly proved fatal. With his strange, fanatical disregard for pain, he had ignored all warning symptoms, had kept on his feet, and was out of doors, on Cambridge Common I believe, when the abscess broke. As soon as he was out of immediate danger he laughed at this adventure and announced his intention of making a record recovery, which he did. He was two weeks in the hospital, one week at home, and then began the work of the opening college year, "blithely and smilingly with springing step," to use his mother's words.

seven notes in all. Moreover, bearing in mind the experiences of the previous summer, it was recognized that even seven notes would come dangerously near overtaxing the patience of the audience. Inasmuch as the coefficient of absorption for C_4 512 had already been determined six years before, in the investigations mentioned, the coefficient for this note was not redetermined. The experiment was therefore carried out for the lower three and the upper three notes of the seven. The audience, on the night of this experiment, was much larger than that which came the previous summer, the night was a more comfortable one, and it was possible to close the windows during the experiment. The conditions were thus fairly satisfactory. In order to get as much data as possible, and in as short a time, there were nine observers stationed at different points in the room. These observers, whose kindness and skill it is a pleasure to acknowledge, had prepared themselves, by previous practice, for this one experiment.

The next experiment was on the determination of the absorption of sound by wood sheathing. It is not an easy matter to find conditions suitable for this experiment. . . . Quite a little searching in the neighborhood of Boston failed to discover an entirely suitable room. The best one available adjoined a night lunch room. The night lunch was bought out for a couple of nights, and the experiment was tried. The work of both nights was much disturbed. The traffic past the building did not stop until nearly two o'clock, and began again at four. The interest of those passing on foot throughout the night, and the necessity of repeated explanations to the police, greatly interfered with the work.

The main purpose of Sabine's work was, of course, utilitarian, though in a highly refined sense, and we shall presently go on to show how he dealt with the acoustic problems of particular buildings, sometimes before and sometimes after their construction. But we must not overlook an important contribution [10] which he made in 1907 to the theory of the musical scale, or, rather, to the theory of the origin of this scale. Referring to Helmholtz and his Sensations of Tone, he says:

Having given a physical and physiological explanation of the harmony and discord of simultaneous sounds and, therefore, an explanation of the musical scale as used in modern composition, Helmholtz was met by an apparent anachronism. The musical scale, identical with the modern musical scale in all essentials, antedated by its use in single-part melody the invention of chordal composition, or, as Helmholtz expressed it, preceded all experience of musical harmony. In seeking an explanation of this early invention of the musical scale, Helmholtz abandoned his most notable contribution, and relegated his explanation of harmony and discord to the minor service of explaining a fortunate, though of course an important use of an already invented system of musical notes. The explanation of the original invention of the musical scale and its use in single-part music through the classical and the early Christian eras, he sought for in purely aesthetic considerations—in exactly those devices from which he had just succeeded in rescuing the explanation of harmony and discord.

Sabine's explanation of the anachronism that troubled Helmholtz might, perhaps, be surmised by anyone who had read the preceding pages, but it is well to give it in his own words:

In many rooms of ordinary construction the prolongation of audibility amounts to two or three seconds, and it is not exceedingly rare that a sound of moderate initial intensity should continue audible for eight, nine, or even ten seconds after the source has ceased. As a result of this, single-part music produced as successive separate sounds is, nevertheless, heard as overlapping, and at times as greatly overlapping tones. Each note may well be audible with appreciable intensity not merely through the next, but through several succeeding notes. Under such conditions we have every opportunity, even with single-part music, for the production of all the phenomena of harmony and discord which has been discussed by Helmholtz in explanation of the chordal use of the musical scale. In any ordinarily bare and uncarpeted room, one may sing in succession a series of notes and then hear for some time afterward their full chordal effect.

But Sabine goes further and suggests a physical explanation of the differences of musical scale developed by different races:

Housed or unhoused, dwelling in reed huts or in tents, in houses of wood or of stone, in houses and temples high vaulted or low roofed, of heavy furnishing or light, in these conditions we may look for the factors which determine the development of a musical scale in any race, which determine the rapidity of the growth of the scale, its richness, and its considerable use in single-part melody.

We have explained for us by these figures [absorptive powers of various materials] why the musical scale has but slowly developed in the greater part of Asia and of Africa. Almost no traveler has reported a musical scale, even of the most primitive sort, among any of the previously unvisited tribes of Africa. This fact could not be ascribed to racial inaptitude. If melody was, as Helmholtz suggested, but rhythm in time and in pitch, the musical scale should have been developed in Africa if anywhere. These races were given to the most rhythmical dancing, and the rhythmical beating of drums and tomtoms. Rhythm in time they certainly had. Moreover, failure to develop a musical scale could not be ascribed to racial inaptitude to feeling for pitch. Transported to America and brought in contact with the musical scale, the negro became immediately the most musical part of our population. The absence of a highly developed scale in Africa must then be ascribed to environment.

[10] Vice presidential address, Section B, American Association for the Advancement of Science, Chicago, 1907.

Turning to Europe we find the musical scale most rapidly developing among the stone-dwelling people along the shores of the Mediterranean. The development of the scale and its increased use kept pace with the increased size of the dwellings and temples. It showed above all in their religious worship, as their temples and churches reached cathedral size. The reverberation which accompanied the lofty and magnificent architecture increased until even the spoken service became intoned in the Gregorian chant. It is not going beyond the bounds of reason to say that in those churches in Europe, which are housed in magnificent cathedrals, the Catholic, the Lutheran, and Protestant Episcopal, the form of worship is in part determined by their acoustical conditions.

The passages thus far quoted from Sabine's writings say little about the form of auditoriums or the position of absorbing or reflecting surfaces, and they might lead one to think these matters unimportant. This would be a serious mistake. When he came to deal with an auditorium so irregular in shape as a modern theater, with boxes and two or three deep galleries, under the requirement that words spoken on the stage should be easily heard everywhere, the problem was not merely, or perhaps mainly, that of reverberation. It was necessary to consider whether the details of shape and surface, actual or proposed, were such as to kill off the sound by "interference" in certain places or to confuse it by an echo.

This problem was not difficult, though it was laborious, for Sabine, when he was consulted in advance of the construction, as he was in the case of the Little Theater in New York City or the Scollay Square Theater in Boston.

As to the plan of the New Theater in New York, his advice was asked only after construction of the building. The architects had undertaken the task of making a very large auditorium suitable for both opera and drama, and, naturally, had not been entirely successful. Sabine advised certain changes of position of boxes and foyer chairs and recommended lowering the ceiling. The minor changes proposed were made, but instead of lowering the ceiling, the architects suspended beneath it a large oval canopy, 70 by 40 feet, with good effect.

Concerning open-air theaters, Sabine remarks, after observing that the presence of an audience diminishes reverberation:

But in the Greek theatre, occupied or unoccupied, ruined or in its original form, there was very little reverberation. In fact, this was its merit. On the other hand, the very fact that there was little reverberation is significant that there was very slight architectural reenforcement of the voice. One might well be unconvinced by such *a priori* considerations were there not excellent evidence that these theatres were not wholly acceptable acoustically even in their day, and for drama written for and more or less adapted to them. Excellent evidence that there was insufficient consonance [11] is to be found in the megaphone mouthpieces used at times in both the tragic and the comic masks, and in the proposal by Vitruvius to use resonant vases to strengthen the voice.

Apparently the acoustical difficulties presented by churches, though often serious, are not usually so hard to deal with as those found in theaters, perhaps because church audiences are not so insistent as theater audiences on hearing every word that is uttered. The first of the following passage describes one interesting church case:

Among a number of interesting problems in advance of construction, the firm of McKim, Mead & White has brought some interesting problems in correction, of which three will serve admirably as examples because of their unusual directness. The first is that of the Congregational Church in Naugatuck, Connecticut. . . . When built its ceiling was cylindrical, as now, but smooth. Its curvature was such as to focus a voice from the platform upon the audience—not at a point, but along a focal line, for a cylindrical mirror is astigmatic. The difficulty was evident with the speaking, but may be described more effectually with reference to the singing. The position of the choir was behind the preacher and across the main axis of the church. On one line in the audience, crossing the church obliquely from right to left, the soprano voice could be heard coming even more sharply from the ceiling than directly from the singer. The alto starting nearer the axis of the church had for its focus a line crossing the church less obliquely. The phenomena were similar for the tenor and the bass voices, but with focal lines crossing the church obliquely in opposite directions. The difficulty was in a very large measure remedied by coffering the ceiling, . . . both the old and the new ceiling being of plaster.

The hall of the House of Representatives in the Rhode Island State capitol illustrated another type of difficulty. In considering this hall it is necessary to bear in mind that the problem is an essentially different one from that of a church or lecture-room. In these the speaking is from a raised platform and a fixed position. In a legislative assembly the speaking is in the main from the floor, and may be from any part of the floor.

In this legislative hall Sabine diagnosed the trouble as an effect not very different from an echo, due to reflection first from wall to wall and then from ceiling to audience.

[11] Sabine takes this word from Vitruvius, *De Architectura*, Liber V, Cap. VIII. It means action by which the voice is "supported and strengthened."

The difficulty was remedied in this case by a change in material without change of form, by diminishing the reflecting power of the two side walls. This was done by placing a suitable felt on the plaster walls between the engaged columns, and covering it with a decorated tapestry.

It is interesting to note that this treatment applied to the lower half of the walls would not have been acoustically effective.

The lecture-room of the Metropoltian Museum of Art illustrates the next step in complexity. . . . In this room the reverberation was not merely excessive, but it resolved itself by focusing into a multiple echo, the components of which followed each other with great rapidity but were distinctly separable. The number distinguishable varied in different parts of the hall. Seven were distinguishable at certain parts.

Certain remedial measures were taken on Sabine's advice, and the result was the one predicted—

the reduction of the disturbance to a single and highly localized echo. This echo is audible only in the central seats—two or three seats at a time—and moves about as the speaker moves, but in symmetrically opposite direction. Despite this residual effect, and it should be noted that this residual effect was predicted, the result is highly satisfactory to Dr. Edward Robinson, the director of the museum, and the room is now used with comfort, whereas it had been for a year abandoned.

This is perhaps the most fitting place for reference to Sabine's exceedingly interesting chapter on "Whispering galleries," which is to be found at present only in the volume of his Collected Papers on Acoustics, issued in 1922 by the Harvard University Press. I should like to reproduce here the whole of it, if such a proceeding were consistent with the usual scope of memoirs like the one now in hand, but I can give only two or three short passages:

It is probable that all existing whispering galleries, it is certain that the six more famous ones, are accidents; it is equally certain that all could have been predetermined without difficulty, and like most accidents, could have been improved upon. That these six, the dome of St. Paul's Cathedral in London, Statuary Hall in the Capitol at Washington, the vases in the Salle des Cariatides in the Louvre in Paris, St. John Lateran in Rome, the Ear of Dionysius at Syracuse, and the Cathedral of Girgenti, are famous above others is in a measure due to some incident of place or association.

The ceiling of the Hall of Statues [in Washington], with the exception of a small circular skylight, is a portion of an exact sphere with its center very nearly at head level. . . .

In citing this gallery in an article on "Whispering galleries" in Sturgis's Dictionary of Architecture, the writer made the statement that "The ceiling, painted so that it appears deeply panelled, is smooth. Had the ceiling been panelled the reflection would have been irregular and the effect very much reduced." A year or so after this was written the fire in the Capitol occurred, and in order to preserve the whispering gallery, which had become an object of unfailing interest to visitors to the Capitol, the new ceiling was made "to conform within a fraction of an inch" to the dimensions of the ceiling which it replaced. Notwithstanding this care, the quality of the room which had long made it the best and the best known of whispering galleries was in large measure lost. Since then this occurrence has been frequently cited as another of the mysteries of architectural acoustics and a disproof of the possibilities of predicting such phenomena. As a matter of fact, it was exactly the reverse. Only the part between the panels was reproduced in the original dimensions of the dome. The ceiling was no longer smooth, the staff was panelled in real recess and relief, and the result but confirmed the statement recorded nearly two years before in the Dictionary of Architecture.

Almost any wall-surface is a much more perfect reflector of sound than the most perfect silver mirror is to light. In the former case, the reflection is over 96 per cent, in the latter case rarely over 90.

On the surfaces of the two mirrors scratches to produce equally injurious effects must be comparable in their dimensions to the lengths of the waves reflected. Audible sounds have wave lengths of from half an inch to sixty feet; visible light of from one forty-thousandth to one eighty-thousandth of an inch. Therefore while an optical mirror can be scratched to the complete diffusion of the reflected light by irregularities of microscopical dimensions, an acoustical mirror to be correspondingly scratched must be broken by irregularities of the dimensions of deep coffers, of panels, of engaged columns, or of pilasters.

From this last passage, and from others declaring that a rough plastered surface acts practically just like a smooth one, so far as sound waves are concerned, one might infer that merely painting a solid wall could have no appreciable effect on its acoustic properties; but this would be an error. Sabine had found the absorptive power of a painted brick wall to be only about half that of the same kind of wall unpainted. This is because of the porosity of natural brick, which enables the sound waves to penetrate the material slightly and so lose a little of their energy. Gradually, through Sabine's suggestions and "the skill and great knowledge of ceramic processes" possessed by Mr. Raphael Guastavino, a kind of tile was developed which "has over sixfold the absorbing power of any existing masonry construction and one-third the absorbing power of the best-known felt."

The words just quoted are found in a paper printed in 1914. We may say that, so far as the properties of auditoriums are concerned, Sabine had, in less than 20 years, brought architectural acoustics from the empirical state, in which success with any new structure was a happy accident and failure was a misfortune often made ridiculous by such attempted remedies as the stringing of wires, to the status of a reasoned science and a precise art.

He had done this by force of his own qualities, with but little favor of circumstances and with so little financial assistance or reward that he was probably a poorer man by thousands of dollars than he would have been had he never attempted it. Moreover, he had published his formulas and his devices freely [12] to the world, for anyone to use who could, patenting only, and this with Mr. Guastavino, the kind of tile described above.

But one great practical difficulty in acoustics remained, the nature of which is shown in the following passage quoted from a paper of Sabine's printed in 1915:

> The insulation of sound as an unsolved problem in architectural acoustics was first brought to the writer's attention by the New England Conservatory of Music, immediately after its completion in 1904, and almost simultaneously in connection with a private house which had just been completed in New York. A few years later it was renewed by the Institute of Musical Art in New York. In the construction of all three buildings it had been regarded as particularly important that communication of sound from room to room should be avoided, and methods to that end had been employed which were in every way reasonable. The results showed that in this phase of architectural acoustics also there had not been a sufficiently searching and practical investigation and that there were no experimental data on which an architect could rely.

In some respects the attempts of well-known architects at sound insulation had been weirdly unsuccessful. Thus, concerning the private house mentioned above, we have the following particulars:

> It was practically a double house, one of the most imperative conditions of the building being the exclusion of sounds in the main part of the house from the part to the left of a great partition wall. In the basement of the main dwelling was the servants' dining room. Rapping with the knuckles on the wall of this room produced in the bedroom, two stories up and on the other side of the great partition wall, a sound which, although hardly, as the architect expressed it, magnified, [was] yet of astonishing loudness and clearness.

Sabine's analysis of the sound insulation problem, perhaps the most difficult that he encountered in acoustics, is well shown in the following paragraph, which, though not put in quotation marks, is made up almost entirely of parts to be found in his paper:

The transmission of sound from one room to another involves three steps—the taking up of the vibration from the air by the solid partition, its transmission through the partition, and its communication to the air of the receiving room. In the case of a solid masonry wall, the transmission from surface to surface is almost perfect; but, because of the great mass and rigidity of the wall, it takes up but little of the vibration of the incident sound. In the case of multiple screen walls, the communication from wall to wall, through the intermediate air space or around the edges, is poor compared with the face to face communication of a solid wall. But the vibration of the screen wall exposed to the sound, the initial step in the process of transmission, is greatly enhanced by its light and flexible character. Similarly its counterpart, the screen wall which by its vibration communicates the sound to the receiving room, is light, flexible, and responsive to relatively small forces. This responsiveness of the screens may compensate or more than compensate for the poor communication between them.

In his studies of sound insulation Sabine used layers of felt, then sheet-iron partitions with air spaces between, then such partitions with layers of felt and of air between. With the last combination, made about 6 inches thick in all, sound of 1,000,000 times minimum audibility

[12] At least one individual thought Sabine was neglecting a golden opportunity. In 1909 a certain young architect, ———, who had dealt successfully, by means of Sabine's formulas and methods, with the acoustic troubles of a certain synagogue and had received a good round sum for his work, visited him, asked him many technical questions, and tried to get a monopoly of his advice for a company ——— proposed to form. Sabine declining this proposition, ——— then undertook to patent, as his own, all of Sabine's formulas and methods. The first intimation to Sabine of this maneuver came in a letter from Mr. Mead, of McKim, Mead & White, saying that ——— had served an injunction on his firm to restrain it from using suggestions made by Sabine for improvement of the acoustic qualities of the lecture room of the Metropolitan Museum of Fine Arts. Fortunately ——— had been a little too enterprising, for though, strangely enough, his claims had been allowed, the patents had not yet been issued. Prompt and vigorous action, in which Mr. Frederick P. Fish, one or two United States Senators, and, I believe, President Taft, had a part, checked Mr. ——— at the Patent Office. In the subsequent legal proceedings testimony of the most positive character was given, showing that Sabine had, previous to 1908, prescribed alterations for improvement of the acoustic properties of seven auditoriums and that in all of these the changes made had been proved successful by years of experience. No evidence was offered by ———, and his application for patents was finally rejected.

on one side of the barrier gave sound of 88 times minimum audibility on the other side. The sound used in this test was that of violin C, 512 vibrations per second.

Sabine makes the interesting observation that, for a thickness of 5 or 6 inches, the combination above described is far superior in absorptive effect to an equal thickness of felt; but felt of 10.4 inches thickness "would entirely extinguish a sound of the intensity of ordinary speech," whereas 10.4 inches of the combination described would not "accomplish this ideal result."

Notable as are these evidences of progress with the problem of sound insulation, it is plain that Sabine regarded his work in this field as only well begun. His chapter on the "Insulation of sound" ends thus:

> At this point the apparatus was improved, the method recast, and the investigation begun anew, thenceforward to deal only with standard forms of construction, and for sounds, not of one pitch only, but for the whole range of the musical scale.

These words, published in 1915, were doubtless written in anticipation of opportunities soon to be afforded him by the liberality of a friend. Colonel Fabyan, an energetic and successful merchant of Chicago, a man of various avocations and enthusiasms, had somehow become acquainted with and deeply interested in Sabine's acoustic investigations. He offered to build, and during the war he did build, at Geneva, Ill., a research laboratory to suit exactly Sabine's purposes. It was planned for the prosecution of work under Sabine's direction, and no expense necessary to the success of this work was to be spared. In fact, this research laboratory was intended by Colonel Fabyan to be also a kind of shrine or temple in celebration of Sabine himself, and to this end there were inscribed on its outer walls four words, names of virtues found in Sabine, and so chosen that the initials of these words were his initials. Sabine smiled appreciatingly and went on with his plans, taking such opportunities as came to him during the stress of war days. Seldom have two so unlike men worked together so happily for a beneficent purpose.

Sabine was not much given to putting his ideas on paper until they were in final shape. So after his death, in 1919, no elaborated scheme of the work he had proposed to do in the new laboratory could be found. Colonel Fabyan, resolved to do all he could to carry out the enterprise in which he and Sabine had engaged with such high hopes, enlisted Dr. Paul E. Sabine, who is a cousin of Wallace, to assume charge of the new laboratory and undertake the work it was intended to accomplish.

In 1900 Sabine married Miss Jane Downs Kelly, originally of New Bedford, Mass. She was a physician of established reputation in Boston before her marriage, and she continued to practice after marriage, especially in connection with the Children's Hospital. The fact that she found time and energy for this professional occupation, while performing with rare competence the duties of a housekeeper and mother of a family, is sufficient evidence of her unusual combination of qualities.

Two daughters,[13] children such as one might hope to see from such parents, were the issue of this marriage—Janet, born in 1903, and Ruth in 1905. In them Sabine's fervently affectionate nature and his fatherly pride rejoiced.

For many years after he became a member of the Harvard Faculty of Arts and Sciences Sabine took no prominent part in its meetings. His colleagues were therefore somewhat surprised when, late in 1905 or early in 1906, he proposed a radical change in the organization of the Lawrence Scientific School, which was under the control of this faculty. Discussion of the affairs of this school was active at this time, because proceeds from the great McKay bequest, intended for the promotion of science teaching at Harvard, were about to come in, and one of the periodical agitations regarding relations between the Lawrence Scientific School and the Massachusetts Institute of Technology had recently occurred.

Sabine's proposition, or the plan growing out of his proposition, was to establish a graduate school of applied science, Harvard College taking over from the scientific school the instruction leading to the degree of bachelor of science. This proposal was approved by the Faculty of

[13] The younger, not thoroughly strong and sorely afflicted by the loss of her father, died suddenly in 1922.

Arts and Sciences, and it was adopted by the governing boards of the university in the spring of 1906. Professor Shaler, who had been dean of the Lawrence Scientific School for many years, died in April of that year, and Professor Sabine was made dean of the new graduate school of applied science.

Sabine took this office at the urgent request of President Eliot, but reluctantly and doubtless with misgivings, being a teacher and student by nature, not an executive, not a manager of men. In one respect he was, perhaps, little fitted for administrative duties. His nature was intense and reserved. Regarding men, and often regarding measures, he had convictions rather than opinions. Dispassionate argument was difficult for him, though he lacked the instinct and temper of the dictator. So the duties of a dean, the real executive head in this case, of an institution in a period of reconstruction, must have been hard for him at times, harder than they would have been for a man of different temperament.

The decision once made, he threw himself into the duties of his new position with characteristic energy, devotion, and elevation of ideals. Anyone reading his brief annual reports for the seven years of his service as dean must be impressed by the vigor of his administration and by his constant endeavors to improve the effectiveness of the departments in his charge, both by changes of plan and by a careful selection of the personnel. The school of forestry was soon put on a satisfactory basis. The Bussey Institution was transformed from a very thinly attended undergraduate school of agriculture to a place for "advanced instruction and research in the scientific problems that relate and contribute to practical agriculture and horticulture"; William Morton Wheeler was called from the Natural History Museum in New York to occupy the newly created chair of economic entomology, and other eminent specialists were enlisted in the warfare against the insect pests with which the trees of Massachusetts had been signally afflicted. Prof. George F. Swain and Prof. Harry E. Clifford were called from the Massachusetts Institute of Technology to chairs of civil engineering and electrical engineering, respectively, in 1909; but at that time "it was impossible to find a suitable appointee for the position in mechanical engineering." In 1911 George C. Whipple came from the practice of his profession in New York City to the professorship of sanitary engineering, and Prof. Eugène Duquesne, from the École des Beaux Arts in Paris, to the chair of architectural design, both of the positions thus filled being new ones at Harvard.

It seems probable that Sabine's methods would in time have built up a strong school of applied science at Harvard, in spite of the great prestige of the Massachusetts Institute of Technology, which had now come under the very able management of President Maclaurin. But President Lowell, of Harvard, who by family tradition was a trustee of the institute and whose main ambition for Harvard did not lie in the direction of applied science, either proposed or assented to the terms of the famous merger which undertook to combine the financial resources, the aims, and the teaching staffs of the two scientific schools. His influence, together with the evident economic advantages of maintaining one school instead of two in one community, prevailed, and so the fusion was, for a time, effected.

Of course this combination involved the extinction of Sabine's office as dean, and he might well have felt that it imperiled some of the aims he had been striving for. Any man, however slight may be his natural ambition for executive power, is likely to become somewhat enamored of it after some years of possession. Moreover, Sabine was strong in the respect and confidence of influential men. I have heard, though I do not profess to speak with authority on this point, that the Harvard Corporation would have rejected the proposed merger if he had opposed it. But he did not oppose, he advocated it. Did he do so with full conviction? I do not profess to know; but my conjecture is that, when once the change had been suggested and he saw that it would involve in some degree a sacrifice of himself, he was no longer able to view the matter with a free mind. Where another might have shown resentment and made opposition, he took inevitably the path of self-effacement.

Sabine was in Germany with his family in the summer 1914. Going to England when the war began, he was witness to the spirit in which the British nation roused to action and was immensely impressed by it. He met leading men of the Government, and, in recognition of

his eminence as an authority on acoustics, received the remarkable compliment of being put on a committee to study the physical conditions of the House of Commons. [14]

The years 1914–15 and 1915–16 were for him a period of comparative quiet, spent in teaching and acoustic investigations, doubtless with much attention to plans for the acoustic laboratory to which reference has already been made. But for the war, he would probably have had before him a long career of growing usefulness and fame, and would have lived to a vigorous old age according to the habit of his ancestors. But from that fiery furnace into which other men were drawn by millions he could not hold himself back. He would have felt recreant if he had escaped unscathed.

Going to France in 1916, with the intention of giving a course of lectures at the Sorbonne in the fall, he engaged during the summer in relief work that took him to Switzerland. Overtaxed in strength, he was attacked during the fall by a malady that compelled the postponement of his lectures and nearly ended his life at once. When he was able to be moved, he went from Paris back to Switzerland, this time as a patient; but he gained strength, studying French constantly meanwhile, and in the spring of 1917 gave his lectures, on architectural acoustics, at the Sorbonne.

Such notes of these lectures as are available contain little that had not already been printed in English, but a few paragraphs taken from them by Dr. Theodore Lyman for use in the Collected Papers may well be given here in illustration of what Sabine considered to be, in a word, his contribution to acoustics, *the consideration of "boundary conditions":*

> In no other domain have physicists disregarded the conditions introduced by the surrounding materials, but in acoustics these do not seem to have received the least attention. If measurements are made in the open air, over a lawn, as was done by Lord Rayleigh in certain experiments, is due consideration given to the fact that the surface has an absorbing power for sound of from 40 to 60 per cent? Or, if inside a building, as in Wien's similar experiments, is allowance made for the fact that the walls reflect from 93 to 98 per cent of the sound? We need not be surprised if the results of such experiments differ from one another by a factor of more than a hundred.
>
> It would be no more absurd to carry out photometric measurements in a room where the walls, ceiling, and even the floor and tables consisted of highly polished mirrors, than to make measurements on the intensity, or on the quantitative analysis of sound, under the conditions in which such experiments have almost invariably been executed. It is not astonishing that we have been discouraged by the results, and that we may have despaired of seeing acoustics occupy the position to which it rightly belongs among the exact sciences.
>
> The length of the waves of light is so small compared with the dimensions of a photometer that we do not need to concern ourselves with the phenomena of interference while measuring the intensity of light. In the case of sound, however, it must be quite a different matter.
>
> In order to show this in a definite manner, I have measured the intensity of the sound in all parts of a certain laboratory room. For simplicity, a symmetrical room was selected, and the source, giving a very pure tone, was placed in the center. It was found that, near the source, even at the source itself, the intensity was in reality less than at a distance of five feet from the source. And yet the clever experimenter, Wien, and the no less skillful psychologists, Wundt and Münsterberg, have assumed under similar conditions the law of variation of intensity with the inverse square of the distance. It makes one wonder how they were able to draw any conclusions from their measurements.

The following extract from a letter written to me August 15, 1917, gives Sabine's own comments on these lectures and some indication of the work in which he engaged when they were ended:

> The lectures at the Sorbonne then began quietly. There were twenty-five in the audience the first day, and this number rose to fifty—but lecturing at the Sorbonne while the world is being transformed into a totally different institution, and life such as is left of it is sobered for years to come—lecturing at the Sorbonne seems a thing apart. I was also asked to lecture at the École des Beaux Arts, to give a public lecture, and to lecture before the Society of Architects. The latter was so kind as to give me a medal as souvenir of the occasion; they find the time and the heart to do things nicely in France, nicely and kindly even in the midst of death.
>
> The lectures had hardly stopped when I was asked to help in the Information Bureau of the United States Navy here in Paris on the submarine question—a week later by the French Bureau des Inventions on submarine and aeroplane questions; I am also definitely on the staff of the Bureau of Research of the Air Service of the American Expeditionary Force—a long title—and I have just received a request from the British Munitions Inventions Bureau to come over to England for consultation on some problems in acoustics.

[14] He told me gleefully that he thereupon asked to be admitted to the floor of the House during a session, for the purpose of making acoustic observations, and was so admitted, greatly to the damage of that formidable English institution, precedent.

The few remaining weeks will be full ones. In two or three days I shall go to Toulon, the Mediterranean base of the French fleet, for some direct experience of the submarine problem—then to Italy and the Italian front—back to Paris and three days later to England—back to the English front, then to the French front, again back to Paris for a few days to report, and then home. This programme is sufficiently active.

In the air service I can not say that I have become a pilot, but I have become a good passenger, and this, the pilots say, is a very good thing to have along. I can also ride in a Paris taxicab without the slightest anxiety, but the other day I was taken by an American officer in a little Ford machine and my heart was in my mouth all the time.

The fact is that his sincerity, scientific acumen, and energy so won the confidence of those high in authority that he became an unofficial *liaison* agent between the French, the English, and the Italians in matters of great military importance. He told me that he found these allies were not exchanging information concerning airplane practice freely, and I believe he said that he personally brought to pass the first official communication between the English and the Italians on this matter.

September 2, 1917, he wrote to his mother:

This summer has been wonderful. It *is* a pleasure to be wanted by the French, British, and Italian Governments as well as by our own. It is a pleasure to be of service. And along with it have come some wonderfully interesting experiences. I was in the last great Italian offensive on the Isonzo—the Carso—with the shells flying overhead in both directions. In a great bombarding aeroplane I was down over the Adriatic and Trieste and later up over and into the Alps. Tomorrow I go out over the Mediterranean in a dirigible, Tuesday from Genoa in a hydro-aeroplane and Wednesday from Toulon in a submarine. Everything has been opened to me.

I venture the opinion that, when he reached America in the fall of 1917, he knew as much about the varied phases of airplane warfare as any other one man of that time.

Fortunately he did not have to wait long for a hearing in Washington. The authorities there saw the value of the information he had brought home and placed him at once in the innermost circles [15] having to do with airplane production and use. It is a great satisfaction to record that, during all the anxious and impatient months that followed, he spoke with enthusiasm of the devotion and ability of those with whom he was thus brought into the closest relations; and, when some of them were afterward virtually placed on trial for misconduct, he stoutly affirmed their merits, anticipating the verdict which the public has now reached regarding them.

Trying to carry at once his work of teaching in Cambridge and his duties as general adviser, information expert, and adjuster of personal relations in Washington, he was constantly taxed beyond the safety limit of his strength. Weekly, while the college year lasted, he would come to Harvard for two or three days, and weekly he would be summoned back to Washington by telegram. During the whole summer of 1918 he made only one or two visits, and these very brief, to his family.

When the college year 1918–19 opened, he saw in the existence of the Students' Army Training Corps at Harvard conditions which appealed to his imagination with such force that he broke away from his regular Washington engagements. Into the work of teaching, teaching for war, he plunged with a crusading [16] enthusiasm, quite prepared to give his life, if need be, in the effort. I tried, as his wife tried, to make him see that failure to meet a class or to take a train for Washington, when he had, let us say, a temperature of 102°, was not quite so base conduct as deserting the front in the crisis of a battle, but such remonstrances made little im-

---

[15] The following is an extract from a letter written February 8, 1919, to Professor Sabine's mother by Col. Edward A. Deeds, from the office of the Director of Military Aeronautics in Washington: "During my first conference with him I was so impressed that I immediately made him one of my staff, giving him a desk in an adjoining office. All cablegrams regarding apparatus passed over his desk. He kept our Allies informed of our progress and in turn interpreted their development to us. His judgment was considered so good that within a few months he was made the final authority to select from the samples sent from overseas, of instruments to be put in production."

[16] In addition to his obvious engagements, he had in mind, as I know from a hint he let fall, some desperate enterprise which I took to be an airplane expedition on a great scale, for dropping immense quantities of high explosives in a place where they would be most likely to end the war. In 1922 I told his mother of this surmise of mine, and she appeared almost shocked, saying gently, "I should think he would have wanted to prevent that." A few days later she said to me in the same gentle way, "I would have opened the earth and let the Germans down." Verily, the militant Quaker is a formidable figure.

pression on him; so long as he could stand, he would do his work. It is a pleasure to reflect that his students, if they did not prove themselves altogether worthy of his efforts in their behalf, did at least give him their admiring attention and their love.

Near the beginning of the year 1919 he underwent a surgical operation, too long delayed. About 10 days before his death, which came on January 10, 1919, I heard of his serious illness and called his house by telephone for further particulars. Sabine himself at once answered me in a voice so cheerful and strong that my fears might have been dissipated if I had not remembered that on a previous occasion, after going home from the laboratory unmistakably ill, he had answered my inquiries in the same way. This was, in fact, his method of censoring health bulletins relating to himself.

A day or two later, not venturing to use the telephone again, I called at his door and asked for Mrs. Sabine. Being told that I could not see her, I left my name and was turning away, when I was called from within the house, and Sabine, who had heard me at the door, came halfway down the stairway from the second story to meet me. Finding that he really wished to have a talk with me, I went to his room. He must have known his condition to be one of great danger, but evidently he was not submissive to the ordinary rules of the sick chamber. Most of the time while I was with him he reclined, half sitting, on a couch, apparently taking whatever position was most tolerable, and I knew it would be useless to offer advice. In his care, or lack of care, for his own health he was now, as he had been during all the 30 years of my acquaintance with him, a defier of precept, a law unto himself.

Any time for the past year or two, looking upon his spiritual, still youthful, face, and noting the smiling obstinacy with which he followed a course of toil that must end his life too soon, one might be tempted to think of him as some elfin being that had taken human form in benevolent caprice, but was now planning departure and adventures new. Not that he ever, save in the very ecstasy of pain and weakness, showed any symptom of world weariness. He was full of affection, full of the zest of life, full of plans for future years. He told me that he never enjoyed his work of teaching more than during the fall just past, so trying to most of those who remained in academic life, and he had been looking forward joyfully to the prospect of resuming his work of research, especially that part of it which was to be carried on in the special laboratory built for him by his friend Colonel Fabyan, at Geneva, Ill.

From his ancestry he should have had long life, and he probably counted too much on this inheritance. He had lived through more than one tremendous crisis of illness, and he seemed to feel that he could brave off any attack of disease. But even if he had seen death unmistakable in his path, he would not have halted or turned aside so long as the war lasted. In fact, he had been repeatedly warned that a surgical operation was needed to save his life, and had replied that he could not stop for this while his country was in danger.

With all his high courage and resolution, however, and a clearness of head likely to take him in safety through difficult passes, he was no seeker of danger for its own sake, no sportsman, in the ordinary sense, no player of rough games. Indeed, during his early years at Harvard, the slightness of his figure, the delicacy of his face, the deferential courtesy of his manner, may have raised in the minds of some the question whether he was fitted for the not always easy task of teaching and controlling a large class of possibly boisterous undergraduates. But this solicitude was quite gratuitous. He was the son of a woman who, at 70 years or more, described her own way of crossing the proverbially dangerous streets of Paris thus: "I have no difficulty; I wait till the street is fairly clear and then I walk across, looking neither to the right nor to the left." So Sabine, telling how to deal with an incipient lecture-room disturbance, said: "It is perfectly easy; all you have to do is to survey the audience and look every man in the eye."

I suppose, however, that he was rarely obliged to use even this measure of discipline. For young men were drawn to him; he spoke in a low, though clear, tone, and they kept still in order not to lose his words; they clustered about him after his lectures, partly to hear more and partly, I suspect, for the mere pleasure of being near him. They took his advice about their studies and their life work, and they could not have done better.

We say of such a man, it is a pity he died so young; if he had taken care of himself, had been regular in his meals and in his hours of sleep, he would have had a long as well as a useful life. Yes; but a man must work according to his nature, and Sabine's temperament was not that of the ordinary man, not that of the ordinary scientific investigator. Some of the high things he did could not have been done by a man who must be regular at his meals and regular in his hours of sleep. When we remember how long the plagues he grappled with had baffled the efforts of others, and with what intensity of labor he finally exorcised them, it seems not irreverent or unfitting to recall the words: "This kind goeth not out but by prayer and fasting."

BIBLIOGRAPHY

(This brief list illustrates the remark made on page 2 of the memoir, that Sabine's papers were remarkably few and remarkably significant. All the articles here mentioned, except the first three, are contained in the volume of his Collected Papers, issued in 1922, by the Harvard University Press)

1. Wave lengths of metallic spectra in the ultra-violet. (With John Trowbridge.) Proc. Amer. Acad. of Arts and Sciences, vol. xxiii, 1887–1888, pp. 288–298.
2. Selective absorption of metals for ultra-violet light. (With John Trowbridge.) Proc. Amer. Acad. of Arts and Sciences, vol. xxiii, 1887–1888, pp. 299–300.
3. Electrical oscillations in air. (With John Trowbridge.) Proc. Amer. Acad. of Arts and Sciences, vol. xxv, 1889–1890, pp. 109–123.
4. Reverberation. (The American Architect, 1900.)
5. The accuracy of musical taste in regard to architectural acoustics. The variation in reverberation with variation in pitch. (Proc. Amer. Acad. of Arts and Sciences, vol. xlii, No. 2, June 1906.)
6. Melody and origin of the musical scale. (Vice-presidential address, Section B, Amer. Association for the Advancement of Science, Chicago, 1907.)
7. Effects of air currents and of temperature. (Engineering Record, June, 1910.)
8. Sense of loudness. (Contributions from the Jefferson Physical Laboratory, vol. viii, 1910.)
9. The correction of acoustical difficulties. (The Architectural Quarterly of Harvard University, March, 1912.)
10. Theater acoustics. (The American Architect, vol. civ, p. 257.)
11. Building material and musical pitch. (The Brickbuilder, vol. xxiii, No. 1, January, 1914.)
12. Architectural acoustics. (Journal of the Franklin Institute, January, 1915.)
13. Insulation of sound. (The Brickbuilder, vol. xxiv, No. 2, February, 1915.)
14. Whispering galleries. (Collected Papers, Harvard University Press, 1922.)
15. Note on measurements of the intensity of sound and on the reaction of the room upon the sound. (Appendix to Collected Papers.)

E. E. Barnard.

NATIONAL ACADEMY OF SCIENCES

Volume XXI
FOURTEENTH MEMOIR

BIOGRAPHICAL MEMOIR EDWARD EMERSON BARNARD

1857-1923

BY

EDWIN B. FROST

Presented to the Academy at the Annual Meeting, 1924

EDWARD EMERSON BARNARD

1857–1923

By Edwin B. Frost

By the death on February 6, 1923, of Edward Emerson Barnard, professor of practical astronomy in the University of Chicago, and astronomer at the Yerkes Observatory, the world has lost one of its foremost searchers of the skies, and the National Academy of Sciences one of its most distinguished investigators in the field of physical science.

He was born at Nashville, Tenn., on December 16, 1857, the son of Reuben and Elizabeth Jane (Haywood) Barnard. The death of his father before his birth threw a heavy burden upon his mother, who was obliged to support herself and her two young sons. Those were hard times for poor people in that section of our country, and they were still harder after the Civil War came on, a few years later, bringing tragedy to all on the scene of conflict. The Battle of Nashville made an impression upon the lad that never left him. Later in his youth he survived an attack of cholera, when that plague raged beyond its usual bounds.

His mother's firmness of character was such that she did not lose her taste for culture in the struggle with poverty. The name Emerson, given to the young son in honor of our American philosopher, was an evidence of this. She inspired the lad with a desire to know good literature, although his opportunities for any regular education in school were most meager; in fact, Edward Barnard attended the common school only two months in his life. His mother had a taste for art, also, and partially supported herself by modeling wax flowers. That she had impressed upon her son character and self-reliance was indicated by her statement that the lad, when less than 9 years old, could be depended upon to do a task in which many other boys had failed. A photographer in Nashville had on the roof of his studio a ponderous enlarging camera which had to be kept pointed at the sun. Most of the lads had lacked the patience necessary in the human substitute for a driving clock, and went to sleep at the task. Edward Barnard justified his mother's confidence; he worked in that studio, in various capacities, for 17 years. His duties, doubtless often monotonous, were fitting him to be a pioneer in the photography of the heavens.

As a boy, he had watched with wondering eyes the starry skies above him, as he lay upon an old wagon box in the yard; and he had, indeed, learned to know the stars, but not their names or their constellations. A man employed in the studio, who had mechanical skill, one day picked up in the street the small objective of a broken spyglass, and, making for it a paper tube, constructed a telescope for the young apprentice. With this, Edward Barnard studied the stars further, but still without the means of identifying them, until chance brought into his hands, somewhat later, a volume of the works of Thomas Dick, who enjoyed a considerable reputation as a writer on astronomy, as well as on theology. It seems that Barnard found his first star map here, and was delighted to learn the conventional names of the objects with which he was already so familiar. Later, he put together a better instrument for which he purchased lenses of 2¼ inches aperture.

The young man was now supporting himself and his mother by his daily work in the studio, but a passion for astronomical observation had already developed. Through rigid economy he gratified this passion by the purchase, in 1876, of a 5-inch telescope, equatorially mounted, from John Byrne, of New York, the price of which, $400, represented some two-thirds of a whole year's earnings. This gives sufficient evidence of his determination to acquaint himself with the science of astronomy. In 1877 the American Association for the Advancement of Science held its annual session at Nashville, and the youth of 20, who was becoming locally well known for his zeal as a stargazer, joined the association. His friends persuaded him to bring his telescope and meet the president, Simon Newcomb. This distinguished astronomer,

20154°—26——35

already eminent at the age of 42, whose researches were in the domain of theory rather than of observation, advised young Barnard that to accomplish anything important in astronomical research he must first be well grounded in mathematics. This advice might have discouraged a less ardent seeker for knowledge of the stars, but it inspired Barnard to do just what was advised. He applied himself diligently to elementary mathematics and other common items of education which he had been obliged to neglect in his youth, and from his own slender earnings hired a tutor for some branches of his study.

In January, 1881, while still an employee at the studio, he married Miss Rhoda Calvert. She had come from Yorkshire, England, to Tennessee a few years earlier with her brothers, who were artists and who also had work in connection with the studio. His marriage greatly influenced his subsequent career, as his wife most unselfishly encouraged and helped him in his efforts to obtain a better education and to overcome some cultural deficiencies of which he was conscious. She proved a true helpmeet in every way, caring for the household in a most prudent manner and taking over all responsibility for his now invalid mother. This made it possible for him to improve every opportunity that presented itself for his advancement.

On May 12, 1881, Barnard discovered his first comet in the morning sky near Alpha Pegasi. He found it again on the next night, but could not afterwards locate it, and inasmuch as he did not send out any announcement to the astronomical world, and it was not seen by any other astronomer, this comet was never assigned a place in the formal records of astronomy—no number was given it, and it was not counted by Mr. Barnard himself. Of course, from what was later known of his reliability and skill as an observer, there could be no question as to the certainty of his observation, but at that time he was unknown among astronomers.

This accidental discovery, however, developed his interest in the search for comets, and he began systematically to sweep the sky for them. His diligence was rewarded on the night of September 17 of the same year, when he found a comet in the constellation Virgo. He announced the discovery to Dr. Lewis Swift, so that it was observed by other astronomers and received the name "Comet 1881 VI." Mr. H. H. Warner, who had established at Rochester, N. Y., the private astronomical observatory of which Doctor Swift was the director, had taken much interest in astronomy and offered a prize of $200 for each unexpected comet discovered by an American observer. The award was made to Mr. Barnard for the discovery of this comet, and it happened opportunely, for at that time the young man was building a little house for his bride and his mother, and the burden of paying notes on borrowed money as they came due was a serious one. Five times in all Barnard received this award for the discovery of a comet, and it meant to his family the possibility of owning their modest home. That dwelling is still known in Nashville as the "Comet House." Few, indeed, are the astronomers whose keen eyesight and extraordinary diligence in the quest for celestial discovery have literally provided them with a roof to sleep under. It was very little, however, that he slept under that roof when the sky was clear.

I am describing these circumstances in this detail because they may seem almost incredible to some of our contemporary European astronomers who have reached positions in our science in university or governmental observatories after passing through a very definite and uniform course of study and training. That inborn genius can find a way of achieving its ideal in America, we may call to witness our lately departed friends, Burnham and Brashear, as well as Barnard.

A year later, on September 13, 1882, the second recorded comet (1882 III) was discovered with the 5-inch telescope. A rather dramatic incident occurred a month later, October 14, of which we have a printed record in Barnard's own words. He had been searching for comets through the early part of the night and had set his alarm clock for a later hour in order to get some much-needed rest. He says that when the alarm clock sounded he had been dreaming of discovering a wonderful field of comets, big and little, with long and short tails, in his field of view. After he awoke he began sweeping in the neighborhood of the great comet of 1882, and to his astonishment saw 12 or 15 small comets of varied appearance in the vicinity. He had obtained positions for 7 or 8 of these when the dawn came. He announced his dis-

coveries to Doctor Swift, but this astronomer did not distribute the information. Speaking of this omission Mr. Barnard has said: "Whether he thought that I was trying to form a comet trust or had suddenly gone demented has never been clear to me." These comets were undoubtedly real, and fragments of the great comet. Schmidt, of Athens, reported one such object which he observed on October 8, another was observed by E. Hartwig on October 9, and still another by Brooks, of Phelps, N. Y., on October 21. They were never separately announced in the list of comets.

During this time young Barnard was working in the studio by day for a livelihood and studying, by himself or with a tutor, on cloudy and moonlit nights. In 1880 he planned to write a booklet on Mars, and solicited subscriptions from friends in Nashville to cover the cost of printing. We have been unable to find a copy of this, which was planned to be a duodecimo; and it is doubtful whether it was ever actually printed. Three years later we find the young amateur conducting an astronomical column in a journal known as the Artisan, which was published twice every month at Nashville. In the same year friends who had perceived the genius of the young man offered him a fellowship in Vanderbilt University at Nashville, giving him an opportunity to devote his time exclusively to his studies and to make use of the 6-inch equatorial of the small observatory of the university. The stipend was only about $300, together with a house on the university campus near the observatory, and it was a venturesome step for a young man with a wife and a mother dependent upon him to give up his work at the studio and endeavor to live on the sum provided. But his wife bravely counseled acceptance of the offer, and said, "We will get along somehow"; and they did. Barnard became enrolled as a special student in the school of mathematics, at the same time having the care of the observatory. Later he received an appointment as instructor in practical astronomy, continuing his studies in mathematics, physics, and chemistry and some of the modern languages.

The young astronomer's first trip into the outer world was made in 1884, when he attended the meeting of the American Association for the Advancement of Science at Philadelphia, visiting en route the observatories at Cincinnati, Allegheny, Washington, Cambridge (Harvard), Albany, and Princeton, and seeing for the first time those cities, together with New York and Boston. Every economy had to be practiced, and he was accustomed to avoid unnecessary hotel bills by traveling in day coaches at night. His record of this trip, in daily postal cards and letters to his wife and his mother, is very humorous and interesting. He had formed in advance, mental pictures of the prominent contemporary astronomers, and many of them turned out to be quite different from his anticipations. Thus, he had expected that Prof. E. C. Pickering would be a formal and distant dignitary, as might befit a native of Boston and the director of the Harvard College Observatory. To his surprise, he found that "Professor Pickering is comparatively a young man, and strongly resembles a simple countryman. Had anyone shown him to me on the street and told me that was the famous director of the Harvard College observatory, I should not have taken his word on oath. I should have been positive there was a mistake. However, he is the most unassuming man that you can imagine, and I admired him very much, indeed."

At this meeting in Philadelphia, he received a most friendly welcome and recognition from the astronomers whom he had at last met in person, and could henceforth feel that he was one of the fraternity.

In 1887, at the age of 30, he graduated from the school of mathematics at Vanderbilt. Meanwhile, he had discovered seven more comets—the last on May 12, 1887—namely, 1884 II, 1885 II, 1886 II, 1886 IX, 1886 VIII, 1887 III, 1887 IV. Of these comets, 1884 II was periodic, with a return expected every 5.4 years, but it escaped detection in subsequent years. In 1887 he published in the Astronomische Nachrichten a parabolic orbit for Comet 1887 III.

In 1883 he had independently discovered the Gegenschein, while sweeping the skies, and had become extraordinarily familiar with the objects in the sky which could be seen with a small telescope. He had given special attention to the planet Jupiter and was an independent discoverer of the great red spot, as he was unaware that its presence had been announced some months previously, in July, 1878, by C. W. Pritchett, of Glasgow, Mo. Barnard was a

careful delineator of what he saw, and many of his early sketches of Jupiter were published in the first volumes issued by the Astronomical Society of the Pacific.

In 1887 the Lick Observatory was nearing completion, and Prof. E. S. Holden had been chosen director, functioning ad interim as the president of the University of California. He corresponded with Barnard, and in that summer offered him a position on the staff then in process of formation. Barnard accepted the opportunity to join this finely equipped institution, and in September he and his wife started on their interesting journey to California. They found temporary lodgings in San Francisco, until they should be able to make their home on Mount Hamilton. There were delays in the completion of the observatory, and the regents of the university were unable to make provision for the astronomers until the trustees of the Lick estate had formally turned over to them the completed observatory. Mr. Barnard, again finding himself in need of the salary which was not forthcoming, obtained work in copying legal papers in the office of a lawyer. Early in the next spring the Lick trustees invited him to come to the mountain and make an inventory of the observatory's equipment. Finally, on June 1, 1888, Director Holden was able to write, with evident relief after the trying delay: "The observatory begins its active existence to-night." S. W. Burnham and J. M. Schaeberle were senior astronomers, J. E. Keeler and E. E. Barnard, juniors; and work began enthusiastically. Mr. Barnard became very warmly attached to these members of the staff. This was his first opportunity to be regularly associated with astronomers of considerable experience, and it was of great importance to him. In turn, his associates highly appreciated Barnard's ability as an observer and his tremendous capacity for work.

The 12-inch telescope was assigned to Barnard, together with the comet-seeker, and his technical knowledge of photography was very soon utilized by Director Holden. On September 2, Barnard discovered Comet 1889 I, and in October, Comet 1888 V. He also observed their positions assiduously with the excellent equatorial and filar micrometer. He further observed nebulae and planets, and in 1889 made a notable observation of the eclipse of Japetus by the ring system of Saturn. He could see that the sunlight illuminated the satellite through the crape ring, thus indicating that the ring was quite transparent, and supporting the view that it was made up of small particles. During that year he discovered Comet 1889 II, as well as 1889 III, which has a computed period of 128 years. His most important work, however, was the beginning he made during that summer in photographing the Milky Way with the Willard lens, which became a famous instrument in his hands. This was a portrait lens of 31 inches focal length, which had been used by some photographer and had received its name from the dealer in such lenses in New York. This camera was strapped to the 6½-inch equatorial, which served as a guiding telescope. Barnard's long exposures with this instrument brought out the wonderful richness of the star clouds and other features of the Milky Way as they had never before been revealed. They thrilled him and his associates with their significance and beauty, and later the entire scientific world shared in this appreciation of them.

Barnard was the first to observe the return of d'Arrest's comet in 1890, and, in the following year, of comets Encke and Tempel-Swift, and he discovered Comets 1891 I and 1891 IV. In 1892 he made the first discovery of a comet by photography, finding on his plate taken on October 12, Comet 1892 V, for which a period of 6.5 years was computed, but which has never been observed at a subsequent return.

As a junior member of the staff of the Lick Observatory, Mr. Barnard did not receive a regular assignment at the 36-inch telescope, but his friend Burnham was always glad to check any important observation for him or give him opportunity of examining the object with the great refractor. Mr. Burnham resigned his position in June, 1892, and resumed work as clerk of the Federal Court in Chicago. Mr. Barnard had naturally been eager for an opportunity to make regular use of the great refractor, but he was unable to secure this privilege until the first of July, 1892, when he received the coveted assignment for one night each week. On the eleventh night of his use of it (September 9, 1892) he made his brilliant discovery of the fifth satellite of Jupiter. We quote from his own account of his observations in Astronomy and Astrophysics, 11: 749, 1892:

Friday being my night with the 36-inch telescope, after observing Mars and measuring the positions of his satellites, I began an examination of the region immediately about the planet Jupiter. At 12 o'clock as near as may be, to within a few minutes, I detected a tiny point of light closely following the planet near the 3rd satellite which was approaching transit. I immediately suspected it was an unknown satellite and at once began measuring its position angle and distance from the 3rd satellite. On the spur of the moment, this seemed to be the only method of securing a position of the new object, for upon bringing the slightest trace of the planet in the field the little point of light was instantly lost.

I got two sets of distances and one set of position angles, and then attempted to refer it to Jupiter, but found that one of the wires of the micrometer was broken out and the other loose. Before anything could be done the object rapidly disappeared in the glare of Jupiter. From the fact that it was not left behind by the planet in its motion, I was convinced that the object was a satellite. A careful watch was kept at the preceding limb of the planet for the reappearance of the satellite, but up to daylight it could not be seen.

Though positive that a new satellite had been found, extreme caution suggested that it would be better to wait for a careful verification before making any announcement.

The following night with the 36-inch belonging to Professor Schaeberle, he kindly gave it up to me, and shortly before midnight the satellite was again detected rapidly leaving the planet on the following side. That morning I had put new wires in the micrometer, and now began a series of careful measures for position. As I have said, the satellite was so small that no trace of Jupiter could be admitted into the field for reference in the measures. It was necessary, therefore, to bisect the satellite, with the planet out of the field, and then by sliding the eyepiece bring the limb of Jupiter into view and bisect it. This method did not permit any measures from the polar limbs of Jupiter. Following the satellite thus, it was seen to recede from the planet to a distance of some 36″ from the limb, when it gradually became stationary. Remaining so for a while, it began once more to approach the planet and rapidly disappeared in the glow near the limb. The measures, repeated as rapidly as possible, thoroughly covered the elongation and gave the means of approximating to its period.

The following morning a telegram was sent out announcing the discovery. Subsequent observations have thoroughly confirmed the discovery.

On account of its extreme closeness to the planet it is difficult to say just what its magnitude is. Taking everything into account, I have provisionally assigned it as thirteenth magnitude. I hope to be able to settle definitely this question by observing some little star near Jupiter, and then afterwards determining its magnitude when the planet has left it. Until this is settled, any estimate of the actual size of the satellite must be the merest guess, but it will probably be found to not exceed 100 miles in diameter, and perhaps less than that.

After the first few observations I inserted a piece of smoked mica in the eyepiece, and using this as an occulting bar, the measures were made with ease and accuracy. Careful measures thus made from the polar limbs for the Jovicentric latitude of the satellite show that its orbit lies sensibly in the plane of Jupiter's equator and that consequently the satellite is not a new addition to the Jovian family, since it would doubtless require ages for the orbit to be so adjusted if the object were a capture.

The reader will note from this extract the element of independence which was a characteristic of Barnard's discoveries. He perceived with a sort of intuition that this was probably a new satellite; in fact, he was convinced of it by his brief observations on the first night. His exercise of great care in making no premature announcement was also characteristic. He, furthermore, was quick to realize that it would be a matter of general interest whether the satellite had been newly acquired by Jupiter, and his measurements to decide this point were made a few evenings later. We also see an illustration of his readiness to adapt his observing methods to difficult circumstances in providing a piece of smoked mica for occulting the brilliant planet. His scientific caution and ingenuity are illustrated by his proposal that the magnitude of the object should be determined by comparison with some star of about the same magnitude which some night would lie near the planet's position and thus afford a reliable basis for an estimate of brightness.

This first addition to the family of Jupiter, which had received careful telescopic observation for nearly three centuries, brought to Mr. Barnard instant recognition as an observer of the first class. The Lalande gold medal of the French Academy of Sciences was awarded to him a few months later for this notable astronomical feat. Professor Barnard followed this satellite with very careful micrometric measurements for many years after its discovery, seeking to improve our knowledge of its orbit, and he published 14 papers covering his observations of elongations, nearly all of them in the Astronomical Journal. The difficulty of observing the object was not because it was so faint, but because of the brightness of the planet. Quite good conditions of seeing were always necessary for observing it, even with the large telescope. So far as is known to the writer, the fifth satellite has never been photographed, and the smallest aperture with

which it has been observed is that of the 18½-inch equatorial of the Dearborn Observatory at Evanston, with which Prof. G. W. Hough observed it on October 15 and November 11, 1892. The discovery of this satellite doubtless renewed interest in the search for others by the use of photography, resulting in the discovery of three further, remote satellites at the Lick Observatory, VI and VII by C. D. Perrine and IX by S. B. Nicholson, while VIII was discovered by P. J. Melotte, at Greenwich.

During his later years at the Lick Observatory Mr. Barnard gave much attention to careful measurements of the diameters of the planets, including the four largest asteroids. He made a comprehensive study of the dimensions in the Saturnian system and measured the ellipticity of Uranus. He gave particular attention to the diameters and the appearance of the brighter satellites of Jupiter. These extensive researches were published in a series of papers in various astronomical journals, several of them appearing after he had left the Lick Observatory.

It was not only to the study of the Milky Way that Barnard was applying photography with distinguished success. He studied the comets, and took a great interest in the remarkable behavior of their tails as revealed on his negatives. Swift's comet (1892 I) was the first to show on Barnard's photographs the extraordinary changes which the tails of comets may undergo. His subsequent photographs of many comets show that these mutations are characteristic of the tails of some comets, but not of others. Cloudy weather had interfered with observations of this comet during March, but the photographs taken on April 4 and 5 displayed extraordinary transformations in the short interval between them. The significance and value of the photographic records of these capricious changes were instantly appreciated by Barnard, as will be seen from the following quotation from his article written some years later and appearing in the Monthly Notices of the Royal Astronomical Society, 59: 355, 1899:

> This [Swift's comet of 1892] was the first comet to show to the photographic plate the extraordinary changes to which these bodies are subject. Indeed, if it had not been for the photographic plate we should have known nothing of the extraordinary changes that occurred in this comet and several that have since appeared. . . .
>
> For the study of the phenomena of the tails of comets, the portrait lens has shown itself most admirably suited. It has added an interest to the physical study of these bodies that did not exist previously; for the most interesting of the phenomena shown by comets must always escape the visual observer and pass unknown, without the aid of the portrait lens and the photographic plate. Unlike the planets, the comets often traverse the entire solar system. They are, therefore, our only means of exploring the regions between the planetary orbits. Instead of ponderous bodies like the planets, they are but flimsy creations of enormous dimensions. They are thus likely to be easily subject to disturbances in their forms that would produce no perceptible effect on their motions. What these influences may be we do not know; probably swarms or streams of meteors, which we know do exist in space, or possibly some other cosmical matter yet unknown. Such objects might be (and possibly have been) revealed to us by their effect upon the form of the comet's tail as it sweeps through space.

The comet discovered by Holmes in the autumn of the same year (1892 III) was also photographed by Barnard when this round, tailless object, whose motion was almost entirely in the line of sight, was situated very near the great nebula in Andromeda. The motion of the comet among the stars was, in fact, so slight that Barnard, with an exposure of 75 minutes, obtained, on the night of November 21, 1892, a sharp picture of the Andromeda nebula, together with the comet! a circumstance which is not likely to be duplicated. Brooks's comet of the next year (1893 IV) excited Barnard's interest in a high degree by its behavior, which was quite exceptional in those early days of cometary photography. He speaks of his plates of October 21 and 22 as follows (ibid., p. 358):

> There is an utter transformation of the comet in this picture. The tail is larger and brighter and very much distorted, as if it had encountered some resistance in its sweep through space. This disturbance seems to have disrupted the northeast edge of the tail. The small side tail has apparently been swept away, while the more distant portion of the main tail is streaming in a very irregular manner. The entire picture is highly suggestive of an encounter with some sort of resistance. Is it possible the tail passed through a stream of meteors such as we know exist in space? Whatever the cause may have been, the appearance of the tail utterly excludes the idea of the phenomenon being due to irregular emission of the matter from the nucleus—an explanation quite satisfactory in the case of Swift's comet.
>
> In passing, this particular photograph seems to explain at least one of the ancient descriptions of a comet, viz., "a torch appeared in the heavens." The comet as shown in the photograph, is sufficiently suggestive of a torch streaming irregularly in the wind.

* * * * * * * * *

[On the next day the tail of the comet] appears a total wreck in this photograph, and is still more suggestive of a disaster. It is very badly broken, and on the southwest side hangs in irregular cloudlike masses. Near the extremity a large gap exists in the tail, as if something had gone through it from the northeast, and a large mass is torn off beyond this break and seems to be drifting independent of the comet.

For nearly 30 years these unexplained caprices of the tails of comets fascinated Professor Barnard, and whenever a new comet appeared in the sky he was filled with suppressed excitement as to its behavior on the photographic plate. Comet Morehouse (1908 IV) was thoroughly satisfying in this respect, and he obtained no less than 350 photographs of it. He would sometimes take successive photographs of it as long as it could be followed above the horizon, before the interference of the moon or dawn.

The results of Barnard's assiduous campaign at the Lick Observatory, from 1892 to 1895, in the study of the Milky Way and comets by photography, are preserved in Volume XI of the Publications of the Lick Observatory. This volume did not appear until 1913—nearly 20 years after the photographs were taken—because of the difficulties which Professor Barnard found in securing satisfactory reproductions of his pictures. His studies of these photographs had been so minute that he recognized details which would have escaped anyone else, so that his standard of excellence of their reproduction became very exacting, indeed, beyond the possibility of the processes of photogravure and heliogravure. The publication of the volume had been made possible by subscriptions for the purpose which Mr. Barnard had secured from California friends of the Lick Observatory and of himself. The collotype process was employed, and the reproductions are as satisfactory as could be expected by any such process. But for a number of years the responsibility of issuing this volume was a heavy one for Mr. Barnard. He became discouraged with what he regarded as the impossibility of securing adequate reproductions, and the work lapsed. He even attempted to return to the Lick Observatory, for distribution among the subscribing friends, the money already expended. He was, however, persuaded to resume his efforts, and, fortunately, was able again to secure the services of the expert in collotype who had begun the work. The volume contains 129 plates, from 92 photographs of the Milky Way and 42 of comets; and it will stand as a monument to the great skill and the untiring zeal of the pioneer in his beginning in this important field of investigation.

A leave of absence was granted Mr. Barnard in the summer of 1893 to make his first trip to Europe. Mrs. Barnard accompanied him, and thus had an opportunity to visit her old home and her relatives in England. The very cordial welcome given to Barnard by his English colleagues also made this a most pleasant visit. He then went over to the Continent and made the personal acquaintance of some astronomers in France and Germany.

Mr. Barnard's residence of nearly eight years in California was full of romantic interest for him. The conditions for his work were very fine, and a clear sky was assured in advance during many months of the year. His residence on the mountain was novel to one who had always lived in a city, and the views of mountain and canyon made a strong appeal to the artistic element in his nature. The life was isolated in winter, but this was broken by visits on Saturday evenings of the winter tourists in California, and acquaintances were established—many of them lasting—with interesting people from different parts of the world. The association with his fellow observers and their families in the little colony was congenial, and particularly close was his friendship with Professor Burnham, who, like himself, was an ardent and expert user of the camera. This phase of their life on the mountain was well brought out in Mr. Barnard's biographical sketch of Mr. Burnham, published in Popular Astronomy, 29: 309, 1921. There was an element of the wild in the howl of the coyotes in the canyons and in the occasional deer seen around the mountain. In the gray dusk, one morning, as Mr. Barnard was nearing the door of his cottage, he saw before him the great form of a panther, or mountain lion, standing a few yards away. Each was returning from his night's work, and each silently respected the rights of the other. After a moment, the panther quietly walked on over the mountain.

The free and hearty cordiality of the Californians, and their appreciation and respect for the men of science on Mount Hamilton, was keenly felt; and Mr. Barnard occasionally

participated in the meetings and activities of the Camera Club and of the Bohemian Club of San Francisco, as well as with the colleagues at Berkeley. Conditions of life on the mountain were comparatively simple and at the start Mr. Barnard's salary was small; but these circumstances were much improved toward the end of his stay, and his opportunities for the use of the great telescope were increased so that he often worked with it on two and sometimes even three nights a week. However, circumstances into which we need not enter finally led him to desire a change, and in 1895 he accepted an invitation to become a member of the staff of the Yerkes Observatory, then in process of construction as a department of the new University of Chicago. His official title was to be professor of practical astronomy and astronomer at the Yerkes Observatory, but no duties of giving instruction were involved for him, beyond an occasional popular lecture in the summer courses at the university. His official connection with the university began on October 1, 1895. As had happened at the Lick, there were unexpected delays in the completion of the Yerkes Observatory, so that for the greater part of a year Professor Barnard lived in Chicago near the Kenwood Observatory, the equipment of which had been presented to the university by Prof. George E. Hale and his father, William E. Hale. This period constituted something of a gap in Mr. Barnard's observational activity, but the time was usefully employed in preparing for publication some of the results of his observations at Mount Hamilton, including his attempts at securing reproductions of the photographs of the Milky Way and comets made there.

In the summer of 1896, Professor and Mrs. Barnard occupied a cottage on the shore of Lake Geneva, and began the construction of the house which was to be their home for the next quarter of a century, on land which they had purchased adjacent to the grounds of the observatory. In February, 1897, Mr. Barnard went to England to receive the gold medal of the Royal Astronomical Society, but, owing to delay of the steamer by bad weather, he unfortunately did not arrive until the day after the annual meeting of the society. A special meeting was held on March 2, at which Professor Barnard exhibited and explained some of his most notable photographs, taken at the Lick Observatory, and a dinner was given like the one prepared on the evening which he missed. As Mr. Barnard was very keen to begin work with the 40-inch, which was then expected to be ready in the spring, as well as to complete the equipment of his new home, in which he took a great interest, he sailed for home after a stay in England of less than three weeks, and was back at Williams Bay by the middle of March.

A few weeks later the 40-inch objective was brought from Cambridge by Alvan G. Clark, and was adjusted by him in its cell on the great instrument. It was first used on the night of May 21, and the tests of its performance were highly satisfactory. There were occasional opportunities during the next week, when the sky was clear, for further tests, and on the night of May 28 Professor Barnard had a narrow escape. He was observing during the latter part of that night, until daylight, and left the dome at dawn. Just before 7 o'clock, as the result of faulty connection of the supporting cables, the moving floor fell, involving its almost complete destruction, but fortunately without injuring the telescope itself. Had this happened a few hours earlier, the observer could hardly have escaped a serious injury or death.

This delayed the formal opening of the observatory until October 21, 1897, and Mr. Barnard had to exercise his patience in waiting for further use of the great refractor. As soon as it was ready for regular work, Professor Barnard again plunged into observing, having the great telescope at his disposal regularly for two and often for three or four nights each week. He was, of course, interested in making some tests of the quality of the 40-inch as compared with the 36-inch telescope which he had previously used. He, therefore, observed some of the difficult double stars, such as Schaeberle's companion to Procyon, and Kappa Pegasi, and secured some elongations of the fifth satellite of Jupiter. He studied some of the variable stars in Messier 5 which had recently been discovered by Prof. S. I. Bailey, finding a couple of additional variables in that cluster. He also measured in the daytime the diameters of Venus and Mercury, sometimes under especially fine conditions of seeing.

He began at this time a micrometric triangulation of some of the globular star clusters, measuring in this first year the positions of 95 stars in Messier 5 and a smaller number in Messier

13, comparing the positions of the latter with measures obtained by Scheiner at Potsdam on photographs taken in 1891. The views of astronomers as to the size of the clusters were quite different then from those entertained at present, and Mr. Barnard had hoped that precise measures with the large telescope would reveal internal motions within a few years. He extended his triangulations to many other globular clusters until he had finally included in his program 20 clusters. In spite of his ardor and his experience as a photographer, Mr. Barnard still found it difficult to recognize the superior advantages of the measurement of star clusters on photographs with the use of rectangular coordinates. He put his trust in the filar micrometer more than in the measuring machine, particularly because he could recognize certain of the cluster stars as triple, which were confused in a single image on the photograph then at hand, taken with an instrument of one-fifth the focal length of the great refractor. A very few years later, when remarkably fine photographs had been secured by G. W. Ritchey with the 40-inch telescope, through a yellow filter and with the double-slide plate holder, Mr. Barnard measured some photographs of the clusters, and in subsequent years took some equally good ones himself. The excellent accordance between his measures on the negatives and those he made visually with the micrometer was to him an evidence that astrometric investigations could be satisfactorily made by photography; to his colleagues this accordance was a demonstration of Barnard's extraordinary skill at the telescope with the micrometer. Similarly, when his visual determination of the parallax of Krueger 60 was very closely confirmed by Schlesinger's measures on plates taken with the 40-inch telescope, it convinced Barnard that good parallaxes could be obtained by the new photographic method; while for us it was again a demonstration of Barnard's great skill as an observer.

It was probably no small disappointment to Mr. Barnard that his measures in the clusters, continued for nearly 25 years, yielded so little in the way of proper motions—in fact, it could hardly be asserted that a single one of the cluster stars showed an appreciable motion with respect to its fellows. From what we know now, it would have been better to omit much of this labor by visual methods, and to trust to photographic records made from time to time for the establishment of the motions which certainly must exist, but which will evidently require a long lapse of time for accurate determination. Progress is being made in the considerable task of evaluating these micrometric measures which were expressed in position angles and distances, and referred to selected stars in the clusters. In Messier 5, 239 stars were included; in Messier 13, 247; and in several other clusters the numbers run over 100. The measures certainly represent accurately the positions of the selected stars during the score of years that they were under Barnard's observation. Plans were begun some 15 years ago for the publication of these measures; but they were delayed in the natural hope that with a longer time some evidences of motion would be established. Considerable attention was given during this work to following changes in brightness of some of the variables, and a few new variables were discovered by Professor Barnard in the clusters. He observed, in particular, Bailey's No. 33 in Messier 5, and determined its period with great precision, contributing half a dozen papers to the discussion of this star alone during the score of years that he observed it. At first he thought that the period was constant, but later the continued observations showed that it first lengthened, then shortened.

Although Mr. Barnard would naturally not be regarded as a regular observer of variable stars, he nevertheless discovered some 10 of these objects, most of them visually, and he followed particular ones for many years; thus, he published three papers on the period and variation of RS Aquarii, which he discovered visually in 1898. He also followed rather closely several especially interesting stars of this sort, discovered by others, which required large optical power when they were near minimum.

The novae were of especial interest to him. He determined their positions micrometrically with great precision with respect to neighboring stars; he estimated carefully their fluctuations in light, and noted the change in focus which resulted from their change in spectrum when the stars were too faint to be observed spectroscopically; he examined them minutely with the great telescope to detect the presence about them of nebulous shells or phenomena of that

nature. He, in fact, discovered visually, in the summer of 1892, the nebulous ring about Nova Aurigae. He included in his studies most of the historical novae for which the positions could be determined—all of this with the 30-or 40-inch telescopes. He was an independent discoverer of Nova Aquilae on the night of June 8, 1918, the date of the American eclipse. After his return to the observatory, he found that he had photographed the star on 54 dates during the preceding 25 years, the Willard lens having been used on 4 dates and the Bruce telescope on the remainder. He then determined the star's brightness on these plates. His observations of this character were very numerous: he contributed no less than 14 papers or notes to cover Nova Persei of 1901 and 8 such papers on Nova Aquilae of 1918.

In view of the great range of temperature through which micrometric measurements are made with the 40-inch refractor, extending from $-25°$ F. ($-32°$ C.) to $+100°$ F. ($+38°$ C.), he began, in 1897, a series of control measures of the difference in declination between Atlas and Pleione. These observations were made on 506 nights during the past 25 years, and thus constitute a great mass of valuable unpublished material bearing both on the constancy of the telescope and micrometer and on that of the stars themselves.

At the Yerkes Observatory he kept up his micrometric observations of the fainter satellites of the planets, which he had begun at the Lick Observatory, and contributed to the Astronomical Journal 10 papers of observations of Saturn and several of Phœbe, the ninth satellite, which he caught as a very faint object in the opposition of 1904, when the planet was 17° south of the Equator. At the oppositions of 1906 and 1912–13, when he had a good ephemeris of the satellite, he observed it several times, and estimated it to be of the fourteenth magnitude. We believe that Professor Barnard's measures with the 40-inch telescope are the only visual determinations of the position that have yet been made of this difficult satellite. He observed visually Perrine's sixth satellite of Jupiter and published his measures in three papers.

Professor Barnard took part in the campaign for observation of the asteroid Eros, during the opposition of 1900 and 1901, for the determination of the solar parallax.

In 1897 Miss Catherine W. Bruce, of New York, at the solicitation of Professor Barnard, gave to the University of Chicago the sum of $7,000 for a photographic telescope of the highest type of excellence with which he could continue his photographic investigation of the Milky Way and comets. Experiments were at once begun with various types of portrait lens, some of them furnished by Mr. Brashear, in order to find which was the most suitable objective for the purpose. At this time the cameras were strapped to a small equatorial, which was later installed for instruction at the university. This search for a suitable objective was continued for several years, and in December, 1899, the quest led Mr. Barnard to Europe, for he was determined to secure an objective which would represent the highest quality attainable in optical construction. Several of the leading European firms made small objectives for the test, but choice was made of the 10-inch doublet produced by John A. Brashear, of Allegheny.

The small wooden observatory for the Bruce telescope, having a dome 15 feet in diameter, was erected, in 1904, at a point 350 feet from the great dome and a less distance from Mr. Barnard's own home. The interest accumulated on the Bruce fund was sufficient to pay for the building. Warner & Swasey had provided for the telescope the excellent mounting, of a new pattern particularly well adapted for the purpose. Besides the 10-inch doublet, the mounting carried a Voigtländer portrait lens of 6¼ inches aperture, which had been refigured by Brashear, and a 5-inch guiding telescope.

Professor Barnard was now provided with equipment which he had awaited for some years. When the sky was clear and not rendered useless by the obnoxious presence of the moon, Mr. Barnard was generally to be found there making a long exposure on some part of the Milky Way or on a comet, unless he had an assignment with the 40-inch telescope.

He was unhampered by any administrative or editorial duties, and free from any engagements in the classroom, so that he was able to gratify to the full his passion for observing. To him, a night at the great telescope was almost a sacred rite—an opportunity for a search for truth in celestial places. Rarely has a priest gone up into the temple with a deeper feeling of responsibility and of service than did this untiring astronomer go up into the great dome. He

was usually ready before the sun had set, and impatiently waiting until the darkness should be sufficient for him to "get the parallel" for the thread of the micrometer before he could observe faint objects. During the day preceding one of his nights, his associates in the observatory were generally conscious of his keen anxiety for a clear sky, as evidenced by a frequently repeated nervous cough, which was always worse if the prospects for the night were unfavorable.

It was a marvel to all of us that his bodily strength was equal to the tasks which he put upon himself. He was accustomed to get on with very little sleep, and if the night was cloudy he could never trust himself to relax, but was constantly on the lookout for a possible clearing of the sky. Nevertheless, he often appeared in his office by 7 o'clock in the morning, and began work on the reduction of his observations of the night before. He was a very painstaking and accurate computer, and it was seldom that the positions of any celestial objects measured by him required correction for any numerical errors after they were published. From about 1906 he had the valuable assistance of Mrs. Barnard's niece, Miss Mary R. Calvert, who helped in his computations and in his correspondence, and in filing and cataloguing the great number of photographs and reduction sheets which he accumulated. The bibliography at the end of this paper is based upon a card catalogue which she had prepared and kept up to date.

The nebulous regions of the Milky Way were always of much interest to Mr. Barnard, and he early discovered on his photographs great nebulous areas which had not been previously suspected. He investigated many cases of nebulous stars, or of stars which seemed to be involved in "nebulosity," a word which he commonly used to describe vague and indefinite nebulous matter, generally of great extent. In some cases the term may represent a real distinction between a gaseous nebula and one which yields a continuous spectrum; in other cases it may denote finely divided matter reflecting light from a stellar source. The following quotation is from one of his early papers, entitled "The great nebula of Rho Ophiuchi and the smallness of the stars forming the groundwork of the Milky Way." [1]

> For many years this part of the sky troubled me every time I swept over it in my comet seeking; though there seemed to be scarcely any stars here, there yet appeared a dullness of the field as if the sky were covered with a thin veiling of dust, that took away the rich blackness peculiar to many vacant regions of the heavens. This was fully fifteen years ago, at Nashville, Tennessee, when I searched for comets with a five-inch refractor.
>
> After going to the Lick Observatory, I still noticed this peculiarity of that part of the sky, and finally found that two small stars north of Antares were involved in nebulosity and that the whole region seemed to be covered with a very weak diluted nebulosity. . . .
>
> This part of the sky coming within the sphere of my work in photographing the Milky Way, on March 23, 1895, I made a photograph of it with $2^h\ 20^m$ exposure. The resulting negative showed a vast and magnificent nebula, intricate in form and apparently connected with many of the bright stars of that region, including Antares and Sigma Scorpii.

Professor Barnard had early formed a plan for securing a photographic chart of the Milky Way, and he was quick to accept an invitation from Professor Hale to bring the Bruce telescope to Mount Wilson for photographing particularly the southern portions of the Galaxy, in so far as they could be reached from that latitude. The telescope was, accordingly, transported to Mount Wilson, under the auspices of the Carnegie Institution of Washington, in January, 1905, and Professor Barnard spent about nine months on the mountain, engaged in this work. The telescope was back in its own dome at Williams Bay before the end of the year, and for the next 17 years was always available for Mr. Barnard's use, being seldom employed by any other observer.

In 1907 the Carnegie Institution undertook to publish the Atlas of the Milky Way when it should be ready, and during several years search was made for the best mode of reproduction of the pictures. Careful experiments were undertaken by experts in photogravure, and with the heliotype process, but the degree of perfection desired could not quite be attained. Finally, Mr. Barnard accepted the suggestion that photographic prints would most faithfully reproduce the wonderful details of the original negatives. Accordingly, with infinite pains, he made positives from the original negatives and then second negatives from which the prints could be prepared. In this way, the contrast in faint regions was increased and details were brought out

[1] Popular Astronomy, 5: 227, 1897.

which might otherwise have been lost. A firm of commercial photographers in Chicago, A. Copelin & Son, personally well known to Mr. Barnard, undertook the task of making the necessary number of 700 prints from each of the 50 negatives selected to represent the Galaxy. For two years, beginning in May, 1915, Mr. Barnard made frequent trips to the city, and personally inspected each of the 35,000 prints, rejecting hundreds and even thousands of those which seemed to him to be lacking, in some detail, the high quality of excellence which he desired.

It was very difficult for Mr. Barnard to take time from the reduction and discussion of current observations in order to devote himself to the descriptions which were to accompany the photographs. He was also constantly finding new points of interest, as he studied each photograph in detail, which led him to desire new photographs centering on special regions, or having longer exposures than he had previously given. The publication of this Atlas was accordingly delayed, but, fortunately, Mr. Barnard had been prevailed upon to give more time to the completion of the text, and it had been finished, so far as the 50 regions illustrated were concerned. It is to be regretted that he had not written the introduction, which would have summed up his views on the structure of the Milky Way, based upon a personal knowledge more intimate than that possessed by any other person. His notes on the introduction are fragmentary, but they can be used, and it is hoped that the Atlas can be published during 1926, in essentially the manner in which Mr. Barnard would have desired it, and accompanied by charts from drawings giving the coordinates of the region of each photograph, with a designation of the important features.

During the last decade, Mr. Barnard had taken a special interest in the dark markings in the Milky Way. At first he called them vacancies, and it was only gradually that he was led to the view that they were, after all, in many instances, dark objects projected against the Milky Way and absorbing its light. The titles of some of his papers show this gradual transition in the interpretation of these extraordinary structures.

In his paper entitled "Some of the dark markings of the sky and what they suggest," Astrophysical Journal, 43: 1–8, 1916, he says:

> An important fact that may come from our knowledge of the existence of dark nebulae is that their masses must be much greater than would be assumed for the ordinary nebulae, because they are perfectly opaque and must be relatively dense, and hence comparatively massive. If this is so, then we must take into account these great masses in a study of the motions of the stars as a whole.

In that paper he placed side by side a luminous gaseous nebula and a dark object of very nearly the same shape: the resemblance is striking.

One of his most notable papers, "On the dark markings of the sky, with a catalogue of 182 such objects," published in the Astrophysical Journal in 1919, summed up his studies of these objects, which will doubtless be designated in the future by the numbers which he assigned to them in the catalogue.

A very important question in recent years has been the proper location in our stellar system of the globular star clusters. From his studies of their appearance on his photographs of the Milky Way, Professor Barnard was led to the opinion that the clusters are in some instances obviously projected against the background of the Milky Way. To show his ideas as to the relative distances of some clusters and the Milky Way, we may quote from a short note published in the Astronomical Journal in 1920:

> Just as the great star clouds of the Milky Way act as a background against which non-luminous masses may be seen in dark relief, they must act also as a screen and thus hide any object that is behind them. This gives us a means of inferring the relative distances, etc., of many of the great globular clusters. The rich regions of Sagittarius and Aquila, in which some of the finest globular clusters occur, are specially remarkable for their density. That these clusters are nearer than the great star clouds is evident, for they would not be seen through the star clouds if beyond them.

He cites as particular examples, N. G. C. Nos. 6266 (M 62), 6273 (M 19), 6293, 6304, 6333 (M 9), 6528, 6656 (M 22), and 6712.

Although Professor Barnard had given great attention to the surface markings of the planets, it was not until 1905 that he began experiments in photographing the planets with the

large refractor, employing a secondary magnifying lens. It will be understood that the direct images of the planets are so small, even in an instrument of long focus, that the grain of the plates makes it impossible to secure a satisfactory photographic enlargement. This work required great skill and patience, because, as Professor Barnard said:[2]

> Better conditions are required for successful work in this direction than for visual observations. One can do much visually under conditions where the best definition is only momentary, but for these enlarged photographs any break in the definition for even a single second during the exposure means injury or total ruin to the image. In all the exposures, though of only a few seconds' duration, it was necessary to guide the telescope to keep the image stationary. This was done by bisecting the polar cap by cross-wires (spider threads) in the focus of the long guiding finder (61½ feet focus) of the 40-inch telescope.

It was intended that a full description of this photographic work on planets should be published in the Astrophysical Journal, but Professor Barnard never found time to do this. He obtained also pictures of Jupiter and of Saturn, but instants of the finest seeing when such work was in progress were too rare to yield entirely satisfactory pictures. When visiting Mount Wilson in 1911, Mr. Barnard obtained fine photographs of Saturn with the 60-inch reflector.

We have mentioned before that Mr. Barnard's early interest in the photography of comets and their tails did not abate after the Bruce telescope was put into operation at the Yerkes Observatory. He secured fine series of photographs of all that appeared in our sky, of which may be particularly named: Giacobini's of 1905–6, Daniel's of 1907, Morehouse's of 1908, Halley's of 1909–10, Brooks's of 1911, Delavan's of 1914. Of these, Comet Morehouse of 1908 and Brooks of 1911 exhibited the most remarkable activity in their caudal demonstrations, and their eccentricities kept Mr. Barnard almost constantly at the telescope while it was possible to photograph them.

The return of Halley's comet was awaited with the keenest anticipation by Mr. Barnard. He took many photographs of the region where it might be expected in 1909, and followed it persistently after it was revealed on Prof. Max Wolf's plate of September 11 of that year. The records of previous appearances of Halley's comet had been most carefully studied by Mr. Barnard, but there were many points on which the history was silent or incomplete. He determined to provide against this deficiency at the return of 1910. He kept very full notes on all his observations during the 20 months through which he was able to follow the comet, and embodied these in a long paper appearing in the Astrophysical Journal for June, 1914, entitled "Visual observations of Halley's comet in 1910" (39: 373–404). In this paper he says:

> Halley's comet at its return in 1910, though a brilliant and interesting object to the naked eye—especially in the month of May—was, nevertheless, a disappointment when considered from a photographic standpoint. It is safe to say that it did not give us any new information concerning these strange bodies.

The expected passage of the tail of the comet so close to the earth as to envelop it on May 18-19, 1910, kept Mr. Barnard on the qui vive, and the sky was watched throughout the day as well as the night with the greatest care. Mr. Barnard felt amply rewarded for his pains by the spectacle of the tail in the early morning of the 19th, when he could map it visually for a length of 120°; even on the preceding morning he had been able to record its length as 107°. He last saw the comet a year later, on May 23, 1911, when he secured a position of it with the 40-inch telescope, with some difficulty on account of its faintness.

In order to have observations of this comet made in longitudes otherwise unoccupied, the Committee on Comets of the American Astronomical Society, of which Professor Barnard was an active member, secured a grant from the Bache fund of the National Academy of Sciences, which made it possible to send Mr. Ferdinand Ellerman, of the Mount Wilson Observatory, to photograph it at Diamond Head, Hawaii. Mr. Barnard spent considerable time in preparing his part of the report of the committee, which was printed in the Publications of the Society in 1915.

It will be understood that in addition to his photographic observations of comets, Professor Barnard was always obtaining their positions with the filar micrometer of the 40-inch telescope, whenever such positions were necessary, upon the first appearance of a comet or after it became

[2] Monthly Notices of the Royal Astronomical Society, 71: 471, 1911.

too faint for moderate instruments. It may seem a little singular that the large number of long exposures made by Mr. Barnard on the Milky Way did not lead to the discovery of other new comets; but such was the case, and we may well believe that this possibility was not overlooked by one to whom comets had meant so much in his earlier career. In addition to those already mentioned, he was the first to observe, at their predicted return, Encke's comet in 1914 and Pons-Winnecke in 1921. On a plate taken while he was at Mount Wilson in 1905, he found, some months later, the impression of a comet, which received the name 1905 f, but was not observed elsewhere.

In spite of Professor Barnard's passion for exact measurement, he still regarded his splendid pictures rather from the point of view of a photographer than from that of an expert in measurement; of course, whenever it was necessary he obtained the positions of comets or other objects on the negatives, but in a general way he had these two distinct attitudes of mind in his work. He was somewhat reluctant to feel that the photographic procedure in astronomy could in many cases supersede the older visual methods for which, in some respects, a much higher degree of expert skill was necessary. His collection of some 1,400 negatives of comets contains material on which a vast amount of measurement could be made, and we trust that this will soon be done in the study of the peculiar internal motions of comets and their tails. The negatives of the Milky Way and of fields of the sky taken by Professor Barnard at Yerkes Observatory number about 3,500, in addition to about 500 taken by him at Mount Wilson. These constitute a rich field for investigation of stellar motions, for discovery of variable stars, and for statistical studies of the structure of the universe. It is hoped that these plates, which extend over nearly a score of years, may soon be investigated under the "blink" comparator for motions and variables, and it is certain that the full study of this splendid series of photographs will bring to light many important facts.

Occasionally Mr. Barnard had time to investigate pairs of plates under the "blink" comparator; thus, on confronting a plate taken in May, 1916, with one of the same field he had obtained with the Willard lens in August, 1894, he discovered the star in Ophiuchus having a proper motion of $10''.3$ per year, the greatest proper motion thus far detected. This motion was, in fact, so unexpectedly large as to make the discovery very difficult, but the plates were numerous enough to confirm its reality. The position of the object, familiarly known by our staff as Gilpin, was carefully measured by Mr. Barnard with the filar micrometer. Its parallax was investigated here and at other observatories and was found to be $0''.53$, corresponding to a distance of 6.1 light years, thus making this dwarf the nearest star, after the system of Alpha Centauri. With the large scale of the 40-inch telescope, photographs taken a week apart make the proper motion evident and measurable!

Professor Barnard was deeply interested in eclipses of the sun, and he secured with a visual lens some excellent photographs of the corona at the total eclipse of January 1, 1889, at The Willows, a point in California not far from Mount Hamilton.

In 1900, at the station of the Yerkes Observatory, at Wadesboro, N. C., he was again favored with a clear sky and secured excellent photographs with the horizontal telescope of 61½ feet focus, but here he denied himself the privilege of a direct view of the corona, remaining inside the spacious camera with Mr. Ritchey to assure the accuracy of the exposures and the perfection of the result. They saw the corona only as it was projected on the photographic film.

Mr. Barnard was invited to join the large expedition to Sumatra organized by the United States Naval Observatory for the total eclipse of May 18, 1901. His station was at Solok, and he planned every detail with the greatest of care for photographs of the corona on a very large scale. The duration of totality was very long, nearly a maximum of six minutes. It was tragic that a thick blanket of clouds prevented him from making any observations at that time. He was absent from the observatory for about six months, and this further deprived him of the opportunity of observing Nova Persei when it was bright.

He was greatly interested in the eclipse of June 8, 1918, and made a trip of inspection with the writer in September, 1917, to select suitable stations in Wyoming and Colorado. He

went on with the advance guard of our party to our principal station at Green River, Wyo., six weeks before the date of the eclipse. He took infinite pains in the adjustment of the horizontal telescope used with our coelostat, and could be content with nothing but perfection in the focusing of all the cameras for which he had any responsibility. Unfortunately, a great cloud drifted across an otherwise perfect sky on that afternoon, covering the sun until two or three minutes after totality was over. Mr. Barnard had been again interested in studying the conditions for the eclipse of September 10, 1923, and up to within a fortnight of his death we had still hoped that he might be a member of our party.

Lunar eclipses were not neglected by Professor Barnard. He had photographed successfully, at the Lick Observatory, the eclipses of 1894 and 1895, and it was his custom to make with the Bruce telescope many photographs of the different phases of each lunar eclipse which occurred in favorable weather. With that instrument he also kept a photographic record of all interesting conjunctions of the planets and similar occurrences.

The displays of the aurora, which are frequently visible in Wisconsin, were a delight to Professor Barnard, and he recorded fully their details and published two extended papers regarding them. His notes covering the aurora for almost another solar cycle are still unpublished. He also gave attention to the self-luminous night haze, which his long vigils had given him unusual opportunities to observe, and he presented two papers on the subject to the American Philosophical Society, one in 1911 and the other in 1919.

We thus find him a keen observer of nature in most of its visible phases. The meteors did not escape him or his photographic plate, nor did the 17-year locusts at their regular recurrence. In the growth of the trees which he had planted about his home he had great satisfaction, and he had much pleasure in following the development and planting of the grounds of the observatory, when that became possible a few years ago.

In his will Professor Barnard bequeathed to the University of Chicago his home, as a memorial to his wife, whose death occurred on May 25, 1921. To the Yerkes Observatory, he left also his scientific books and the medals and awards he had received in recognition of his notable services to science.

Professor Barnard's home had been a center of generous hospitality for a quarter of a century, and nowhere was he more entertaining that as host in his own home. He was full of humor and could tell most amusingly of his experiences in early life and of his travels. It has been the writer's good fortune to make many railroad trips with him, and he was always a most agreeable companion. He was shy and restive in larger companies where he was not well acquainted with the other guests, and was often quite nervous before giving a lecture on a subject with which he was perfectly familiar. After he was well started in an address he quite lost this shyness and would describe the intimate details of his pictures in a charming way. He never spoke more interestingly than in one of his last lectures which he gave one evening, on the subject of comets, at the meeting of the American Astronomical Society, at the Yerkes Observatory, in September, 1922.

Mr. Barnard's delicious vein of humor was very familiar to his intimate friends. He could tell of some of his varied experiences in a distinctly original manner. Conformity to the latest fashion was of no special concern to him. He was accustomed to wear a black tie of a type perhaps more familiar in the seventies, though evidently somehow procurable even at the present time. This variety is mounted on a pasteboard frame, and is attached, rather precariously, to the collar button by a small elastic cord. Accidental detachment was, therefore, of frequent occurrence; in fact, the presence or absence of the tie was sometimes used by a friend as a test of vision; but to the suggestion that a more modern type of tie, the kind that passes around the neck of the wearer, might save the inconvenience of perpetual uncertainty as to whether or not the tie was attached, the reply was made by Mr. Barnard: "Why, this kind of tie once saved my life!" To the surprised inquirer he added: "You see, I was at the Grand Canyon, and looking down into that vast chasm, suddenly the tie fell off and floated down half a mile into the depths below. What if it had been around my neck!"

Always mindful of the difficulties that he had to overcome in his early beginnings as an astronomer, Professor Barnard was most generous in giving advice and assistance to all sincere aspirants for knowledge of astronomy who approached him with questions, by letter or in person, and he gave his time freely to such, in exhibiting and explaining his most significant photographs. He willingly took his turn in speaking to the large number of visitors admitted to the Yerkes Observatory on Saturday afternoons, and often stayed long after the closing hour in explaining details to those who had evinced a real interest. He was most kind to other workers in astronomy, and tolerant in expressing opinions of them, even though their views might differ very greatly from his, and though he might regard them as radically wrong. He avoided controversy, and seldom took his pen to oppose the views of others.

Professor Barnard was not a teacher. He had missed the inspiration and opportunity of studying astronomy under some gifted enthusiast. He had, of course, profited by taking part in the work at an institution so well planned and organized as was the Lick Observatory by Director Holden, and had received much benefit from the sane counsel of his seniors there, particularly from Mr. Burnham; but he did not realize from experience the mutual importance of the relation of teacher and pupil, or know the satisfaction of the teacher in having an apt follower in his research to whom he may pass on the acquisitions of his years of study. Mr. Barnard could not bring himself to lose time at the telescope in having a pupil take part in measurements, which he could himself make so much better, and he begrudged the possible loss in quality of a photograph if some one less skilled than himself took some part in the guiding. Accordingly, he trained no one to be his successor; he left no disciple who could take up his work after receiving the benefit of his unequaled experience as an observer and of his exceptional knowledge of the heavenly bodies.

Mr. Barnard was stricken with diabetes early in the year 1914, and had to undergo the severe privation, by the doctor's orders, of giving up observations with the large telescope for a year. As a result of his obedience, his health was greatly improved, and for seven years longer he kept up his observing most industriously and really beyond the measure of his bodily strength. It was regarded by the director of the observatory as no small part of his duties to see that such a man should be induced to spare himself as much as possible and to restrict his night work both to save him from exhaustion and to gain time for the reduction and discussion of his great accumulation of observations. But it was almost impossible for Mr. Barnard to keep away from the Bruce photographic telescope when the sky was clear and the moon did not interfere.

He was greatly affected by the death of his wife after a brief illness and after 40 years of married life in which she had devoted herself completely to his comfort. They had no children, and thus Mr. Barnard missed the joys and responsibilities of parenthood, even as he had himself missed the experience of the relation of son to father.

His final illness was of only six weeks' duration, and began rather acutely. The best of medical skill was given him, and up to a short time before his death the specialist was hopeful of his recovery. He died at 8 o'clock on the evening of February 6, and simple funeral services were held on the following day in the rotunda of the observatory, which seemed to us the appropriate place. The interment was at Nashville, after services attended by many friends in his native city and from his alma mater. He had always been highly appreciated at Nashville, and one of the interesting evidences of this was the erection, not long ago, by the Nashville Automobile Club, in cooperation with the Nashville Historical Committee, of a tablet at the place in the city where the young enthusiast discovered his first comet in 1881.

Measured by the calendar, his life was but little more than 65 years, but, by the number of hours he had spent under the nocturnal sky or in the domes, his period of activity was more than that of many who had passed four score years.

His services to science were recognized by the learned societies. He was vice president of the American Association for the Advancement of Science in 1898, and delivered an address upon the "The development of photography in astronomy." In the same year he was elected

a foreign associate of the Royal Astronomical Society. He became a member of the American Academy of Arts and Sciences in 1892, of the American Philosophical Society in 1903, and of the National Academy of Sciences in 1911. He was made a director of the B. A. Gould fund, under the auspices of the National Academy, in 1914, and at the same time an associate editor of the Astronomical Journal. He had been for three years (1892–1894) associate editor of the journal Astronomy and Astrophysics. In addition to the medals mentioned heretofore in this article, he received from the French Academy of Sciences in 1893 the Arago gold medal, and in 1900 the Janssen gold medal; he was the recipient of the Janssen prize of the Astronomical Society of France in 1906, and was awarded the Bruce gold medal of the Astronomical Society of the Pacific in 1917. The last-named society had three times awarded him the Donohoe comet medal. He was given the honorary degree of doctor of science by Vanderbilt University in 1893, and that of doctor of laws by Queen's University, of Kingston, Ontario, in 1909.

The voluminous character of his contributions to astronomical literature has already been indicated, but we may add that a card catalogue of his writings includes no less than 900 items, without being complete. A bibliography of his articles which appeared since his connection began with the University of Chicago in 1895 has been published in the list of publications of the faculties issued annually by the University of Chicago and also collected in two special volumes of this character covering the first 25 years of the work of the university. These contain the titles of 377 articles and 6 book reviews by Professor Barnard, to which number will be added, as time permits their preparation, numerous posthumous papers covering his unpublished observations.

His last measurements with the 40-inch telescope were made on the night of December 16, 1922, when he secured the position of Baade's comet; his last visual observations with that instrument were on December 21, when he made 19 estimates of the brightness of Nova Persei, referred to 13 comparison stars; and his last use of that instrument was later on that night when he made a photograph of the cluster Messier 36 with an exposure of two hours. A photograph of the region of Gamma Leonis, made on the following night with the Bruce telescope, closed his long and untiring work with that instrument. His last visual observation was of the occulation of Venus on the morning of January 13, 1923, which he observed from the window of his sick room. Thus closes the record of the astronomical activity of one of the greatest observers of our time, of whom may be truly said, "Aperuit caelos."

20154°—26——36

BIBLIOGRAPHY, BY TOPICS, OF THE PRINCIPAL SCIENTIFIC PAPERS OF EDWARD EMERSON BARNARD

We are indebted to Miss Mary R. Calvert for the preparation of the following bibliography, which has been arranged topically, and without giving the exact title of each paper. We believe that it will be of more convenience to the user to have the articles on a given subject grouped in this manner. A strict chronological order has not been followed in the arrangement under each topic, but it seemed wiser to collect together the papers which appeared in the same journal, and a regular order has been followed where a series of journals contained the papers. Titles have not been given in full except for papers which were of a special importance or were of a general character and summed up Professor Barnard's studies in that particular field, but many minor contributions and notes have not been included in this list.

The reference "Monthly Notices" refers to the Monthly Notices of the Royal Astronomical Society of London.

BOOKS

Photographs of the Milky Way and of comets. Publications of the Lick Observatory, vol. 11.

An atlas of the Milky Way (illustrated by fifty prints taken with the Bruce photographic telescope of regions north of 40° S. declination). In preparation for publication by the Carnegie Institution of Washington.

Micrometric measurements in some of the globular star clusters. To appear in the Publications of the Yerkes Observatory.

ARTICLES

DOUBLE STARS

Duplicity of β Capricorni. Astronomische Nachrichten, vol. 108, 1884, p. 369; vol. 110, 1884, p. 133.

Measures of the companion of Sirius. Astronomische Nachrichten, vol. 182, 1909, pp. 13–16; vol. 214 (Jubiläumsnummer), 1921, p. 3. Astronomical Journal, vol. 20, 1900, p. 167; vol. 23, 1903, p. 132; vol. 26, 1910, p. 143; vol. 27, 1912–1913, pp. 107, 193; vol. 29, 1915, p. 38; vol. 30, 1916, p. 24; vol. 30, 1917, p. 182; vol. 32, 1919, p. 56.

Measures of the companion of Procyon. Astronomische Nachrichten, vol. 182, 1909, pp. 13–14. Astronomical Journal, vol. 19, 1898, pp. 23–24; vol. 20, 1899, p. 44; vol. 21, 1900, p. 16; vol. 23, 1903, p. 132; vol. 27, 1912–1913, pp. 107, 194; vol. 30, 1917, p. 214.

On the hypothetical disturbing body in the system of 61 Cygni. Astronomische Nachrichten, vol. 172, 1906, pp. 379–384.

Micrometric measures of double stars. Astronomische Nachrichten, vol. 149, 1899, pp. 167–170; vol. 172, 1906, pp. 383–384. Astronomical Journal, vol. 18, 1897, p. 46; vol. 19, 1898, pp. 23–24, 113–116; vol. 20, 1900, p. 170.

On the parallax and proper motion of the double star Krueger 60. Monthly Notices of the Royal Astronomical Society, vol. 68, 1908, pp. 629–655; vol. 76, 1916, pp. 592–606. Astronomical Journal, vol. 21, 1900, p. 64; vol. 23, 1903, p. 169.

CLUSTERS

Photographic determination of the colors of some of the stars in the cluster M 13 (Hercules). Astrophysical Journal, vol. 12, 1900, pp. 176–181; vol. 29, 1909, pp. 72–75; vol. 40, 1914, pp. 173–181.

Triangulation of clusters. Publications of the Astronomical and Astrophysical Society of America, vol. 1, 1899–1902, pp. 77, 193.

Remeasurement of Hall's stars in the Pleiades. Popular Astronomy, vol. 27, 1919, p. 523.

On the motion of some of the stars in the cluster M 92. Astronomische Nachrichten, vol. 176, 1907, pp. 17–24; vol. 182, 1909, pp. 305–308. Popular Astronomy, vol. 18, 1910, pp. 3–7. Publications of the Astronomical and Astrophysical Society of America, vol. 1, 1909, p. 323.

Sur des grands mouvements propres soupçonnés dans les étoiles voisines de l'amas Messier 11 = N. G. C. 6705. Comptes Rendus, vol. 161, 1915, p. 411.

VARIABLE STARS

Discovery of variable stars. Astronomical Journal, vol. 19, 1899, p. 193; vol. 29, 1916, pp. 75, 148; vol. 30, 1916, p. 4; vol. 30, 1917, pp. 84, 176; vol. 32, 1919, pp. 37, 48, 102. Astrophysical Journal, vol. 12, 1900, pp. 182–184.

Observations of variable stars. Astronomische Nachrichten, vol. 147, 1898, pp. 243–248; vol. 149, 1899, pp. 167–172; vol. 172, 1906, pp. 345–348; vol. 191, 1912, pp. 387–390. Monthly Notices, vol. 55, 1895, p. 442; vol. 73, 1913, pp. 390–392, 500–518; vol. 76, 1915, pp. 95–97. Popular Astronomy, vol. 27, 1919, pp. 374, 485.

Observations of the variable star No. 33 in M 5. Astronomische Nachrichten, vol 184, 1910, pp. 273–284; vol. 191, 1912, pp. 439–442; vol. 196, 1913, pp. 11–14.

Observations of the variable star RS Aquarii. Astronomische Nachrichten, vol. 173, 1907, pp. 337–346; vol. 181, 1909, pp. 309–312; vol. 191, 1912, pp. 181–182.

PROPER MOTION STARS

A small star with the largest known proper motion. Astronomical Journal, vol. 29, 1916, pp. 181–184; vol. 30, 1917, pp. 79–80. Popular Astronomy, vol. 24, 1916, pp. 504–508.

On the motion of a small star in the Pleiades. Astronomische Nachrichten, vol. 194, 1913, pp. 7–8.

Max Wolf's two small proper motion stars in Serpens (A. N. 4981). Astronomical Journal, vol. 33, 1920, pp. 69–70.

NOVAE

The temporary stars. On the present appearance of some of these bodies. Astronomische Nachrichten, vol. 194, 1913, pp. 401–408.

Some peculiarities of the Novae. Proceedings of the American Philosophical Society, vol. 61, 1922, pp. 99–106.

Recent observations of some of the Novae. Publications of the Astronomical Society of the Pacific, vol. 34, 1922, p. 164.

On the identity of Anthelm's new star of 1670 (11 Vulpeculae), Astronomische Nachrichten, vol. 199, 1914, pp. 1–4.

Hind's new star of 1848 (Nova Ophiuchi No. 2). Monthly Notices, vol. 81, 1921, pp. 167–173.

Nova T Coronae of 1866. Astrophysical Journal, vol. 25, 1907, pp. 279–282.

Nova Cygni of 1876. Monthly Notices, vol. 62, 1902, pp. 405–419; vol. 72, 1912, pp. 525–534, 676; vol. 82, 1921, pp. 116–120. Astronomical Journal, vol. 32, 1919, p. 6.

Nova Andromedae of 1885. Astronomische Nachrichten, vol. 112, 1885, p. 403. Sidereal Messenger, vol. 4, 1885, pp. 240, 247, 308. Astrophysical Journal, vol. 8, 1898, p. 226.

Nova Aurigae of 1891. Astronomische Nachrichten, vol. 130, 1892, pp. 344, 407; vol. 131, 1893, p. 377; vol. 135, 1894, p. 385; vol. 137, 1895, p. 233. Monthly Notices, vol. 53, 1892, p. 35; vol. 62, 1901, pp. 61–65. Popular Astronomy, vol. 2, 1895, p. 307.

Nova Sagittarii of 1898. Astronomische Nachrichten, vol. 173, 1906, pp. 113–118.

Nova Persei of 1901. Astronomische Nachrichten, vol. 159, 1902, pp. 49–58; vol. 160, 1902, p. 239; vol. 162, 1903, pp. 379–382; vol. 176, 1907, pp. 323–326; vol. 179, 1908, pp. 199–204; vol. 198, 1914, pp. 123–124. Monthly Notices, vol. 80, 1920, pp. 710–722; vol. 82, 1922, pp. 457–461. Astronomical Journal, vol. 30, 1917, p. 86. Astrophysical Journal, vol. 14, 1901, pp. 151–157; vol. 16, 1902, pp. 183–185.

Nova Geminorum of 1903. Astronomische Nachrichten, vol. 198, 1914, pp. 123–124. Monthly Notices, vol. 66, 1906, pp. 355–361. Astronomical Journal, vol. 23, 1903, p. 81. Astrophysical Journal, vol. 17, 1903, pp. 376–377.

Nova Lacertae of 1910. Astronomische Nachrichten, vol. 187, 1911, pp. 63–64; vol. 194, 1913, p. 405. Monthly Notices, vol. 72, 1912, pp. 659–676.

Nova Geminorum of 1912. Astronomische Nachrichten, vol. 196, 1913, pp. 167–168; vol. 198, 1914, pp. 123–124. Monthly Notices, vol. 72, 1912, p. 675.

Nova Aquilae of 1918. Monthly Notices, vol. 80, 1920, pp. 582–586. Astrophysical Journal, vol. 49, 1919, pp. 199–202. Popular Astronomy, vol. 26, 1918, pp. 492, 558–561; vol. 27, 1919, p. 450. Publications of the Astronomical Society of the Pacific, vol. 32, 1920, pp. 222–225.

Nova Ophiuchi of 1919. Publications of the Astronomical Society of the Pacific, vol. 32, 1920, pp. 226–227.

NEBULAE

Visual observations of nebulae. Astronomische Nachrichten, vol. 104, 1883, p. 285; vol. 108, 1884, p. 369; vol. 110, 1884, p. 125; vol. 112, 1885, p. 391; vol. 113, 1885, pp. 31, 47; vol. 115, 1886, pp. 315, 323; vol. 122, 1889, p. 253; vol. 125, 1890, pp. 315, 379; vol. 126, 1890, p. 53; vol. 126, 1891, pp. 277, 293; vol. 127, 1891, p. 135; vol. 129, 1892, p. 17; vol. 130, 1892, pp. 7, 77, 223, 233; vol. 134, 1893, p. 129; vol. 138, 1895, p. 211; vol. 139, 1895, pp. 41, 43; vol. 173, 1906, pp. 117–124; vol. 177, 1908, pp. 231–236. Monthly Notices, vol. 49, 1889, p. 418; vol. 55, 1895, p. 442; vol. 56, 1895–1896, pp. 66, 162; vol. 59, 1899, pp. 372–376. Sidereal Messenger, vol. 1, 1882, pp. 57, 135, 238; vol. 2, 1883, p. 226; vol. 3, 1884, pp. 60, 91, 184, 189; vol. 4, 1885, pp. 30, 53, 124, 125, 223, 247, 313, 315; vol. 5, 1886, pp. 25, 27, 31, 154, 255, 286. Astronomical Journal, vol. 11, 1891, pp. 37, 168; vol. 15, 1895, p. 24; vol. 18, 1897, p. 112; vol. 30, 1917, pp. 175–176. Astronomy and Astrophysics, vol. 13, 1894, p. 642. Astrophysical Journal, vol. 2, 1895, p. 350; vol. 8, 1898, pp. 226–228; vol. 14, 1901, pp. 151–157.

On the exterior nebulosities of the Pleiades. Astronomische Nachrichten, vol. 136, 1894, p. 193. Monthly Notices, vol. 59, 1899, p. 155; vol. 60, 1900, pp. 258–261. Astronomy and Astrophysics, vol. 13, 1894, p. 768.

The great photographic nebula of Orion, encircling the belt and Theta nebula. Astronomy and Astrophysics, vol. 13, 1894, p. 811, reprinted in Popular Astronomy, vol. 2, 1894, p. 151.

On the probable motion of the annular nebula of Lyra (M 57) and the peculiarities in the focus for the planetary nebulæ and their nuclei. Astronomische Nachrichten, vol. 140, 1896, p. 283. Monthly Notices, vol. 60, 1900, pp. 245–257; vol. 66, 1906, pp. 104–113.

On the Owl nebula, Messier 97=N. G. C. 3587. Monthly Notices, vol. 67, 1907, pp. 543–550, 577.

The variability of the nucleus of the planetary nebula N. G. C. 7662. Monthly Notices, vol. 68, 1908, pp. 465–480.

Measures of small stars in the Dumb-bell nebula (N. G. C. 6853). Monthly Notices, vol. 62, 1902, pp. 466–468; vol. 76, 1915, pp. 90–95. Astronomy and Astrophysics, vol. 13, 1894, p. 445.

Observations of planetary nebulae. Astronomische Nachrichten, vol. 151, 1900, pp. 355–356; vol. 172, 1906, p. 123. Astronomical Journal, vol. 30, 1917, p. 208.

CELESTIAL PHOTOGRAPHY

General papers. Monthly Notices, vol. 50, 1890, pp. 310–314; vol. 57, 1896, pp. 10–16. Sidereal Messenger, vol. 6, 1887, p. 58; vol. 10, 1891, p. 331. Astrophysical Journal, vol. 2, 1895, pp. 351–353; vol. 8, 1898, p. 240; vol. 21, 1905, pp. 35–48. Proceedings of the American Philosophical Society, vol. 46, 1907, pp. 417–429, reprinted in Popular Astronomy, vol. 16, 1908, pp. 286–298. The Photographic Times, vol. 27, 1895, p. 65.

A simple and rapid method on detecting changes on celestial photographs due to motion or variability of the celestial bodies. Astronomische Nachrichten, vol. 130, 1892, p. 77.

On an instrument for accurately photographing an unseen, moving, but known celestial body. Astronomische Nachrichten, vol. 144, 1897, pp. 331–332.

The development of photography in astronomy. Proceedings of the American Association for the Advancement of Science, vol. 47, 1898, pp. 19–51,reprinted in Popular Astronomy, vol. 6, 1898, pp. 425–455.

MILKY WAY

Photographic studies of the Milky Way. Monthly Notices, vol. 56, 1895, p. 63; vol. 59, 1899, pp. 354–370. Knowledge, vol. 13, 1890, pp. 174–175,* 186; vol. 14, 1891, pp. 50,* 93, 232*; vol. 15, 1892, pp. 14, 16; vol. 16, 1893, pp. 10–12*; vol. 17, 1894, pp. 16, 17, 61–62*; vol. 19, 1896, p. 205. Astronomy and Astrophysics, vol. 13, 1894, pp. 177, 791. Astrophysical Journal, vol. 1, 1895, p. 10; vol. 2, 1895, p. 58; vol. 5, 1897, p. 1; vol. 9, 1899, pp. 157–158; vol. 17, 1903, pp. 77–80; vol. 23, 1906, pp. 144–147; vol. 41, 1915, pp. 253–258. The Journal of the Royal Astronomical Society of Canada, vol. 16, 1922, pp. 51–53.

On a great photographic nebula in Scorpio near Antares. Astronomische Nachrichten, vol. 138, 1895, p. 211–214. Monthly Notices, vol. 55, 1895, p. 453. Popular Astronomy, vol. 5, 1897, pp. 227–232.

On the comparative distances of certain globular clusters and the star clouds of the Milky Way. Astronomical Journal, vol. 33, 1920, p. 86.

On a nebulous groundwork in the constellation Taurus. Astrophysical Journal, vol. 25, 1907, pp. 218–225. Reprinted in Bulletin de la Société Belge d'Astronomie, vol. 13, 1908, pp. 129–136.

On a great nebulous region and on the question of absorbing matter in space, etc. Astrophysical Journal, vol. 31, 1910, pp. 8–14.

Observations of dark regions in the sky. Astronomische Nachrichten, vol. 108, 1884, p. 369. Sidereal Messenger vol. 2, 1883, p. 259. Astrophysical Journal, vol. 38, 1913, pp. 496–501; vol. 43, 1916, pp. 1–8. Popular Astronomy, vol. 14, 1906, pp. 579–583; vol. 23, 1915, p. 596; vol. 26, 1918, p. 685. Proceedings of the National Academy of Sciences, vol. 1, 1915, pp. 394–396. Publications of the American Astronomical Society, vol. 3, 1915, p. 116.

On the dark markings of the sky, with a catalogue of 182 such objects. Astrophysical Journal, vol. 49, 1919, pp. 1–23, 360.

THE SUN

Observations of solar eclipses. Reports on the observations of eclipses, published by the Lick Observatory, 1889 and 1891. Astronomical Journal, vol. 18, 1897, p. 23. Popular Astronomy, vol. 9, 1901, pp. 527–544. Publications of the United States Naval Observatory, 2d series, vol. 4, Appendix 1, pp. D211–D216.

THE MOON

Observations of the moon. Astronomische Nachrichten, vol. 130, 1892, p. 7. Monthly Notices, vol. 55, 1895, p. 456. Sidereal Messenger, vol. 9, 1890, p. 282. Astronomical Journal, vol. 10, 1890, p. 31; vol. 22, 1901, p. 33. Astrophysical Journal, vol. 2, 1895, pp. 347–349. Nature, vol. 66, 1902, pp. 5–6.

Periodical changes in the size of the glow surrounding the lunar crater Linné. Astronomische Nachrichten, vol. 170, 1906, pp. 293–296.

Lunar eclipses. Astronomische Nachrichten, vol. 161, 1903, pp. 81–84. Sidereal Messenger, vol. 8, 1889, p. 137. Popular Astronomy, vol. 3, 1895, p. 102; vol. 21, 1913, pp. 277–278.

* Text by A. C. Ranyard.

THE PLANETS

A micrometrical determination of the dimensions of the planets and satellites of the solar system. Astronomische Nachrichten, vol. 157, 1902, pp. 261–268. Monthly Notices, vol. 55, 1895, pp. 367–382, 493; vol. 56, 1896, pp. 163–172; vol. 58, 1898, pp. 216–218. Astronomical Journal, vol. 14, 1894, p. 97; vol. 27, 1912, pp. 99–104. Astrophysical Journal, vol. 40, 1914, pp. 259–267. Popular Astronomy, vol. 2, 1895, pp. 295–297; vol. 5, 1897, pp. 285–302.

Measures and notes on the diameters and ellipticity of Uranus. Astronomical Journal, vol. 16, 1896, pp. 73–78, 109.

Transit of Mercury, May 9, 1891, and November 9–10, 1894. Sidereal Messenger, vol. 10, 1891, p. 291. Astronomical Journal, vol. 14, 1895, pp. 177–180. Popular Astronomy, vol. 2, 1894, p. 168.

Transit of Venus. Astronomische Nachrichten, vol. 105, 1883, p. 231. Sidereal Messenger, vol. 1, 1883, p. 290.

Physical and micrometrical observations of Venus. Astronomische Nachrichten, vol. 126, 1891, p. 295. Astrophysical Journal, vol. 5, 1897, pp. 299–304.

Observations of Mars. Sidereal Messenger, vol. 8, 1889, p. 428. Astronomical Journal, vol. 12, 1892, p. 91; vol. 23, 1903, p. 166. Astronomy and Astrophysics, vol. 11, 1892, p. 680. Popular Astronomy, vol. 19, 1911, pp. 498–501.

Observations of the polar caps of Mars. Astronomical Journal, vol. 20, 1899, p. 124. Astrophysical Journal, vol. 17, 1903, pp. 249–257. Popular Astronomy, vol. 2, 1895, pp. 433–443.

Measures of the diameters of Mars and the positions of his satellites. Astronomical Journal, vol. 17, 1897, pp. 145–147; vol. 26, 1910, pp. 69–70.

Photographs of Mars. Monthly Notices, vol. 71, 1911, pp. 471–472.

Jupiter, physical observations. Astronomische Nachrichten, vol. 128, 1891, p. 291; vol. 178, 1908, p. 389; vol. 181, 1909, pp. 303–310; vol. 196, 1913, pp. 381–382. Monthly Notices, vol. 51, 1891, pp. 543–549; vol. 52, 1892, pp. 6–16, 156. Sidereal Messenger, vol. 8, 1889, p. 222; vol. 10, 1891, p. 413. Astronomical Journal, vol. 8, 1888, p. 64; vol. 9, 1889, p. 85; vol. 12, 1893, p. 162; vol. 14, 1894, p. 101; vol. 16, 1896, p. 49; vol. 20, 1899, p. 129; vol. 25, 1905, p. 32; vol. 25, 1906, p. 83; vol. 25, 1907, p. 177; vol. 26, 1910, p. 125; vol. 27, 1912, p. 101; vol. 29, 1916, p. 79. Astronomy and Astrophysics, vol. 11, 1892, pp. 93, 686; vol. 13, 1894, pp. 277, 736. Popular Astronomy, vol. 2, 1894, p. 129. Publications of the Astronomical Society of the Pacific, vol. 1, 1889, pp. 89–111. Scientific American, vol. 43, 1880, p. 356 (this was the first contribution of a scientific article by Professor Barnard).

Jupiter, observations of satellites. Astronomische Nachrichten, vol. 125, 1890, p. 317; vol. 128, 1891, p. 45; vol. 134, 1894, p. 229; vol. 144, 1897, p. 321; vol. 172, 1906, pp. 119–122; vol. 174, 1907, p. 327; vol. 179, 1908, p. 17. Monthly Notices, vol. 51, 1891, pp. 543, 556, 557; vol. 52, 1892, p. 156; vol. 54, 1894, p. 134; vol. 55, 1895, pp. 382–390. Sidereal Messenger, vol. 2, 1883, p. 31; vol. 4, 1885, pp. 124, 158; vol. 5, 1886, pp. 155, 184. Astronomical Journal, vol. 27, 1912, p. 103; vol. 29, 1916, p. 73. Astronomy and Astrophysics, vol. 13, 1894, pp. 272, 438, 821.

Discovery and observations of a fifth satellite of Jupiter, period, etc. Astronomische Nachrichten, vol. 131, 1892, pp. 73, 135; vol. 137, 1894, p. 45; vol. 181, 1909, pp. 301–310. Monthly Notices, vol. 53, 1892, p. 36. Astronomical Journal, vol. 12, 1892, pp. 81–85, 103; vol. 12, 1893, pp. 161–174; vol. 13, 1893, p. 141; vol. 14, 1894, pp. 97–104, 135, 150; vol. 16, 1896, pp. 49–50; vol. 20, 1899, pp. 125–130; vol. 23, 1903, pp. 149–156; vol. 25, 1905, pp. 25–32; vol. 25, 1906, pp. 81–83; vol. 25, 1907, pp. 177–178; vol. 26, 1910, pp. 123–125; vol. 27, 1912, pp. 99–104; vol. 29, 1916, pp. 77–79. Astronomy and Astrophysics, vol. 11, 1892, p. 749; vol. 12, 1893, p. 788; vol. 13, 1894, pp. 556–557. Popular Astronomy, vol. 1, 1893, pp. 76–82. Bulletin of the Yerkes Observatory, University of Chicago, No. 10, 1899.

Saturn, physical observations, etc. Astronomische Nachrichten, vol. 141, 1896, pp. 79–80; vol. 162, 1903, p. 275. Monthly Notices, vol. 68, 1908, pp. 366–368. Astronomical Journal, vol. 23, 1903, pp. 143–144, 180–181. Popular Astronomy, vol. 11, 1903, p. 408.

Saturn, observations of the ring system. Astronomische Nachrichten, vol. 199, 1914, pp. 101–102. Monthly Notices, vol. 52, 1892, pp. 419–423; vol. 68, 1908, pp. 346–366, 494; vol. 69, 1909, pp. 621–624; vol. 73, 1913, pp. 601–605. Astronomical Journal, vol. 11, 1891, p. 42; vol. 26, 1910, p. 82; vol. 27, 1912, p. 129. Astronomy and Astrophysics, vol. 11, 1892, p. 119. Astrophysical Journal, vol. 27, 1908, pp. 35–44, 234–235.

Saturn, observations of satellites. Astronomische Nachrichten, vol. 166, 1904, pp. 141, 159; vol. 172, 1906, pp. 191–192; vol. 177, 1908, pp. 145, 147–150; vol. 194, 1913, p. 333; vol. 198, 1914, p. 223. Monthly Notices, vol. 50, 1890, p. 107. Astronomical Journal, vol. 24, 1905, pp. 165–166; vol. 26, 1910, pp. 79–82; vol. 27, 1912, pp. 116–129; vol. 28, 1913, pp. 1–9, 32; vol. 29, 1915, pp. 33–37; vol. 30, 1916, pp. 33–40; vol. 31, 1918, pp. 49–53. Astronomy and Astrophysics, vol. 11, 1892, p. 119. Popular Astronomy, vol. 27, 1919, p. 57.

Observations of the satellites of Uranus. Astronomical Journal, vol. 16, 1896, pp. 73–78; vol. 26, 1909, pp. 47–50; vol. 27, 1912, pp. 104–106; vol. 29, 1915, pp. 39–40; vol. 30, 1916, pp. 20–21; vol. 32, 1919, pp. 105–107.

Observations of the satellite of Neptune. Astronomische Nachrichten, vol. 181, 1909, pp. 321–326. Astronomical Journal, vol. 13, 1893, pp. 10–12; vol. 14, 1894, pp. 9–11; vol. 15, 1895, pp. 41–44; vol. 19, 1898, pp. 25–29; vol. 20, 1899, pp. 41–44; vol. 22, 1901, pp. 27–31; vol. 23, 1903, pp. 105–108; vol. 25, 1906, pp. 41–42, 100; vol. 25, 1907, p. 164; vol. 26, 1910, p. 144; vol. 27, 1912, pp. 111–112; vol. 28, 1913, pp. 10–11; vol. 29, 1915, p. 39; vol. 30, 1916, pp. 2–4; vol. 30, 1917, pp. 214–216; vol. 32, 1919, pp. 103–104.

Micrometrical measures of the minor planet Eros for the determination of the solar parallax. Astronomical Journal, vol. 21, 1900, p. 29. The University of Chicago Decennial Publications, First Series, vol. 8, issued also as Publications of the Yerkes Observatory, vol. 2, pp. 77–116.

ASTEROIDS

Micrometric observations of asteroids. Astronomische Nachrichten, vol. 126, 1890, p. 53; vol. 128, 1891, p. 199; vol. 136, 1894, p. 31; vol. 172, 1906, pp. 121–124; vol. 176, 1907, pp. 89–92; vol. 177, 1908, pp. 149–152; vol. 185, 1910, pp. 325–328; vol. 189, 1911, pp. 193–194; vol. 190, 1912, pp. 397–398; vol. 192, 1912, pp. 115–116; vol. 194, 1913, pp. 171–172. Astronomical Journal, vol. 30, 1917, pp. 78–79; vol. 32, 1919, pp. 53–56; vol. 32, 1920, pp. 159–160; vol. 33, 1920, pp. 42–45.

Micrometric measures of the diameters of some of the asteroids. Monthly Notices, vol. 54, 1894, p. 571; vol. 56, 1895, p. 55; vol. 60, 1900, pp. 261–262; vol. 61, 1900, pp. 68–69. Astromony and Astrophysics, vol. 13, 1894, p. 341.

COMETS

Observations of comets for position. Astronomische Nachrichten, vol. 102, 1882, p. 155; vol. 104, 1883, p. 287; vol. 112, 1885, pp. 371, 375; vol. 115, 1886, p. 323; vol. 116, 1887, p. 251; vol. 117, 1887, pp. 57, 243, 327, 337, 385; vol. 120, 1888, pp. 65, 91, 237; vol. 121, 1889, pp. 223, 301, 335; vol. 122, 1889, pp. 115, 309–314; vol. 124, 1890, p. 409; vol. 125, 1890, pp. 43, 177–195, 377; vol. 126, 1890–91, pp. 137, 231; vol. 127, 1891, pp. 231, 367; vol. 128, 1891, pp. 47, 237, 239; vol. 131, 1892, p. 63; vol. 180, 1909, pp. 159–162; vol. 181, 1909, pp. 401–402; vol. 190, 1911, pp. 25–28. Sidereal Messenger, vol. 4, 1885, pp. 179, 252; vol. 5, 1886, pp. 89, 218, 275; vol. 6, 1887, pp. 114, 161. Astronomical Journal, vol. 7, 1887, pp. 63, 72, 79, 96, 99, 111, 126, 127; vol. 8, 1888–89, pp. 102, 108, 110, 120, 134, 168, 183; vol. 9, 1889–90, pp. 5, 29, 47, 54, 55, 66, 86, 87, 119, 131, 142, 189; vol. 10, 1890–91, pp. 4–7, 88, 92, 95, 100, 110, 111, 125, 127, 136, 148, 151, 190; vol. 11, 1891–92, pp. 5–7, 13, 36, 51–54, 62, 63, 75, 79, 158, 160; vol. 12, 1892–93, pp. 87, 102, 112, 120, 126, 149, 157, 175; vol. 13, 1893–94, pp. 157, 165, 204; vol. 14, 1894–95, pp. 31, 151, 166; vol. 15, 1895, pp. 9–10, 128, 143; vol. 19, 1898, p. 55; vol. 22, 1902, p. 174; vol. 23, 1903, p. 8; vol. 24, 1905, pp. 164, 180; vol. 25, 1906, p. 60; vol. 26, 1909–10, pp. 43–44, 62, 76–77, 86, 137; vol. 27, 1912, pp. 113–114, 147–152; vol. 28, 1914, pp. 137–141; vol. 30, 1916, pp. 17–19, 25–29; vol. 32, 1919–20, pp. 58–59, 145–149.

Physical observations of comets. Astronomische Nachrichten, vol. 102, 1885, p. 207; vol. 104, 1883, pp. 267–272; vol. 120, 1888, p. 43; vol. 122, 1889, p. 267; vol. 125, 1890, pp. 177–196; vol. 128, 1891, p. 425; vol. 185, 1910, pp. 229–234; vol. 186, 1910, pp. 11–12; vol. 188, 1911, pp. 215–216. Sidereal Messenger, vol. 1, 1882, pp. 57, 221; vol. 1, 1883, p. 255; vol. 2, 1883, pp. 192, 226, 257, 286; vol. 3, 1884, pp. 28, 54–55, 168, 188, 313; vol. 5, 1886, pp. 157, 191, 288, 310; vol. 8, 1889, p. 360; vol. 9, 1890, pp. 116, 420; vol. 10, 1891, p. 287. Knowledge, vol. 15, 1892, p. 229; vol. 17, 1894, p. 34. Astronomical Journal, vol. 7, 1887, p. 41; vol. 9, 1889, pp. 32, 77; vol. 10, 1890, p. 67; vol. 11, 1891, p. 46; vol. 12, 1892, p. 127; vol. 20, 1899, p. 60; vol. 25, 1906, pp. 83–84; vol. 28, 1914, pp. 137–141; vol. 29, 1915–16, pp. 40, 138; vol. 30, 1916, pp. 25–29. Astronomy and Astrophysics, vol. 11, 1892, pp. 386–388. Astrophysical Journal, vol. 3, 1896, pp. 41–46; vol. 39, 1914, pp. 373–404; vol. 46, 1917, pp. 83–84; vol. 51, 1920, pp. 102–106. Popular Astronomy, vol. 20, 1912, pp. 579–580; vol. 22, 1914, pp. 500, 535–536. Science, vol. 40, 1914, pp. 769–770.

Photographic observations of comets. Astronomische Nachrichten, vol. 174, 1907, pp. 3–8; vol. 175, 1907, pp. 377–380; vol. 188, 1911, pp. 117–118; vol. 196, 1913, pp. 181–182. Monthly Notices, vol. 53, 1892, p. 36; vol. 69, 1908, pp. 52–54, 114–115, 624–626. Astronomical Journal, vol. 26, 1910, pp. 76–77; vol. 27, 1912, pp. 113–114. Astronomy and Astrophysics, vol. 13, 1894, pp. 421, 706, 789. Astrophysical Journal, vol. 18, 1903, pp. 210–217; vol. 22, 1905, pp. 249–255; vol. 24, 1906, pp. 255–258; vol. 28, 1908, pp. 292–299, 384–388; vol. 29, 1909, pp. 65–71; vol. 36, 1912, pp. 1–13. Popular Astronomy, vol. 1, 1893, p. 145; vol. 3, 1895, p. 13; vol. 12, 1904, pp. 1–5; vol. 16, 1908, pp. 591–596; vol. 17, 1909, pp. 531, 597–609; vol. 18, 1910, pp. 321–322; vol. 22, 1914, pp. 500, 535, 607–610; vol. 23, 1915, p. 14. Proceedings of the American Philosophical Society, vol. 49, 1910, pp. 3–16. The Photographic Times, vol. 28, 1896, p. 1.

Notes on the photographs of Halley's comet taken at Diamond Head by Ferdinand Ellerman, Publications of the Astronomical and Astrophysical Society of America, vol. 2, 1915, pp. 221–227.

Visual observations of Halley's comet in 1910. Astrophysical Journal, vol. 39, 1914, pp. 373–404.

Cometary orbits. Astronomische Nachrichten, vol. 117, 1887, p. 59. Astronomical Journal, vol. 7, 1887, p. 95.

METEORS

Observations of meteors. Monthly Notices, vol. 59, 1898, pp. 107–108. Sidereal Messenger, vol. 1, 1882, p. 174; vol. 3, 1884, pp. 188, 192; vol. 5, 1886, p. 74; vol. 10, 1891, pp. 426, 470. Astronomical Journal, vol. 14, 1894, p. 122; vol. 17, 1896, p. 40; vol. 18, 1897, pp. 28, 107; vol. 24, 1904, pp. 128–129. Astronomy and Astrophysics, vol. 12, 1893, p. 938. Astrophysical Journal, vol. 9, 1899, pp. 151–156. Popular Astronomy, vol. 1, 1893, p. 192; vol. 5, 1897, pp. 281–284; vol. 11, 1903, p. 580.

THE AURORA

Observations of the aurora. Astrophysical Journal, vol. 16, 1902, pp. 135–144; vol. 31, 1910, pp. 208–233. Popular Astronomy, vol. 26, 1918, pp. 377–379. Nature, vol. 95, 1915, pp. 536–537, 703.

Self-luminous night haze. Proceedings of the American Philosophical Society, vol. 50, 1911, pp. 246–253; vol. 58, 1919, pp. 223–235.

THE GEGENSCHEIN

Observations of the Gegenschein or zodiacal counterglow. Sidereal Messenger, vol. 2, 1883, p. 254; vol. 4, 1885, p. 30. Astronomical Journal, vol. 7, 1888, p. 186; vol. 11, 1891, p. 19; vol. 13, 1893, p. 169; vol. 17, 1897, p. 151; vol. 20, 1899, pp. 131–132. Popular Astronomy, vol. 1, 1894, p. 337; vol. 7, 1899, pp. 169–179, 290; vol. 26, 1918, p. 561; vol. 27, 1919, pp. 109, 289; vol. 29, 1921, p. 149.

MISCELLANEOUS

Introduction to "The Science-History of the Universe," by Waldemar Kaempffert (Current Literature Publishing Co., New York), 1909, pp. IX–XVI.

Astronomical coincidences. Astronomische Nachrichten, vol. 144, 1897, p. 331; vol. 145, 1898, p. 237. Sidereal Messenger, vol. 10, 1891, p. 18.

A few astronomical events of the past fifty years. Popular Astronomy, vol. 24, 1916, pp. 479–487.

Focal length of the 40-inch telescope of the Yerkes Observatory. Astronomical Journal, vol. 31, 1917, p. 24.

Lewis Swift. Astronomische Nachrichten, vol. 194, 1913, pp. 133–136.

Sherburne Wesley Burnham. Popular Astronomy, vol. 29, 1921, pp. 309–324.

REVIEWS

Burnham. General Catalogue of 1290 Double Stars. Astrophysical Journal, vol. 12, 1900, pp. 228–236.

Pickering. The Moon. Astrophysical Journal, vol. 20, 1904, pp. 359–364.

Wolf. Die Milchstrasse. Astrophysical Journal, vol. 29, 1909, pp. 89–90.

Chambers. The Story of the Comets. Astrophysical Journal, vol. 31, 1910, pp. 92–94.

Wolf. Stereoscopbilder von Sternhimmel. Astrophysical Journal, vol. 44, 1916, pp. 131–135.

Aitken. The Binary Stars. Publications of the Astronomical Society of the Pacific, vol. 31, 1919, p. 170–176.

WASHINGTON : GOVERNMENT PRINTING OFFICE : 1926